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PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY
J. Y. W. MacALISTER
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE ELEVENTH

SESSION 1917-18

PARTS I & II

GENERAL REPORTS

SECTIONS:—

ANÆSTHETICS BALNEOLOGY AND CLIMATOLOGY
OF DISEASE IN CHILDREN DERMATOLOGY ELECTRO-THERAPEUTICS
PIDEMIOLOGY AND STATE MEDICINE LARYNGOLOGY MEDICINE
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Marcus Beck Laboratory Reports.—No. 7.

Note on a New Counting Chamber for the Enumeration of Protozoa and other Organisms.[1]

By J. W. Cropper, M.B., M.Sc.

During recent years several research workers in protozoology have considered it advisable to make an enumerative study of the organisms with which they have been dealing, and it was during some experiments[2] on attempts to cultivate the intestinal amœbæ of man, that I was led to believe that an accurate knowledge of the number of organisms present might be of value. At the outset I was faced with the difficulty that an efficient method of counting was not available, and I therefore undertook to try to find a method which would be applicable, not only to the enumeration of organisms in stools, but also to that of protozoa in general. Such a method would doubtless be welcome to others who have hitherto laboured under the disadvantage of not being equipped with a suitable counting chamber or other means of enumeration. With the object of indicating the kind of organism whose enumeration would be rendered easier by the use of one or other of the methods described below, the following examples are quoted from the work of previous experimenters. Much of the earlier work was carried out with the ova of worms, but latterly the scope of enumerative methods has been considerably extended.

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[1] Read before the Section of Pathology, February 19, 1918.

[2] These experiments were carried out on behalf of the Medical Research Committee in the Marcus Beck Laboratory during 1916.

Dr. Annie Porter [6] counted the cysts of *Giardia* (*Lamblia*) *intestinalis* in human dysenteric fæces and constructed curves to demonstrate that in some of the cases there was a periodic rhythm in the number of organisms present, the maxima recurring at intervals of about a fortnight. The enumeration was carried out by means of a Thoma-Zeiss hæmocytometer. The parasites are often so plentiful in this disease that a sufficient number are present on the ruled surface of the slide for the purpose of counting. When, however, the infection becomes scanty, I have found it a decided advantage to have a larger area of the chamber ruled, so that a sufficiently large number of the organisms can be counted to reduce the errors due to "random sampling." Alternatively the well-known "method of fields" could be used, by means of which organisms can also be counted on the non-ruled part of the slide, and it was mainly with a view to obviate certain disadvantages of this latter method that the present chamber was designed.

Knowles and Cole [5] made an exhaustive enumerative study of amœbic infections of the intestine. They made not only a *total* count of all the organisms present, but also a *differential* count based on the relative number of their nuclei. Their remarks with regard to the use of a Thoma-Zeiss slide for the purpose of counting entamœbæ illustrate the precise kind of difficulty that I have experienced. They say: "In this case, however, low counts are difficult as there may be no amœbæ at all on the ruled area. In using the Thoma-Zeiss slide it is better to go over the whole of the chamber contained within the circular trough, working backwards and forwards in lines, until all amœbæ in the cell have been counted."

Calkins [1] made a series of experiments to determine the influence of normal and of cancerous tissues on the rate of reproduction of certain ciliate protozoa (*Didinium nasutum*), and he found it necessary to count the organisms periodically at different stages of their cultivation. Owing to their large size and visibility and to their comparative scarcity (up to sixty) in the cultures, no special enumerative method would be required, but as each *Didinium* was supplied with a daily uniform food-ration of nine paramœcia, this would apparently frequently involve the counting of 540 of these protozoa. No indication is given as to how this was done beyond stating that they were counted out.

Goodey [4], while investigating the limiting influence of soil protozoa on bacterial growth, found it necessary to make frequent counts, and for this purpose he made use of two indirect methods of enumeration. He states that: "As methods they undoubtedly leave much to be desired, for there are so many uncontrollable factors entailed in their use. At the same time they are the only methods at present available, and one is justified, I think, in drawing conclusions. . . ."

In collaboration with R. W. H. Row, of King's College, and later with the assistance of Miss M. Lovibond, V.A.D. [2], I endeavoured to count the cysts of *Entamœba coli* in the stools of a carrier of this organism and, owing to the comparative scantiness of the infection at the time, I was at once led to discard the hæmocytometer for the purpose. Often but a single cyst could be found in the whole chamberful, and it was the exception to find one at all on the ruled area. The method of fields was then tried and was found tedious and in other ways unsatisfactory for rapid routine work. I therefore undertook to find a better method of counting and also decided to make a daily enumeration of the cysts during a period of three months, firstly to establish by statistics the accuracy and practical utility of the new method, and secondly to determine whether any cycle could be demonstrated for *Entamœba coli* as was done by Miss Porter in the case of *Lamblia*. The results of this enumeration will be published later and the method finally adopted is described below (Method 2A).

ENUMERATIVE METHODS.

The principle on which enumerative methods are based is of course the preparation of a uniform suspension of the organisms or cells in a suitable liquid medium, and the subsequent counting of these in a definite known volume of the emulsion or of a dilution thereof. For a method to be of practical value it is necessary that it should be accurate, within a reasonable and unavoidable experimental error, and that the labour expended and time occupied in the actual process of counting shall be reduced to a minimum. Hitherto the counting chambers intended for the enumeration of blood cells (and of bacteria) have fulfilled these requirements, but when we come to deal with protozoa, and meet with organisms of very different sizes and of very variable abundance, it becomes necessary to devise new means of carrying out the enumeration. As an important corollary to the question of the labour of counting is the fact, which is frequently lost sight of, that a large number of the organisms must be counted in order to reduce the statistical errors to a reasonable amount. Thus it has been pointed out by Ross and Stott that if the error is required to be within 10 per cent. one must count at least 271 individuals. The three factors—viz., accuracy, speed of counting, and number of organisms counted—must all be taken into account before the most efficient method to be adopted in a particular case can be decided upon, and it has been the consideration of these points which has guided me in attempting to devise new methods for the enumeration of protozoa and other organisms.

There are three chief means available for obtaining a small definite volume of an emulsion to be used for counting purposes, viz.:—

(1) *Drop-measuring method;*
(2) *Graduated-pipette method;* and
(3) *Counting-chamber method.*

These will now be considered in detail.

METHOD (1).—DROP-MEASURING METHOD.

Capillary pipettes in whose preparation a certain amount of care has been taken in making the point level will deliver successive drops whose volume is almost uniform, provided that certain details as regards the expulsion of the drop are attended to. For instance, the pipette must be held at the same angle each time a drop is delivered and a uniform pressure must be used to regulate the rate of flow. The temperature and the surface-tension of the liquid also influence the size of the drop. Donald [3] has used this method extensively for the enumeration of bacteria and of the cells of blood and cerebro-spinal fluid. Except in cases where the need for great accuracy arises, I think the refinements introduced by this method are unnecessary for the enumeration of protozoa, but the method is undoubtedly a precise means of obtaining a uniform volume of a liquid or of a suspension of cells.

METHOD (2).—GRADUATED-PIPETTE METHOD.

A definite volume of an emulsion can be expelled from a graduated capillary pipette. It is of importance to remember that the volume expelled is not necessarily equal to the volume indicated by the graduations, and in actual practice the error may be considerable, but is diminished by experience gained in manipulation of the pipette. The error is a variable one and with a given pipette it depends largely on the length of the column delivered. Obviously the shorter the column the greater the percentage error is likely to be. With the object of determining the extent of the error which one may expect when using graduated capillary pipettes, twenty successive volumes of an emulsion of stool, each of 20 c.mm., were expelled on to a microscope slide and covered with a slip. The weights of the slide before and after each addition of the emulsion were taken and the difference represented (approximately) the volume of the liquid expelled. The average weight of the supposed 20 c.mm. was found to be 19·75 mgm., and the maximum errors were 4 per cent. below and 6 per cent. above this figure taken as a mean. It is therefore better always to make at least two counts when using the graduated pipette method and to take the average.

In order to make use of this method it is necessary either to count the organisms in the whole of the drop expelled, or to prepare a film of uniform thickness and known area and count the organisms in a portion of this area, say by the method of fields. Having previously ascertained the area of the microscope field by any of the usual methods, a simple calculation will enable one to tell what fraction of the drop has been examined. The film may be obtained by placing the drop on a microscope slide and allowing a cover-glass to fall flat upon it. The size of the drop can be arranged so that it just occupies the whole of the area under the cover-glass, but it is not an altogether satisfactory method since the heavier particles tend to accumulate in the centre, and the cells are not equally distributed. I find that the drop should not exceed 10 c.mm. when using a ⅞ in. cover-glass, nor 20 c.mm. for a 1 in. circle; otherwise the drop tends to spread beyond the edge and the number of organisms is under-estimated. This method has recently been used by Yorke and Blacklock [7] for the enumeration of *Microfilaria nocturna* in blood and urine. When the number of organisms is less than 500 per c.c., "concentration methods" are necessary to enable the method to be employed. It is preferable, at any rate in cases where the organisms can be readily distinguished from other material in the drop, and can be counted with reasonable accuracy with a low power of the microscope, to use the method of a graduated pipette for routine purposes and *count the organisms in the whole of the expelled volume.* This was effected by using a slide ruled in columns with a ring of paraffin-wax as a support for the cover-slip. I shall refer to this as the "ruled slide and wax ring method."

METHOD (2A)—RULED SLIDE AND WAX RING METHOD.

A microscope slide 3 in. by 1½ in. is ruled in columns (*vide infra*) ½ mm. wide, parallel to its long axis, across almost its entire width. The slide is placed on a turn-table, which is then made to revolve, and by means of a small camel-hair brush a drop of molten paraffin-wax is applied to the ruled surface of the slide (fig. 1). The drop spreads out into a ring of practically uniform thickness and sets solid. The internal diameter of the ring should be made sufficiently large, say 1 in. to 1¼ in., to allow at least 20 c.mm. of an emulsion to be spread out without extending to the wax, and for this reason ordinary 3 in. by 1 in. slides are not very suitable. The depth of the ring, which of course determines the depth of the chamber when completed by the application of a cover-slip (1½ in. circle), can be regulated by using a

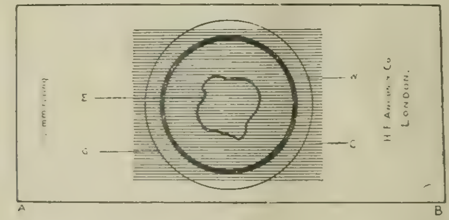

FIG. 1.

Diagram to illustrate the Ruled Slide and Wax Ring Method for the enumeration of protozoa and other organisms with a low power. A B, slide; C, ruled columns, ½ mm. wide; W, wax ring; E, emulsion (20 c.mm.); G, cover-glass. (Drawn to scale; natural size.)

greater or less amount of the molten wax, and after a little practice it is quite easy to prepare in a few minutes a dozen of such slides, each one with a chamber about 0·1 mm. to 0·2 mm. deep.[1] The improvised counting chamber prepared in this way can be used over and over again without material deterioration, and when the ring has become too thin it can be removed with xylol and a fresh one applied. If the original ring is found to be too deep it can be thinned down by gently rubbing with a cloth. One of the advantages of this method is that the depth of the chamber can be varied, and in the case of large organisms which are present in small numbers it may be desirable to examine a large volume, and this can be made possible by increasing the thickness of the wax ring. This method has been in constant use for the enumeration of *Entamœba coli* cysts in stools, and in the case of organisms which do not require a high magnification it is the best means available. It is superior for this purpose to any of the counting chambers, which are expressly designed for counting with a high power, since a much larger number of organisms can be seen in one preparation. In fig. 2 the distribution of the cysts in 20 c.mm. of a 5 per cent. saline emulsion of stool is shown.

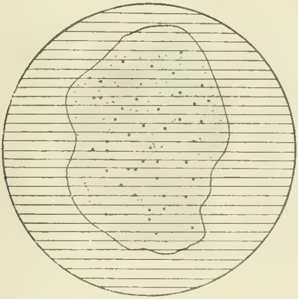

FIG. 2.

To illustrate the distribution of *Entamœba coli* cysts in 20 c.mm. of a 5 per cent. saline emulsion of stool. Forty three cysts present = 43,000 per gramme. (*Del. ad nat.*)

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[1] It is unnecessary to know the exact depth. A definite volume is delivered from the graduated pipette.

Preparation of Ruled Slides.—The ruled slides can be made in the laboratory or a more finished article can be obtained from Messrs. Angus and Co., Wigmore Street, London, who can also supply the new counting chamber. The most serviceable ruling is in columns ½ mm. in width, since with this size and a low magnification, say a ⅔ in. objective and a × 7 eye-piece, the column will occupy about the middle half of the microscope field. In actual practice it is found that a more rapid count can be made with this than with a wider column. The counting also tends to be more accurate since organisms at the top and bottom of the field are less likely to be missed.

In order to prepare a number of ruled slides for counting with a low power all that is necessary is a few 3 in. by 1½ in. slides and a writing diamond. The use of a dissecting microscope is an advantage in ruling the lines, but an ordinary magnifying glass, mounted in some way so that the slide can be viewed through it without interfering with the manipulation of the diamond, will suffice. One of the slides, previously well cleaned, is placed on a dark background, and is held down by means of a piece of plasticine under each end of the slide. Lines are then ruled with the diamond in series about ½ mm. apart almost the whole width of the slide. The lines must be *ruled* on the glass, not scratched on, as in the latter case they tend to crack with use, and they must not be too thin as they are then difficult to recognize. The columns should be of fairly uniform width, since if any of them are wider than the microscope field counting becomes troublesome. In view of these difficulties I think the use of slides made by expert optical manufacturers is preferable.

METHOD (3).—COUNTING CHAMBER METHOD.

This is the method which is in common use for the enumeration of cells and organisms, and the volume is determined by the space enclosed between a cover-slip and the ruled area on a small platform which is depressed to a known level, usually $\frac{1}{10}$ mm., below the general surface of a microscope slide. The majority of counting chambers already in existence have been designed for the enumeration of blood cells, and as these are relatively abundant, it has only been necessary to construct a very small chamber to enable a sufficient number of cells to be counted in a single preparation. When dealing with protozoa, however, the number of these organisms is as a rule relatively scanty, and it is desirable to employ a chamber whose dimensions are as large as possible, compatible with accuracy in construction and with ease and rapidity of manipulation. After full consideration of the question of the size of the chamber likely to be most suitable for general use, it was decided that a ruled area of 5 mm. by 5 mm. would be the maximum permissible, in view of certain practical objections to the use of a larger one, and would satisfy the requirements for the enumeration of protozoa and other organisms and cells under most circumstances. The depth of the chamber was retained at $\frac{1}{10}$ mm. since it was considered that the volume—namely, 2·5 c.mm.—would be adequate, and that the necessity for repeated focusing with a deeper chamber might be a handicap to the speed of counting. As the new chamber, constructed with these measurements, was designed especially for the counting of protozoa, I suggest that the term "Protozoometer" would indicate its chief use, and would serve to distinguish it from counting chambers used for other purposes.

DESCRIPTION OF THE NEW COUNTING CHAMBER.

The chamber consists of a small circular platform about 8 mm. in diameter constructed in the centre of a special glass slide (fig. 3). Cover-glasses which have been carefully ground to a uniform thickness, so that when one is placed over the chamber the depth of the latter will be constant over the whole of its ruled area, are supplied with the slide. The slide itself is similar to that already employed by manufacturers of optical instruments in the construction of the hæmocytometer, and the only important modification introduced in the new chamber is in the dimensions and character of the area ruled on the platform (fig. 4). The surface of the platform is depressed $\frac{1}{10}$ mm. below the general surface of the slide by a process of grinding, and it is surrounded by a shallow groove or trench which acts as an overflow for any slight excess of liquid which is used in making the preparation. On the platform a large square 5 mm. by 5 mm. is ruled, and this is sub-divided by lines intersecting at right angles into a number of smaller squares. As most protozoa, even small ones, can be readily distinguished from surrounding objects with a $\frac{1}{6}$ in. objective, the magnification produced by this lens with a × 7 eye-piece was selected to determine the size which these smaller squares should be.

This is a matter of the first importance in enabling the enumeration of organisms to be carried out as a daily routine both accurately and at the same time *rapidly*. The square should occupy neither too large nor too small a portion of the field of the microscope, as in both these circumstances the speed of counting is materially diminished. Bearing these points in mind, it was found that if the large square was divided into 625 small squares, each one of which was $\frac{1}{5}$ mm. along the side, the whole of the small square occupied only the central portion of the field and the counting could be carried out to the best advantage. As a matter of fact it is possible that if the ruled area were simply divided into columns instead of squares, each column being $\frac{1}{5}$ mm. in width, the count could be made equally well, but as circumstances may arise in which it is desirable to count only a portion of the ruled area the chamber has been ruled in squares. So far as I am aware the dimensions of the ruled area of this chamber are larger than those of any of the chambers already in use for blood work, most of which are modifications of the Thoma-Zeiss pattern. The Türck disk, for instance, has an area of 3 mm. by 3 mm., and this combined with a depth of $\frac{1}{10}$ mm. gives a volume of less than 1 c.mm., whereas the new chamber here described has a volume of 2·5 c.mm.

In order to use the counting chamber it is only necessary to place a small quantity of an emulsion of the organisms or cells in the centre of the ruled area and then to apply the cover-glass, care being taken to avoid bubbles and an undue amount of liquid. It is advisable to apply the cover-glass rapidly before the cells have time to settle down, as otherwise their distribution may not be uniform. Provided this is done the chamber can be relied upon to give an accurate count of organisms whose specific gravity is comparatively high. For further details of the procedure reference may be made to any text-book describing the use of the hæmocytometer.

The chamber has been tested for the enumeration of a variety of protozoa, both pathogenic and non-pathogenic. In the case of free-living forms it may of course be necessary to fix the organisms first. Although it is not designed to supersede the chambers already in use for blood counts, it can be employed for both red and white corpuscles. In the former case a greater dilution of the blood than usual is required, but in the case of the latter, in a normal count with the usual 1 in 20 dilution, as many as a thousand are present on the ruled area, and the necessity of making several counts, as with a Thoma-Zeiss chamber, is avoided. The chamber could doubtless be employed for the enumeration of the cells in serous exudates, cerebro-spinal fluid, milk, &c., and speaking generally in any cases where the cell-content is comparatively scanty.

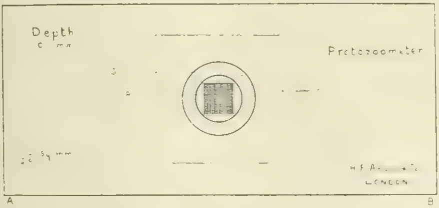

Fig. 3.

Diagram to illustrate the dimensions of the ruling of the new Counting Chamber ("Protozoometer"). A B, slide; R, ruled area; G, groove for overflow; C, cover-glass. (Drawn to scale; natural size.)

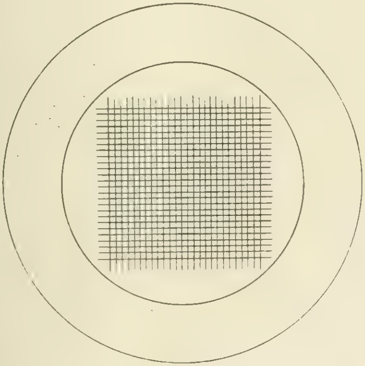

Fig. 4.

To illustrate the ruling of the platform of the new Counting Chamber. Area of large square, 25 sq. mm.; small squares, $\frac{1}{25}$ sq. mm. (Drawn to scale; × 5.)

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The Royal Society of Medicine.

CONJOINT MEETING[1]

OF THE

Sections of Surgery and Neurology, with the War Office Committee for the Study of Tetanus.

Chairman—Major-General Sir ANTHONY BOWLBY, K.C.M.G., K.C.V.O., F.R.C.S. (President of the Section of Surgery).

MUSCLE CONTRACTURE FOLLOWING INJURY.

Major-General Sir DAVID BRUCE, K.C.B., F.R.S.

SOME months ago a circular was sent to a large number of medical officers in England, asking for information regarding the condition of localized contracture of the muscles of a wounded limb. A great many cases of contracture occur among our wounded soldiers. What the exact number of these cases of contracture is, or what proportion they bear to the wounded, cannot be stated, since the War Office has no statistics available dealing with this condition. It has been suggested that many of them are merely instances of a local reflex contracture set up by the irritation of the nerves in or near the wound. If this occurs in war wounds, one would imagine it would also occur in wounds in civil life, such as those caused by machinery, by railway accidents, and by surgical operations. The questions asked in the circular were as follows:—

---

[1] Held February 14, 1918.

(1) Can you mention any case of persistent contracture following injury or surgical interference occurring among civilian patients? Please indicate the nature of the lesion.

(2) What percentage would you roughly estimate such cases to form of the total number of cases of injury of the same type coming to your notice?

(3) Were any of the cases of contracture contaminated by earth infection?

(4) Did any of these cases receive a prophylactic injection of anti-tetanic serum?

(5) What was the duration of the contracture?

Now, almost without exception, these questions were answered by the civilian surgeons in England in the negative. In fact I think that only in one case was the answer in the affirmative.

The question then arises why these cases of contracture should be almost wholly restricted to war wounds. Is there some factor in war wounds which tends to nerve irritation? Is that something wanting in civil wounds? Or is the tetanus toxin in reality at the bottom of it?

To try to elucidate this question is the reason the Neurological and Surgical Sections have been asked to meet the Tetanus Committee of the War Office this evening.

The question for discussion, then, is: What is the cause of the localized contracture of the muscles of a wounded limb?

As Dr. Wilfred Harris has brought some cases here, I think it might be better for him to demonstrate them now, before beginning the discussion.

DR. WILFRED HARRIS.

I demonstrate these five cases as examples of different forms of local muscular contracture following upon wounds, none of which in my opinion has any connexion with tetanus.

Hysterical Contracture.—This first patient was wounded as long ago as September 25, 1915, at Loos, by a bullet which passed through the left forearm, breaking no bone, but severely damaging the ulnar nerve. He still has complete ulnar nerve palsy, both motor and sensory. He was blown up by a mine explosion a few months after he was wounded in the arm, and while being taken to the dressing station three hours after his injury he noticed the fingers of his left hand gradually drawing down, in the manner you see. They became more and more flexed, until now the four fingers at the interphalangeal joints are flexed to their limit. At first he could straighten his fingers by pushing them back, but that became more difficult, and now he cannot open them, and there is a shortening of tendons. No splint has been applied. I show it as a case of *hysterical contracture* following a wound. It has been suggested by some who have seen him that it might possibly be chronic local tetanus. I have seen a precisely similar state in a carpenter in civil life. While at work he wounded his hypothenar eminence. It bled a little, after which the little finger began to draw down, and, during the night, the other fingers of the hand followed suit, until the fingers became tightly flexed. The spasm did not involve the thumb. He was so upset about it, that he came to hospital in the middle of the night. I saw him next morning, and he had complete flexion of the four fingers. One could bend the fingers back, but the hand was completely anæsthetic. Faradism and suggestion dissipated the spasm. In this present case there is no anæsthesia.

This next patient was wounded last year: a piece of shrapnel injured the upper and outer forearm. A splint was applied, and the fingers kept straight: it was retained for five weeks. On removing the splint the fingers began to contract in flexion, mostly on the ulnar side. There is now complete contracture, somewhat like that in the first case. He says the bandage was tight and the fingers became discoloured; this made me think of the possibility of Volkmann's ischæmic contracture, but I do not think it is that. He has a painful flexor spasm of the little and ring fingers, so that when passive extension is started he complains of severe pain, which he refers to his palm rather than to his forearm: the pain is most marked when drawing on the ring finger. I show it as a case of *hysteroid flexor spasm of fingers.* Under hot water the spasm relaxes somewhat.

The next case is somewhat similar: I show it as *hysterical flexor contracture.* He also has ulnar nerve injury: a rifle bullet passed through the lower forearm, fracturing the bone. A splint was applied, and an attempt to relieve by operation was not successful, as now there is fairly complete ulnar nerve palsy. There are large contractions to galvanism, thus differing from the other case, although he has this flexor contracture with organic shortening now. He has obvious hysterical weakness of all the movements of his wrist, fingers and forearm, and there are jerky, ill-sustained contractions, such as one sees in hysteroid weakness.

Clonic Facial Spasm.—This next patient was wounded in the left parotid region, twice in two years in the same place. The first was in 1916, when teeth were knocked out, and he had no facial spasm. The second time was on July 31 last, and no teeth were removed. A week later, the contractions of the left facial muscles began. For three months they got progressively worse, and after that the condition remained stationary, as you see it now. You will note the violent contractions in the anatomical distribution of the nerve. The occipitofrontalis is not so much involved as the other muscles, notably the platysma, ear muscles, orbiculares oris et palpebrarum and the zygomatici. I ascribe this to mechanical irritation of the facial nerve from the scar.

Facial Tic.—This last patient was wounded on July 25, 1916. He was holding a Lewis gun when it was hit by an enemy machine gun, spattering his face with pieces of metal, which were afterwards picked out by his medical officer. No spasm was noticed until immediately after the extraction of the bits of metal, when the twitching commenced. The spasms gradually improved until last April, nine months after receiving the wound. He then got severe shell shock from the collapse of a dug-out, developing neurasthenia, many of the symptoms of which still persist. In October the spasm on the left side of his face was very violent, and involved the depressors of the lower jaw, not only the platysma, but the mylo-hyoid probably; the jaw was pulled down with considerable force, even when the platysma was not working, showing it was not facial spasm such as the other patient had. The right side also was somewhat involved at first in this case. I regard this as facial tic, not as facial spasm due to irritation of the nerve.

Mr. JOCELYN SWAN.

This patient was wounded on September 21, 1917, being spattered with shell fragments in the back of his left thigh, left calf, and the lower part of his left leg. The wounds were very slight, and no operation was performed. He was admitted to a hospital, but was marked out as convalescent on November 10, six weeks after the infliction of the wound. On November 30, whilst at a Command Depot, he noticed what he called "spasm" in the left leg and contraction of the leg and foot, the foot becoming distinctly arched, more than is evident now, with the toes curled up underneath. This recurred on several occasions during the next few days. He was returned to the medical officer, and the patient says he was given some injection. We have no record of what it was, and, so far, I have been unable to trace his field card. Presumably he was given antitetanic serum when he was first wounded. Later he was admitted to the Royal Herbert Hospital, where I saw him on January 19. At that date his whole leg was spastic, particularly the calf muscles, gastrocnemius, soleus, and deep muscles. The foot was very arched, the instep prominent, the toes acutely flexed. Any stimulation of the calf muscles produced clonus in those muscles, also in the extensor muscles of his thigh, in the hamstrings, and in the tensor vaginæ femoris. His reflexes were very exaggerated, the knee-jerks particularly, on the left side, not on the right. He had suprapatellar clonus and ankle clonus, but the epigastric, hypogastric and cremasteric reflexes were normal. He had a brisk supinator longus jerk on both sides. The knee movements were good, also those of the thigh, but he could not dorsiflex his foot, nor could I flex it passively. I regarded it as chronic tetanus, and gave him, at that time, three doses of 1,000 units intramuscularly, that is, one dose on the same day, one on the next day, then I missed a day, and gave him a fourth dose a week later. After the second dose clonus was still evident on stimulation, but the reflexes were much less brisk. Five days after his first injection of serum all his spasm had disappeared, except in the tibialis posticus; no clonic spasms could be elicited in any of the muscles, and the knee-jerks were normal. His condition has remained practically unchanged since then, but he still has contracture of his foot, but less severely, for his toes are now supple, the movements of the knee-joint are good, and the calf muscles are much softer. The skiagram shows a small fragment in the lower part of the leg, behind the tibia. It differs from the cases which have been shown by Dr. Wilfred Harris in being one of chronic or local tetanus.

Dr. WORSTER-DROUGHT.

From the time of this man's admission to hospital until about February 3, he gave a definite extensor plantar reflex.

Dr. F. Golla.

I have a cat suffering from local tetanus, which shows a homologue of local tetanus in man. There is a purely rigid condition, there is a paralysis, not so well marked as we usually find it: it exists to some extent in the toes, and there is marked rigidity in the gastrocnemius and the skeletal muscles. The cat has been injected with tetanus toxin in the leg, but in the case of man, unless we had actual proof, we should be in doubt whether he had had tetanus or not.

In regard to contracture, we are dealing with what is a new chapter in the surgical history of wounds.

On the initiative of the Tetanus Committee a memorandum was sent to the principal surgeons in England, especially those whose practice is among a large industrial class, where injuries in civil life are common. In the large industrial centres thousands of mangling and serious accidents occur in connexion with machinery, and we thought we should get as near a homologue as possible in those cases to the injuries inflicted on the field of battle. We got answers from about eighty surgeons. All of them, with one exception,.said they had never seen, in the course of their medical lives, cases of contracture occurring as a sequel of wounds. That answer was a little surprising. We were prepared to find that a small percentage of wounds caused contracture, but the results—which will probably be published shortly—show that to surgeons in busy practice it has been practically unknown. The single exception was that of a London surgeon, who said he had seen such cases, but was unable to cite them, or give particulars, and he regarded them as probably of extreme rarity. Any explanation of contracture must account for the extraordinary incidence of contracture during the present war, and its practical absence in civil practice prior to the War. The Staff Reports of the last two wars in which medical matters were observed accurately—the Franco-German War, and the American War of Secession—tell the same story. In the first of these, one was able to find very few probable cases of contracture, but even those it would be difficult to swear to. In the War of Secession one has found, so far, five cases, among, of course, an enormous number of wounded. So we have now to do with something new, not new as a symptom *per se*, but as a general symptom in association with wounds.

What are the new conditions which obtain during the present war? There is the psychical condition, it is true: mankind has never before been subjected to such an intense amount of shock in battle. In earlier days, most of the wounds were due to musket balls or to sabre cuts, and the psychical strain was much less than now. Another factor is that introduced by the use of antitetanic serum. There has never before been a time when so much earth was introduced into the body through wounds. Since the early days of the War in 1914 we have followed the practice of injecting a dose of antitetanic serum into cases of wounds. A third new point is the occurrence of anaërobes, other than the tetanus bacillus, in wounds.

In considering the question of contracture, it is very important to be absolutely clear as to what we mean when using what is a very ill-advised term. There is a real and a false kind of contracture, and the real kind is that of nervous origin. It is a constant contraction, without remissions, and it is dependent on the functional continuity of the muscle with the central nervous system. Perhaps the best analogy we have to contracture is the contraction of the adductor muscle in the mollusc, where there is a chronic contraction kept up without work being done and without discharges, as in tetanus proper. Contracture may be of cerebral or of spinal origin, and according to its origin we shall find it differently dealt with when we look at the subject under anæsthesia. If the contracture is of cerebral origin, a light degree of anæsthesia, sufficient to abolish consciousness, will abolish the contracture. If it is of spinal origin, we shall have to push it to the degree of general muscular relaxation to get rid of the contracture. It is important to define also the contracture of myogenic origin. That may arise as a secondary symptom to nervous contracture—that is to say, if a contracture of nervous origin be kept up sufficiently long certain changes in the muscle will occur, and the muscle itself will take on a permanent contraction, one which is independent of the central nervous system.

Among other causes of myogenic contracture are ischæmia, such as Volkmann's ischæmic contracture, or simple paralysis and secondary contraction, which we may get in a paralysed facial muscle. Those will not be resolved by chloroform, nor by ablation of the central nervous system; they are of myogenic origin. If in five or six days I were to put the cat under chloroform, the contracture would still remain, and so it would after ablation of the central nervous system, or use of curare.

Cases of nervous contracture have been extensively discussed at the Société de Neurologie, in Paris, for the great frequency of these cases in the French Army and in ours has aroused great interest. Obviously, a great number of the cases are of functional origin. When a wounded man develops, within five minutes of receiving his wound, acute contraction of muscles in the wounded limb, most of us would consider it of this class: and when cases can be resolved by hypnosis, by ordinary suggestion, or by strong faradization, there is no doubt about their origin. But some cases are resistant, and Babinski attempted to establish the discredited theories of reflex contracture, using them to bolster up his former theory of hysteria never showing symptoms which are not under the control of the will. That is to say, if a case be functional, the symptoms shown must always be those which can be reproduced by an act of the will. The commonest experience is absolutely against these contentions of Babinski. When a person blushes, an action takes place which is not under his control: you have all the responses of the vegetative nervous system: the passing of motions involuntarily, involuntary micturition, sweating, all contradict that idea. These vegetative responses accompany psychical activity and yet are not under control of the will. And, more recently, we have witnessed the demonstration of Dr. Waller as to the psycho-galvanic reflexes. In this matter Babinski fell into error: we should never attempt to say what the mind cannot do, because none of us have sufficient experience of the functions of the body to enable us to make any pronouncement as to what the influence of mind on the body can be. Babinski considers we have to do with an organic lesion, which he calls reflex contracture, because he finds loss of temperature in a limb, increased deep reflexes, and signs of diminished oscillation of an artery in a limb.

Physiology is silent as to the nature of such reflex contracture. It is impossible to produce reflex contracture in the laboratory; there is no method of stimulation to assist the animal to keep up persistent contraction of muscles for more than a few minutes.

The type of wounds does not appear to supply any reason for such a particular reflex contracture, and the final answer lies in the experience elicited by the question of the tetanus referendum. Wounds occur in civil practice of as great gravity, with as much laceration of tissue, as those in war; yet in all the terrible wounds due to machinery, and street accidents, this contracture appears, as a result from our inquiry, to be unknown. Therefore we can dismiss the question of reflex contracture.

We are, therefore, now left with hysterical contracture, and with contracture of neural origin. How can we decide this? The obvious thing is, not to argue whether the accompanying or concomitant symptoms could be under the control of the will, or not, but to see what happens when you remove cerebral control. When the patient is unconscious, he cannot be hysterical, and if a contracture disappears under an anæsthetic which has been pushed only to unconsciousness, we can say the contracture is due to consciousness, or is associated with it. But there is one point to be remembered. If hysterical contracture has been allowed to go on for many weeks or months, changes in muscle will have taken place, and an anæsthetic, even if pushed to the full, will not abolish the contracture. Therefore the diagnosis should be made within a few days after the beginning of the nervous contraction. If the contracture is only checked when the anæsthetic has produced muscular relaxation, the contracture is of neurogenic origin. I do not see any reason for assuming reflex contracture, a condition of which we know nothing, and one which nobody has induced experimentally. The only contracture we know of which we can ourselves produce is that due to injection of tetanus toxin. That also provides an answer to the first question we asked: How can we account for the appearance of a new series of symptoms following wounding?

We see that the cat, which is resistant to tetanus, can develop local contracture—a local, not general, tetanus. We want to elicit the opinions of surgeons as to whether we are justified, in such cases in which the influence of the anæsthetic is resisted in the first degree—supposing it is not a functional case—in assuming that such contractures are due to local tetanus. It is a point of great practical importance, because in the case of local tetanus we have to make a diagnosis as soon as possible, because, however unsuccessful we may be in the treatment of tetanus, we can prevent tetanus becoming generalized if we have sufficient time to give large injections of antitetanic serum. It is difficult to see on what grounds the diagnosis of tetanus can be made. Increased deep reflexes occur, *pace* Babinski, in 50 per cent. of functional contractures which yield to suggestion, or are abolished by a dose of chloroform, and these we do not doubt are of central origin. When we were in the Line under shell fire, we tested the deep reflexes of the people as they came in. Everybody was in a "jumpy" state, and their reflexes were exaggerated.

The other point in the diagnosis of local tetanus which seems to have received much attention, judging from our reports of cases of tetanus, is the occurrence of ankle clonus in both limbs. I do not know whether that occurs much in the field. I should like to hear whether we can accept it as a critical point in the diagnosis of tetanus.

A third point, specially noticed by the French, is twitching—local clonic contractions of muscle—as apart from constant tonic contraction of muscle. Judging by the reports sent in to Sir David Bruce from our local tetanus inspectors, it is as often absent as not.

Lastly, we have a variety of anaërobic infection of wounds, some of which are not tetanic in origin. Is there an ascending toxin, causing local contraction, very much like local tetanus, which never leads to generalized tetanus? That is a point on which we would like the opinions of neurologists. Are we, as physicians, to class as tetanus those cases which chloroform does not clear up? Or shall we, as bacteriologists, split them up and say some are due to tetanus toxin, and others to toxins of various sorts which may occur in a wound?

Dr. F. Ransom.

Local tetanus is a central disease. You can produce local tetanus by injecting the toxin direct into the spinal cord. A short time after such injection into an animal the muscles become stiff, and at first—and that is an important distinction—nothing else happens, especially in cats: there is no sign of increased reflexes, nothing except that the muscles are thrown into a state of spastic contraction. A little later there are indications of reflex over-excitability of reflexes, and then the contractions are superimposed on the contracted muscles. Thus increased reflexes and the contraction of muscles are two definitely distinct phenomena; in fact, in frogs it has been shown that those functions can be controlled from different points of the spinal cord. There are centres in the cord which control contractures, and centres which control the reflex action. The difficulty in these cases of contracture is—in looking upon them as simply nerve irritation—their constancy. I think this is a central condition, which can hardly be due to any irritation from the wound, from cicatrix pressure, and so on. I think it must be toxic, and that it must be central; and I suggest that this contracture is very often due to tetanus toxin. That theory was suggested to me more especially by what I have seen in animals. I remember, for instance, a cat which, after section of the spinal cord, was injected with tetanus toxin in both sciatic nerves. It developed tetanus in both hind limbs, both being stretched out and quite stiff.

and superimposed upon that was an acute jerk, which went on continuously; the muscle was never relaxed for three weeks, after which the cat was killed. I remember also a dog which had toxin injected into the facial nerve, at the point where it emerges from the skull. A little time afterwards, the face began to be drawn to the same side as the injection, and the eye on that side was practically closed. That condition persisted for some time, but finally cleared up completely. It was a condition not unlike that of one of the patients shown to-night. In another dog the toxin was injected into the infra-orbital nerve, and shortly afterwards the ear on the same side was stiff and, except for continuous twitching, immovable. After a time recovery took place.

Dr. F. E. Batten.

I have seen definite cases of contraction which occurred after injury in civil life, and can recall a case of injury to the hand with marked contracture. If Dr. Golla would extend his inquiry among others than surgeons—viz., insurance doctors and neurologists—no doubt he would hear of such cases. I am not prepared to deny that these may not be cases of local tetanus. Local tetanus was not then recognized. The case I have referred to was that of a man who was shoeing a horse and a nail ran into his hand, and it may have been a case of tetanus. The case had been under treatment, in the Midland counties, by a surgeon and neurologist, and it was regarded as of functional nature.

Sir Kenneth Goadby, K.B.E.

The fact that many wounds contain end-sporing organisms, culturally and morphologically indistinguishable from the tetanus bacillus, that tetanus bacilli have been isolated from wounds of men free from clinical tetanus, sufficiently explains the possibility of wide incidence of local tetanus. In the last 200 cases of septic wounds which I have examined, 54 per cent. contained end-sporing organisms. How many of these were the true tetanus bacillus I am not prepared to state. All the patients had received prophylactic injections.

The outstanding difference between wounds occurring amongst the civil population in industrial accidents and those occurring in the present War is the infection with anaërobic bacteria referable to the tetanus group.

Every wounded man receives a prophylactic injection within the first few hours of his wound in most cases, and the increased incubation period of declared tetanus has more than trebled in length. These three factors appear to me to be quite sufficient to explain the occurrence of numerous contractures, clonic spasms, and other similar manifestations which show such a high incidence in war, over industrial wounds. Definitely to state that such spasm *is tetanus* may not be always easy upon one examination, but I consider that all cases of clonic spasm, painful in character and irregular in occurrence, should be regarded as tetanic in origin until the contrary can be proved. I have been compelled to adopt this view from personal observation of numerous cases of local tetanus. Three of my cases remained unprogressive for more than a week; at the end of this period acute symptoms supervened, and in two instances the patient died rapidly from generalized tetanus. The third case recovered, but is interesting from its length: A man with a compound fracture of the femur, which long remained septic, developed stiffness in the face muscles and neck, with inability to open mouth fully, fifty days after wounding; 1,500 units of antitetanic serum were given and the whole of the stiffness passed off. A sinus remained, leading to a sequestrum, the thigh was flexed at the hip-joint to an angle of 45°; nerve irritation reflexly produced by the sequestrum was stated to be the cause. On the seventy-eighth day there were jerky spasms of the affected leg, mostly at night and with pain. There were no other symptoms. The sequestrum was regarded as the cause of the spasm and its removal was advised. No cessation of spasms occurred, and seven days later there was rigidity of abdominal muscles, mainly on the right side, followed three days later by an attack of opisthotonos, although four injections of antitetanic serum were given. On the hundred and fifth day the muscular twitching passed off, but the right masseter remained hard; the left was quite soft. The abdominal muscles were still rigid. The flexure of the right leg on the abdomen, which had been treated by extension, gradually subsided. Ankle clonus was still marked on both sides. The patient was last seen four months after the occurrence of tetanus: the leg was straight; there was no ankle clonus; there was no rigidity; the calf muscles of the left leg and the thigh were much wasted; the knee-jerk on the right side was still increased.

The chief symptoms which I consider diagnostic of local tetanus are:—

(*a*) *Clonic spasm* (*twitching*), either of the wounded muscle itself or of those in the vicinity. In the majority of cases the spasm is painful, and follows voluntary movements elsewhere or an attempt to move the affected limb. "Jumping" is the term generally referred to it by the patient, and a wound which "jumps" when it is dressed is common in local tetanus. In three cases in my series the "jumping" when the wound was dressed progressed to further symptoms of tetanus, including trismus, but recovered. I am convinced that the tetanus toxin affects the muscles themselves, in addition to the nerves.

(*b*) *Increased Reflex Excitability.*—Besides the patellar reflex in the leg, the elbow reflex in the arm, &c., the reflex tonic contraction lasting for many seconds, sometimes minutes, may be observed on exciting the distal muscle; thus, squeezing the quadriceps extensor will produce a tonic spasm lasting several seconds in the gastrocnemius, and vice versa. A similar effect may be produced in the arm by squeezing the biceps. If the hand is laid on the contracting muscle at the moment of contraction, either in these tonic spasms or in the clonic spasm of the quadriceps on testing the patellar reflex, the muscle is felt to be contracting irregularly. Its different fibres are apparently contracting at different rates.

(*c*) *Tonic persistent spasm* of groups of muscles or of single muscles. Occasionally in the vicinity of the wound alone or, as in the case shown to-night, affecting the whole of the foot. The local tonic spasm in the vicinity of the wound is often masked by chronic œdema. In two cases of my series a curious "wandering" œdema occurred, persisting for several days.

I should hesitate to diagnose local tetanus on *local tonic spasm* alone, but it is essential to give a guarded diagnosis in such a case if it be only seen once, unless a clear history is obtained of the presence or absence of twitching or painful jerks or cramp at night. In all my series (45) of local tetanus, twitching, either local or general, was present, and in the majority of instances preceded the appearance of tonic local spasm by several days where the cases were under direct observation. In four of my cases there was no clonic spasm nor twitching up to the time of occurrence of the disease. In one instance, a wound of the back, the muscles in the immediate vicinity were hard and tense, and only two days later twitching of the upper fibres of the trapezius was noticed. This case was further complicated by rigors; no malaria parasites were found in the blood, but no trismus nor other symptoms of generalized tetanus appeared. Twelve of the series had complete rigidity of one or other limbs. The majority of all cases gave ankle clonus, which is not confined to wounds of the lower limb, but where the lower limb is involved in the wound the clonus always remains persistent on the injured side longer than on the uninjured.

Treatment by intramuscular injections of antitoxic serum was the routine method used in my series of local tetanus, and improvement of the symptoms always took place, often in cases of apparent long-continued contracture. It is difficult to understand the action of the serum in view of the generally accepted theory that once the toxin has gained entrance to the nervous system it is inaccessible to the neutralizing action of antitoxin, unless there be some direct muscular linking of the toxin with the muscle substance.

All cases of contracture should be treated with intramuscular doses of high potency antitetanic serum. There is practically no danger of anaphylaxis with the quantities of serum necessary—2 or 3 c.c.—and the improvement of an obstinate case of contracture under the influence of the serum is in favour of the diagnosis of local tetanus.

Mr. Jocelyn Swan omitted to state that in his case the spasm was preceded by pain commencing under the toes, two days before the clonic spasm.

Dr. F. W. ANDREWES, F.R.S.

The definition of local tetanus is easy from the pathological point of view. It is the form of tetanus seen in a relatively insusceptible animal, or in a partially protected man, in which generalization of the disease does not occur, and the bulbar and pontine centres are shielded. The manifestations of disease are confined to the cord segment in relation with the nerves from the wound, or, at most, overflow from this to segments immediately adjacent.

The clinical definition is more difficult because there exists no definite criterion of what is, and what is not, tetanus. The one criterion which might have been expected to prove reliable—namely, the presence of the tetanus bacillus in the wound—is not conclusive, because the bacillus is not rarely found in the wounds of those who show no signs of tetanus. Every gradation exists between purely local tetanus and the generalized disease; the latter may supervene on the local type as protection becomes less and less.

During the past eighteen months I have been asked to see twelve cases of what I believe to have been local tetanus, but some of them were seen on one occasion only. The muscular spasm of local tetanus may be tonic or clonic, but most often presents a combination of the two. The presence of this combination—tonic rigidity of the muscles with painful cramp-like exacerbations—is distinctive of local tetanus, especially if the deep reflexes are at the same time exaggerated. Such cases—and they form the majority of those I have seen—present no difficulty in diagnosis. In three of my cases tonic rigidity was absent, and the spasms were clonic only. The diagnosis here is more open to doubt. Two cases yielded in a few days to intrathecal antitoxin, and it is possible that this fact may assist in the diagnosis in clonic cases. Those cases, however, in which tonic spasm is predominant seem to show little or no response to antitoxin, however administered. One case was peculiar in the fact that the spasm seemed purely tonic. There was pure rigidity of the thigh, without pain, and with no increased deep reflexes.

My conclusions from the cases of local tetanus I have seen are that the combination of local rigidity with painful clonic spasm is diagnostic, but that purely clonic or purely tonic spasm is more difficult to be sure about. Increased reflex excitability is in favour of tetanus, and the subsidence of clonic spasm after antitoxin is an argument in the same direction.

Dr. Wilfred Harris.

There is no one, two, or even three symptoms by which one can make the diagnosis of local tetanus. As Tetanus Inspector for South London, I have now seen a fair number of cases, and it seems to me one must take the whole picture into consideration, as well as the surroundings and the history. If asked to pick out one or two symptoms of predominant importance, I should agree with Dr. Andrewes about tonic and clonic spasm associated with pain, the pain being of a degree and character far exceeding the amount of spasm present. You see a man with a spasm which, in itself, you would judge, should not cause pain, yet there is intense agony. I think clonic spasms rather than tonic are important, but it is difficult to rely on them. Recently I saw a case in an officer, who had been wounded in October. About a month ago, he began to notice that he could not bend his left knee. He had a wound on his heel, and several others elsewhere, but the heel one was all that remained. He got a curious rigidity of the left thigh, the extensors of the thigh being particularly involved. He had no pain. At times it would relax. That went on for a week without either pain or visible clonic spasm. At the end of a week he had tonic spasm, and with it, pain, and I think that is a strong diagnostic point. He also developed some trismus, and even then the diagnosis was overlooked, the trismus being accounted for by the fact that he had had a wound at the angle of the mouth, and teeth had been knocked out. When I saw him he was a typical picture of tetanus: with a stiff neck, some retraction, and trismus, with extraordinary rigidity of the thigh, with tonic spasms; it was local tetanus gradually becoming generalized. What seem to me to clinch the diagnosis are the painful clonic spasms supervening on the tonic. Tonic spasm alone I cannot believe is necessarily tetanic, because it follows an ordinary wound, as in the cases I have shown to-night. In my own experience, and that of others, these cases are most resistant and difficult to treat. I have tried to straighten these fingers and put them into splints, under an anæsthetic. I straightened one, I thought successfully, until I found I had fractured one of the metacarpal bones. The condition in the man who has spasm of his face has been going on ever since last August: there is no pain, nor sign of generalized tetanus. Obviously, irritation of the facial nerve is causing the spasm, which is mechanical, not due to tetanus. I am not inclined to lay much stress on the disappearance of a symptom under treatment. I do not think we can be sure of tonic spasm, as such: I lay great stress on the fact that pain is disproportionately severe compared with the amount of spasm.

Mr. R. C. Elmslie.

In an orthopædic hospital these cases with tonic spasm are so common that we do not pay special attention to the condition. At present, in one small auxiliary hospital, I have five cases of spasm causing inversion of the foot, the foot being in the posture of talipes equino-varus. In only two of those cases has there been a wound: the others are cases of injury of the ankle associated with considerable shock, the patients having been blown up and buried. In one case the wound was superficial on the dorsum of the foot, and in the other a deep penetrating wound of the calf, in the region of the posterior tibial nerve. I think we can acquit the unwounded cases of having been due to tetanus, and they react to treatment in the way one would expect of functional cases—that is, by giving an anæsthetic to make sure there is no mechanical obstruction to mobility, and treating them afterwards by suggestion and electrical stimulation. This does not cure all functional cases, for there are some which resist every known form of treatment. I might mention two cases of spasm in the hand which I have at the present time. One is very similar to a case shown by Dr. Harris, in which the little and ring fingers are contracted by spasm. It was due to a foreign body, which had penetrated the muscles on the ulnar side of the forearm, and the spasm disappeared on the removal of the foreign body. In the second case there is spasm of all the fingers of the hand, following a penetrating wound of the radial side of the forearm. It is a most resistant spasm : the hand was originally in the flexed position. I gave an anæsthetic and extended the fingers, and the patient then had spasm in the extended position. On pulling the fingers into the flexed position by splinting, he again had spasm in the flexed position. All these spasms are tonic. I have seen only a few cases of clonic spasm, excluding cases of shivering stump, which are in a different category, as they are almost certainly due to irritation of the cut ends of the nerves. I can think of one patient with clonic spasm in which there had been a wound in front of the head of the tibia, which had been healed for several months when I saw him. He had tonic spasm of the quadriceps extensor femoris, and whenever he, or anyone else, attempted to flex his knee, the quadriceps went into severe clonic spasm of a painful kind. I feel that this may have been a case of local tetanus.

I am surprised at the answers Dr. Golla has received from surgeons. If neurologists and orthopædic surgeons had been asked, I am sure there would have been replies in the positive. Many hysterical deformities in civil life are produced by spasm, and hysterical deformities following injury are not uncommon. I could mention two or three straight off which have come my way in a few years. I have now, attending St. Bartholomew's Hospital, a girl who had a slight cut on the ulnar side of the wrist. She came to the massage department, where the sister showed her to me because she had the little and ring fingers contracted. At first I thought the ulnar nerve was severed, but there was no cause for that suspicion. She had spasm similar to that in Dr. Harris's cases. She was receiving compensation for the injury, and I do not doubt it was a functional case. I made light of the case to her, and told her she could return to work in a few days' time. She then developed convulsive facial spasm, almost identical with that shown by a patient this evening. Here, then, was a case of both tonic and clonic spasm due to a functional condition, and those who have insurance work will have seen such cases in relation to compensation claims.

Professor C. S. SHERRINGTON, F.R.S.

My experience of tetanus in man is limited to very few cases, some years ago, but I have handled many hundreds of animals—mice, guinea-pigs, rabbits—and I have seen some dozens of horses with local tetanus since the War began. But in no instance have I seen clonic spasm of any kind resembling that in the cases described, and I do not think it could have existed without it having been observed. By handling the animals so much one is accustomed to the feel of the muscles, and if one injects a guinea-pig in the subcutaneous tissue the quickest way to detect the earliest symptom is to simply pick up the pig with one hand. In such, clonic spasm is, in my experience, unknown. And I take it Dr. Ransom will agree it is very rare as he has only seen two cases.

Dr. CAMPBELL THOMSON.

These contractures occur more frequently in civil life than would appear from the returns. One typical case occurred at the Middlesex Hospital three or four years ago. A woman had an injection into the forearm which was followed by intense contraction of the fingers, similar to the case Dr. Wilfred Harris showed to-night. From what has been said, it is evident that these cases of local tetanus will require to be very carefully diagnosed from cases of functional disease of the nervous system.

Dr. C. M. HINDS HOWELL.

Dr. Campbell Thomson is right as to the care which must be exercised in avoiding the classing of these cases of tetanus as hysterical: a mistake the other way round is not so serious. I particularly recall one case occurring last summer, while I was acting for Dr. Andrewes as Tetanus Inspector, which was similar to the case which Mr. Elmslie referred to, of inversion of the foot and contracture of the tendo Achillis. The tendons had been lengthened, but the contracture still persisted. A foreign body was subsequently removed from his tibia, and he then developed generalized tetanus, and died. I do not doubt that this was a case of local tetanus to begin with.

With regard to contracture following cases of injury in civil life, I have at present, in the National Hospital, a girl whose arm was injured with a ginger-beer bottle, and as a result, she developed flexor contracture of the fingers. Electrical treatment for this was followed by clonic spasm of her arm on the affected side, obviously of an hysterical character. She is now in process of being cured. I can remember one or two more of similar character, and feel sure that the condition is by no means uncommon.

The CHAIRMAN (Surgeon-General Sir ANTHONY BOWLBY, K.C.M.G.).

We do not see so many cases of local, late, tetanus in France as you do here. But, on the other hand, we see these cases of contracture following shortly upon injuries. There is no doubt that, in a considerable number of cases, there is a good deal of local spasm following upon injury, and we see this in the so-called hospitals for shell shock. In many cases these contractures, which are evidently functional or hysterical, yield comparatively quickly either to suggestion or to nursing or other kind of care, and keeping the men under as favourable conditions as possible, having removed them from the noise, stress and strain of battle. But we do occasionally see cases of local tetanus. My experience, in general terms, may be stated in this way. The local spasm which supervenes very quickly after the injury is nearly always functional. When it supervenes later it is more likely to be due to local tetanus. We have laid much stress on the reflex extension of the leg at the knee-joint, caused by tickling the sole, and in the cases I have seen of local tetanus there has been the painful clonic spasm on which other speakers have laid stress.

DR. GOLLA.

I cannot help feeling, after what Dr. Head has been telling me, that there is probably some ambiguity about the question. Possibly those of whom we made inquiries did not think we wanted them to refer to cases which they regarded as hysterical: they may have thought we were looking for records of only organic cases. No doubt that accounts for the negative character of the reports which, to a neurologist, is a startling thing.

The CHAIRMAN.

I think we may take it that the occurrence of local tetanus is very good evidence that the treatment by prophylactic injections of anti-tetanic serum has been successful in preventing tetanus in a very large number of our soldiers.

Major-General Sir DAVID BRUCE, K.C.B., F.R.S.

In summarizing the remarks and statements made by speakers, it appears to me that as far as I can understand the matter, this discussion has failed to bring into prominence any decisive point that can be of guidance in the elucidation of what appears to be a new clinical problem. This is all the more to be regretted as there seems reason to believe that a fairly clear issue lies in the diagnosis between functional spasm and local tetanus.

One thing alone emerges fairly clearly, and that is, that a contracture of only a few days' standing which cannot be completely abolished by anæsthesia must be regarded with suspicion, and the medical officer will not go far wrong in resorting to vigorous antitetanic measures.

The inquiries into the frequency of contracture in civil surgical practice made by the War Office Tetanus Committee yielded surprising results.

Dr. Batten and Mr. Elmslie remarked that if neurologists and orthopædic surgeons had been consulted, there would have been many more answers in the affirmative. This may be true, but at the same time the fact seems to stand out, that these cases of contracture are far and away more numerous in war wounds than in civil practice. The excuse for addressing the circular to surgeons is that it is the civil surgeon who sees and treats the vast majority of injuries received in civil life, and not the neurologist or orthopædist. Probably the reason of this greater frequency of contracture after war wounds is to be found, partly in the intensely irritable psychical condition incurred by the strain and stress of modern war, and, to a much greater extent, in the earth contamination of wounds by fragments of shells and the grossly infected condition of the soil brought about by trench warfare. A practical point arising out of the discussion seems to be that it is very important to separate the cases of functional spasm from the cases of local tetanus at the earliest possible date, in order that each may receive appropriate treatment. We have seen the results of delay in functional cases in some of the cases exhibited to-night, and I have read in many reports of war tetanus cases the result of neglecting or mistaking the symptoms of local tetanus. This has resulted in the spread of the mischief to more vital centres and too often in the death of the patient. There must be early diagnosis and early treatment, else we shall see hundreds of discharged soldiers with incurable deformities due to neglected functional spasm or local tetanus, as well as a heightened rate of mortality in our tetanus returns.

Before sitting down I would like, if time permits, to place on record some of the more prominent features of tetanus as they appear in the light of our experience in the present war. First, the undoubted usefulness of prophylactic injections. As the Chairman said the occurrence of local tetanus is very good evidence that the treatment by prophylactic injections of antitetanic serum has been successful in preventing tetanus in a very large number of our soldiers. During the first months of the war these injections were not given and the incidence of tetanus was high. As soon as they were introduced this fell at once to about a tenth of the former incidence and has remained down ever since. With the introduction of the prophylactic injection the incubation period was much lengthened and the rate of mortality gradually fell from 57·8 per cent. in the first year of the war to 14·8 per cent. in the last analysis.

Another fact which stands out is that tetanus bacilli may lie *perdu* for months or even years in the depths of wounds, whether healed or unhealed, and that operative interference is apt to light up the smouldering embers. In the last published analysis there were thirteen such cases where no prophylactic injection was given before the operation. Nine out of the thirteen died. I suggested in this analysis,[1] that at the present time and with our present knowledge it is a question as to whether a surgeon who neglected this prophylactic injection before operation on war wounds would not render himself liable to an action for malpractice.

Another interesting feature of tetanus in war time is that cases of injury to tissues without visible breach of continuity of the skin, as in trench foot, are often followed by tetanus. At first these did not receive a prophylactic injection and the cases of tetanus occurring among them were almost all acute and fatal. When this was remedied by giving all cases of trench foot a prophylactic dose of antitoxin this fatality ceased.

Not only is there a danger of the tetanus bacilli gaining entrance through unconsidered breaches in the skin, but the intestine itself must be considered in war time as a danger zone. Living and eating and drinking while smothered in the mud of the trenches must lead to the swallowing of many tetanus spores. It is not to be wondered at then that several cases of operation involving the intestine, such as appendicitis, have been followed by tetanus. Captain Tulloch, a member of the Tetanus Committee, has found the bacilli in the intestinal contents of such cases.

[1] A summary of this analysis appears in the *Brit. Med. Journ.*, March 16, 1918.

Another question of practical importance in connexion with prophylaxis is, whether several injections are more efficacious than a single one. The Tetanus Committee laid down in their latest memorandum, dated June, 1917, that four prophylactic injections should be given to every wounded soldier at intervals of seven days. What has been the result? Well, as to case mortality I think I am almost justified in saying that multiple injections make little or no difference. In the last 300 cases of tetanus, the rates of mortality in those who received one, two, three or four injections were 22·2, 16·1, 15·7, and 13 per cent. This would appear on the face of it to be in favour of multiple injections, but when it is considered that men may be attacked by tetanus before they can receive their second or third dose, this apparent advantage disappears.

In regard to the rate of incidence, which underwent such a marvellous change as the result of one prophylactic injection, nothing at present can be said. The data for the calculation are not yet available. I think, however, that we are justified in believing that a lowering of the incidence-rate will be found to have taken place as the result of the multiple prophylactic injections. Until further evidence is forthcoming I would strongly urge that the four injections be continued.

Another matter of controversy is the dosage in prophylactic injections. The War Office Tetanus Committee laid down that 500 units should be the standard dose as well in primary as in secondary injections. In France the teaching is, according to Sir William Leishman, that 500 U.S.A. units is a sufficient dose in the majority of wounds, but in instances of severe injury, where the wounds are large or deep and are heavily contaminated, especially if accompanied by fracture of bone, the dose of 500 U.S.A. units is not so good as one of 1,000 or 1,500 units. For my own part when I consider how impossible it is to prophesy from the appearance of a wound the chances of the occurrence of tetanus, I am still in favour of a standard dose; it leaves nothing to the idiosyncrasy of the operator, it renders statistics more easily manipulated, there is less risk of serum rashes from the smaller dose of foreign serum, and therefore until it is proved that 1,500 units is appreciably a better dose in lowering incidence and mortality, I will vote for the standard dose of 500 units repeated four times at intervals of a week.

I agree with Major Cushing, U.S.R., however, that "in the interest of our troops the question of whether a single dose of 1,500 units is preferable to repeated doses of a smaller amount ought to be definitely settled." Yes, but how to settle it?

The introduction of a double serum against tetanus and gas-gangrene which has recently been effected through the energy of Dr. O'Brien, a member of the Tetanus Committee, working with toxin prepared by the method of Major Bull, of the U.S. Army, should still more lessen the incidence of tetanus as one of the preparers for the tetanic feast will probably be absent.

Lastly, in regard to the prophylaxis of tetanus it is up to the surgeon to do the rest. I remarked in the Sixth Analysis of cases of tetanus treated in home hospitals from March to June, 1917: "Surely it is not beyond the ingenuity of man to devise some form of surgical treatment which will prevent the development of gas gangrene or tetanus in wounds. Perhaps the addition of an anti-gas gangrene serum would have some effect." We will now have an opportunity of learning whether this is so or not. If the result is not wholly satisfactory then a treble serum including a *Vibrion septique* antitoxin must be tried.

Secondly in regard to the results of the therapeutic use of antitetanic serum we are on much more unstable ground. To arrive at a true estimate of the curative value of antitoxin is of the greatest importance, but unfortunately it presents great and, up to the present, insurmountable difficulties. It is difficult to convince oneself that the therapeutic use of antitoxin has any real value. Perhaps, however, the following argument may have some cogency. The rate of mortality in pre-serum days was 85 per cent. During this war the percentage among those who received no prophylactic inoculation, but were treated with serum after the disease appeared, was 58 per cent. The difference between 58 per cent. and 85 per cent.—27 per cent. would therefore seem to give the value of the therapeutic treatment of antoxin. But the subject bristles with fallacies.

Another question in the therapeutic use of antitoxin which has given rise to controversy is the route of injection. In France there is a tendency to extol the intramuscular at the expense of the intrathecal. and in this country also the intramuscular route is becoming daily more popular. Its ease and freedom from danger are doubtless in its favour. My working hypothesis, however, is still, that in general tetanus the best method of treatment lies in the earliest possible injection of large doses of antitetanic serum by the intrathecal route.

It is impossible to prove this from human statistics, but Professor Sherrington has shown in the most unmistakable manner that in experimental animals, inoculated with a known lethal dose of toxin, the intrathecal route is by far the most efficacious. In the monkeys treated subcutaneously or intramuscularly the disease was hardly retarded in its fatal result, whereas almost all those treated by the intrathecal route recovered.

At the second session of the Research Society of the American Red Cross in France, Major Cushing, U.S.R., agreed that general experience was in favour of the intrathecal method when the disease was once established, and he wondered whether the size of the lumbar puncture needles which were employed might not possibly account for the unpopularity of this form of treatment. Some years ago when the lumbar puncture outfits in England were submitted to the Tetanus Committee for criticism it was found that many of the needles looked more like gas tubing than needles suitable for lumbar puncture. These were turned down, and the Committee recommended that needles 2¾ in. and 3½ in. in length and 1 mm. and 1·2 mm. in diameter should be used. For my own part I am sure the hesitation to use the intrathecal route is due to its difficulty and supposed danger. Like many minor operations it requires some practice and experience. Many Medical Officers have never performed it. *Omne ignotum pro magnifico.* Nevertheless, reasoning from animal experiments, the intrathecal route is far and away more effective than the intramuscular and subcutaneous route.

To sum up the whole therapeutic matter. Antitoxin has no power of neutralizing toxin fixed in the nervous system. If a fatal amount has been absorbed then no amount of antitoxin will save the patient's life. If there is any free toxin circulating in the blood or lymph, the antitoxin can neutralize it, and so prevent further mischief. If then a fatal amount has not been absorbed the injection of antitoxin may be of use. By animal experiment it is proved that the intrathecal route is the best. It is believed that the intravenous route is also good, but more dangerous on account of the liability to anaphylactic shock. Therefore the Tetanus Committee, in their last Memorandum, recommended antitoxin to be given intrathecally at the first sign of tetanus.

The Royal Society of Medicine.

President—Sir RICKMAN J. GODLEE, Bt., K.C.V.O., M.S.

The Early Treatment of Wounds at the Front, with Special Regard to Primary Suture.[1]

By Sir ANTHONY BOWLBY, K.C.M.G., K.C.V.O.

(ABSTRACT.)

[This paper is printed *in extenso* in the *British Medical Journal*, March 23, 1918, p. 333.]

(1) THE most important treatment is the careful dressing of the wound and the subsequent complete excision of all dead, badly damaged, or grossly infected tissue. It is never necessary nor right to remove more than a very thin edge of skin.

(2) Complete asepsis is of the highest importance.

(3) The wound may be sutured at once, or may be left unclosed for one, two, three, or four days. It can then be sutured with results which are identical with those obtained by immediate suture.

(4) If, after operation, the wound is not sutured, it should be dressed in such a way that it need not be disturbed till the time for closure has come.

(5) If the wound is sutured at the time of operation, the patient should be retained in hospital for not less than a week. If he cannot be retained then the wound must not be sutured.

(6) It is very seldom that even after the most careful operation the wound is sterile, and bacteria can generally be found in the secretion on the following day. The presence of a few bacteria, whether anaërobes or the ordinary pyogenic forms, does not prevent healing of the wound.

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[1] At a meeting of the Society, held February 13, 1918.

Streptococci which can hæmolyse blood constitute the most dangerous form of infection, and if they are detected the sutures should be at once removed.

(7) No definite rule can be laid down as to the lapse of time after which suture should not be done, but the sooner the wound can be *operated* upon the greater is the probability of success. It can be sutured later.

(8) If the wound edges are found on examination to be already indurated and inflamed, or if definite gas gangrene has already started, the wound must never be closed. Whenever there is any doubt at all it is best to leave the wound open, knowing that it can be equally well sutured in a day or two if this appears advisable.

A SERIES OF 626 WOUNDS TREATED BY PRIMARY SUTURE.

	Total	Healed by first intention	Healed after slight suppuration	Suppurated	Not known
Superficial wounds ...	119	74	9	—	36
Deep wounds ...	260	184	38	30	8
Fractures ...	131	93	17	21	—
Amputations ..	37	24	5	8	—
Joints ...	79	69	—	10	—
Total ...	626	444 (68 per cent.)	69	69	44

ONE HUNDRED CONSECUTIVE CASES OF COMPOUND FRACTURE TREATED BY PRIMARY SUTURE.

	Total	Healed by first intention	Healed after slight suppuration	Suppurated
Femur	18	11	2	5
Tibia and fibula ...	5	4	—	1
Tibia	10	9	—	1
Fibula	2	2	—	—
Humerus ...	34	26	6	2
Radius and ulna ...	3	3	—	—
Radius	15	12	3	—
Ulna	13	8	4	1
Total	100	75 (75 per cent.)	15	10

It will be seen that out of a total of 626 cases, 68 per cent. were successfully sutured and healed per primam.

At the "Observation Hospital" there have been 123 wounds sutured, with success in 83 per cent.

I have already stated that at "Z" there were 453 cases, with successful suture in 67 per cent.

Thus, in these three separate areas there is a total of 1,202 cases under various surgeons, with success in about 70 per cent. And in many of the remaining 30 per cent. there was a very early closure of the wound in spite of superficial suppuration.

The success obtained in cases of fracture is also very striking, for the above table sent by Surgeon-General Wallace shows 75 per cent. of successes. The results of the "Observation Hospital" support this. The numbers of the latter are:—

	Success	Failure
Humerus	5	–
Forearm	1	—
Metacarpus	1	-
Femur	3	5
Tibia and Fibula	2	
Tibia	4	1
Fibula	3	—
Tarsus	4	1
Metatarsus	2	
Totals	25	7

i.e., 72 per cent. of successful suture.

The amputation cases at the "Observation Hospital" have been few, but they also have been very satisfactory. Of eleven cases, all the patients have recovered. Seven were amputations through the thigh; two through the leg, and two were Syme's. Ten out of the eleven healed soundly after either immediate suture, or after closure of the flaps on the third or fourth day. One failed to unite, and suppurated.

Another very satisfactory result is that the whole of the patients have recovered whose wounds were sutured at the "Observation Hospital." There has as yet been no death in the 175 patients operated upon for suture whether the latter was successful or not. They are not all sufficiently recovered to be free from danger at the present moment and included in the table of percentages. Some of them have been too recently operated upon therefore for their cases to be included in the figures I have quoted. But, apart altogether from the question of primary union, it is a very important fact that no one has died. Yet many of these men were very badly injured. More than forty of the 175 had fractured bones; two of them had torn femoral arteries (but have kept their limbs after ligature of the vessel); two had torn posterior tibial arteries; eight had wounds opening the knee-joint, in one of which the patella had to be excised; several had fractures opening the elbow-joint; more than twenty had multiple injuries; two of them required transfusion of blood. I do not believe that all these men would have recovered if their wounds had not been closed.

The Royal Society of Medicine.

President—Sir RICKMAN J. GODLEE, Bt., K.C.V.O., M.S.

The Characteristics of Amputation Neuromata.[1]

By G. MARINESCO.

I HAVE examined the neuromata themselves and the surrounding tissues for the presence of foreign bodies, the total number of which is twenty. These are rarely fragments of shrapnel, and almost always minute foreign bodies (principally fibres of cotton or silk). The foreign bodies are often visible to the naked eye, and in that case the fibres form small bundles; sometimes there is only one, or there may be two or three. Around the bundles there is a wall made up of giant cells, which may penetrate between the fibres to which they become adherent and finally envelop them. One often sees fragments of fibres in the interior of the giant cells situated in a digestive vacuole. The fibres stain red with carmine and fuchsin, and black with hæmatoxylin (Heidenhain's method). Among the giant cells there are fibroblasts, lymphocytes, and mono- and polynuclear leucocytes. Further afield are to be seen inflammatory nodules of various forms and dimensions. These inflammatory nodules, which are very numerous in a case (Captain Penna) in which there were macroscopic fragments of shrapnel in the median nerve, start from the level of the wound, for they can be found in the bones (osteomyelitis) in the muscles (myositis), in the tendons, and above all in the cicatrix. It may be added that the vessels which accompany the nerves have undergone pathological changes such as peri-arterial inflammation, venous and arterial thrombosis, and obliterative arteritis.

The inflammatory character of the lesions and the presence of inflammatory nodules in all or almost all the tissues which form the amputation stump indicate the infective nature of this inflammation. In fact it may be said that in the neuromata examined the changes characteristic of neuritis were always present—viz., areas of perivascular exudation, inflammatory nodules around certain arterioles, inflammation around the nerve-bundles. This endoneuritis and perineuritis throw light upon a certain rôle in the genesis of the pain suffered by subjects of amputation. It is just these lesions which explain two phenomena which we have often witnessed in Mr. Corner's patients—namely, hyperæsthesia to touch, pain and heat, and the pain on compression of the trunks above the neuroma.

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[1] At a Meeting of the Society, held April 9, 1918.

In spite of the inflammatory lesions seen in the nerves there is no one particular organism present in the degenerative lesions of the fibres in the nerve-bundles of which the neuromata are composed. On the contrary, we witness phenomena of collateral regeneration quite close to the inflammatory foci, and even neurotization of the foci.

Histological findings are quite sufficient to show the infective nature of the lesions in amputation neuromata accompanied by pain. But more direct proof must be forthcoming. Cultures made by Mr. Corner in two cases showed in the second case the presence of a microbe, often in the form of a diplococcus, staining by Gram's method, by fuchsin and by thionin.

The amputation neuroma is essentially composed of nerve-bundles of varying arrangement and course in the upper, middle and lower regions of the tumour. It is an important fact that these bundles anastomose, forming a very complicated plexus. In order to differentiate it from plexiform neuromata properly so-called, an amputation neuroma is a neuroma with an internal plexus.

The presence of foreign bodies in amputation neuromata observed in this war modifies more or less profoundly the arrangement of nerve fibres, and subsequent inflammation may favour the proliferation of cicatricial connective tissue. Another practical conclusion may be drawn. If pains are observed with amputation neuromata one should always remove the nerve cicatrix and the surrounding tissues.

The bundles or nerve fibres of the new formation neuroma neurotize the vessels, cicatricial tissue, muscles, periosteum and the bone, as well as the inflammatory foci and hæmorrhagic foci. Moreover I have established a fact of much biological importance: it is the existence of fibres and nerve bundles in the interior of a coagulum. The nerve bundles are formed after the type of the neuroma bundles.

The Royal Society of Medicine.

President—Sir Rickman Godlee, Bt., K.C.V.O., M.S.

The Surgery of Painful Amputation Stumps.[1]

By Edred M. Corner, M.C.

Professor Marinesco has clearly shown that the chief factor causing pain in stumps to be fitted with artificial limbs is inflammatory changes within the nerves, and they have even been found as long as three years and two months after the amputation in a healed stump, a length of time which would allow regenerated nerve fibres to come into contact with the inflammatory foci and thereby become painful. When a nerve fibre regenerates, a single fibre is replaced by many, which immensely increases the chances of pain. Apparently also there is no time limit to nerve regeneration, and this is another possible source of future trouble.

There has been an immense amount of good work done abroad on abdominal wounds, wounds of joints, and wound treatment. This is an attempt to improve the results of the operation of amputation. Before the War we had little or no trouble in this respect, and consequently the knowledge of amputation stumps was confined to very few.

In the first place I would draw your attention to the times after wounds were received that the amputations were done. Some were done immediately, and there is excuse for any imperfections of the technique to be found in the circumstances. But some amputations were done more than six months after the wound was received. Under such circumstances there can be no hurry and no lack of thoughtful technique. The imperfect result is a direct consequence of imperfect work, sometimes done quietly and at home. Probably the imperfection resulted from carelessness which crept into our work during the pre-war period of little infection. The information contained in this paper will, I hope, remove much of this stain on surgery and improve the technique of future amputations.

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[1] At a meeting of the Society, held April 9, 1918.

Before the War it was customary in large hospitals for the surgeon to hand over amputations to the house officer. The injuries of this War have compelled us to study amputation operations and their results, and we have now acquired knowledge which should make the senior more responsible for any demerits in the junior's manual work than has hitherto been the case. If the senior does not do the operation himself, at least he must overlook all the details of his junior's work both at and after the operation. Fresh knowledge in these particulars is indispensable both to the surgeon abroad and the surgeon at home, particularly the latter, who has more time and resource at his command.

The occurrence of painful nerves in amputation stumps was not unknown before the War. Instances of painful scars and nerve endings (bulbs) were known but they were uncommon. Mr. E. Muirhead Little tells me that during the last year undisturbed by war factors, 1913, the Royal Surgical Aid Society had no such cases amongst 529 applicants for artificial limbs. Nor was this knowledge of the troubles of amputation stumps at all diffused among our profession. In 1915 the work of Roehampton House was begun by Colonel Openshaw, who tells me that out of the first ninety-three cases sent there as ready for the fitting of artificial limbs, thirty-eight were rejected by him as being unready. Since this date things have been steadily improving. Whereas at the beginning of the War Colonel Openshaw had to reject about 40 per cent., Mr. E. Muirhead Little has now only to reject numbers varying monthly from 3 to 12 per cent. This very marked improvement has been mainly in evidence in the most obvious disqualifications for the fitting of an artificial limb, such as unhealed sinuses. So much good work has been done at the Casualty Clearing Stations, particularly in the way of the treatment of abdominal wounds and wounds of joints, that we as surgeons are very proud of the good work done there, and feel it incumbent on us, as Mr. Murray, of Manchester, has so well said, "to try to make other fields of surgery as praiseworthy." Let us at Roehampton try to do for amputation cases what other surgeons have done in wounds of the abdomen and joints. With us the main problem is that certain patients, with wounds healed, are unsuitable for the fitting of an artificial limb because the stumps are painful and cannot bear pressure. Moreover, some who have been fitted and have worn artificial limbs come back to us, as they can no longer wear them, because of the pain caused in the stump. It is up to us as surgeons to inquire into these cases, and to see if it is not possible to improve upon that old field of surgery, amputations.

The object of the Queen Mary's Auxiliary Hospital at Roehampton was the fitting of artificial limbs to amputation stumps. It was soon seen that surgeons' ideas differed as to what was necessary or desirable in such a stump, and a work has grown up in which stumps which had been sent to Roehampton as suitable and found to be unsuitable were refashioned and made so that limbs could be fitted to them. This work cannot satisfactorily be entrusted to outside surgeons because of the differences of opinion between the surgeon and the limb-adviser. Thus, at the Roehampton Hospital we have side by side the great "dry-bones" work of artificial limb making and fitting, and the living labour of preparing stumps for the work of the fitter. This second work, arising from an attempt to do the first, has even now to exist on the charity of others. Nothing is provided for its being carried out.

The end of a nerve in healing has to go through three stages, any of which may be minimized, but none can be totally abolished. These stages are: First, the stage of repair and inflammation; secondly, the stage of compression; thirdly and finally, the stage of regeneration. The diminution of the first lessens the power of the second, and makes the third slower and less prolific. To reduce the first it is necessary to close the end of every structure left open by the cutting and sawing of the amputation; infection must be no more; the wound can then be closed and drainage no longer used, the discharge of serum being no longer poured out from open ends of divided structures. In this, amputation surgery comes into line with that of the complete suture of other wounds. The subject of amputation surgery is complicated by an additional difficulty of threefold character—the compression, inflammation, and regeneration of nerve endings. Professor Marinesco has made some beautiful slides demonstrating the presence of active inflammation in the nerve ending, and showing how active inflammation is kept alive by the irritation caused by the cellular ingestion of vegetable and unabsorbable ligatures. These four preparations show something similar—a focus of inflammation in a nerve bulb and a flattened focus higher up in the nerve trunk, indicating more general inflammation of the bulb. They will serve to remind you of the active trouble that there is in some nerve endings, the "burning fiery furnace" through which the unprotected regeneration fibres have to grow, and the continuous production of fibrous tissue which will contract and compress the young nerve shoots. The specimen which showed the large cluster of inflammatory cells was removed from a man whose forearm was shattered. Two years after the receipt of the wound, and in quiet leisurely circumstances, the limb was amputated. This operation necessitated three further operations for painful nerves! The surgery of amputations at home offers scope for improvement!

It is curious that these changes should go on in the nerve ending, and that the products of irritation from ligatures should be carried to the nerve lymphatics. The condition reminds us of tetanus, in which the poisons are conveyed by the neural lymphatics to the nervous system. The amputation stump patients often have pain in spasms and of a cramplike nature, this increasing the resemblance.

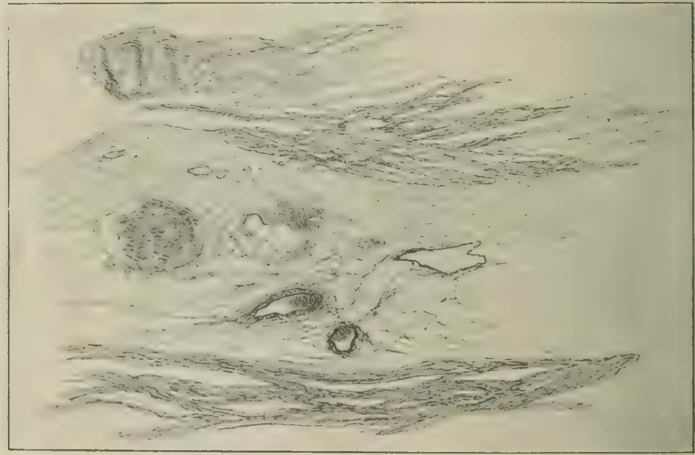

Fig. 1.

C. Wounded, May 23, 1915. Sequestrectomy of clavicle twice in 1916. Suture of brachial plexus above clavicle, November, 1916. Amputation, March, 1917. Section of nerve bulb showing inflammatory nodule in the increased connective tissue between the nerve bundles. Also hypervascularity and œdematous connective tissue.

Compression was regarded as the main cause of pain before the advent of the great era of infection. It acts in hundreds of ways and is mainly brought about by the contraction of the fibrous tissue on to the expanding growing tissue of the cut nerve end. The contracting cap of fibrous tissue over the nerve end is derived from the endoneurium, perineurium and epineurium, coats of the nerve. Through the meshes in them the unprotected regenerated fibres have to pass. This is the main place at which the strangulation of the nerves takes place. If any of the nerve sheaths can be uninjured in the division of the nerve, so much less the strangulation of the new nerve fibres to be formed by regeneration. This is shown by the different shapes of the nerve endings if the epineurium is preserved or not. If no epineurium is retained the nerve ending is truly bulbous. If the epineurium is retained the nerve ending becomes pencil shaped. Between these two extremes there are any number of variations in the shape of nerve endings, such as those obtained by my "swinging-door method" of dividing nerves. A method, adopted on a considerable scale and studied extensively, was designed to divide the nerve fibres and retain the nerve coats as much as possible. In the first place the nerve was divided by a tenotome plunged in its side. But this way proved to be most erroneous, a number of fibres were undivided, especially those in the peripheral parts of the nerve and unless the nerve was large the

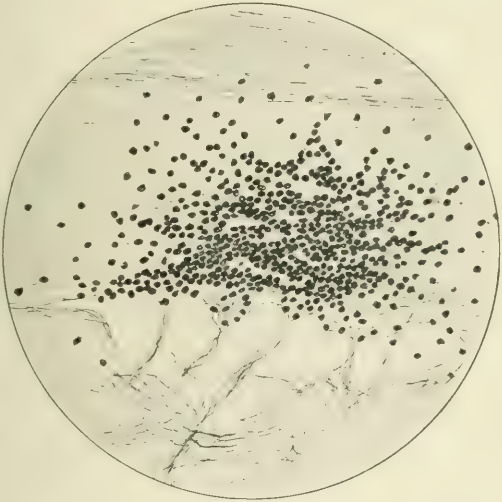

FIG. 2.

The inflammatory nodule, shown in fig. 1, under a higher power.

operation was difficult. The methods of attaining this end consisted in crushing the nerve, preferably with an appendix clamp with smooth surfaces; serrations cut the coats of the nerve, usually after an injection into the trunk and proximal to its crushing. By means of the clamp it was possible to pull on the nerve, thereby possibly inflicting injury on the nerve cell in the spinal cord responsible for the early regeneration of the fibres. Fortunately perhaps for the patient, the force of the pull appeared to be lost amongst the cords of the plexus and there was never any evidence of its having reached the spinal cord. But the method was discarded, as study of the microscope sections showed that many forms of clamp divided much of the connective tissue coats and were uncertain in the division of the nerve fibres. The best and surest results were obtained with the appendix clamp that Messrs. Allen and Hanbury made for me in 1903. Clinically the physiological division of the nerves was unsatisfactory and the method was dropped, but was revived when the nerves were divided below the crushed part. I do not think that it matters exactly how the nerve is divided below the crush, which is the actual division and which preserves the epineural sheath, and a certain and varying amount of the endo- and peri-neural sheaths. The distal division should be made with a sharp instrument and by two snips so as to cut "swing doors." Immediately around the nerve ending is a zone of inflammatory tissue, such as would be produced by the escape of the fluid of an intraneural injection into the connective tissue, which will contract on the nerve bulb and in time may strangulate

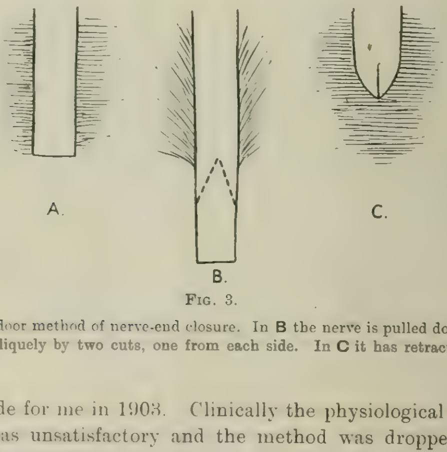

FIG. 3.

Swing-door method of nerve-end closure. In **B** the nerve is pulled down and divided obliquely by two cuts, one from each side. In **C** it has retracted and closed.

a young regenerating nerve fibre. But the tissue farther from the nerve-wound scar tissue, having ceased to contract, becomes a mere scaffolding along which the nerve fibres grow, like a stick or string for training a scarlet runner. Thus are the new fibres directed into the skin scar, the old track or a new track. They grow and progress along the lines of least resistance and naturally progress most quickly where they have an abundant blood and lymph supply as in the inner side of the arm.

We have now considered two stages and factors in nerve healing—namely, inflammation and compression—the new and the old factors in clinical pain—there remains for discussion the stage of regeneration. In order to produce a painless stump the stage of inflammation and the contraction of the scar tissue should be over before the nerve fibres push their way out of the nerve bulb and through the meshes of the tissue of the " internal " scar. Normally, more or less of this takes place, but in these war cases regeneration begins very early after nerve injury and its processes proceed rapidly, this perhaps being due to the increased vascularity brought about by the inflammation and the irritation of the sepsis. Thus the nerve fibres begin to regenerate, in a few days according to Italian authorities, and they branch and branch again; one original fibre becoming represented by many fibres, each of which is a potential source of trouble—it may encounter an inflammatory nodule or become compressed. Thus possibilities of future trouble are manifold; the early regeneration enables the young fibres to become long enough to be strangulated before the scar tissue has ceased to contract, and the greater number of branches of new fibres make the occurrence of trouble still more probable.

After the growing fibres of nerve regeneration have escaped from the nerve bulb and the zones of certain and possible compression, they follow the direction of the scaffolding of connective tissue and make their appearances in strange places, completely foreign to the course of a nerve. For instance in one example regeneration had taken place from the sciatic nerve and reached the popliteal artery, where part of the advancing regeneration fibres followed the artery through the adductor magnus and appeared on the front of the thigh, forming a tender lump. In other cases the progress of the regeneration fibres is like the course of a river, and where an obstruction is met the flow pools up until the obstruction is overcome. Thus regeneration neuromata are formed and mark the site as a regeneration " pool." No less than three neuromata were present, attached to the long saphenous nerve in the case of Thomas and the small sciatic nerve in the case of Anstey. So far as I know at present these regeneration neuromata seldom possess foci of inflammation, but they may be sources of pain, not of spontaneous pain but they cause pain when pressed upon.

In inflammation and compression, two forms of the causation of pain are in evidence. The inflammation does not remain in the nerve bulb and only extended peripherally, but it also progresses up the nerve as an ascending neuritis. How far this extends up the nerve trunk it is far from easy to say. Clinically I do not believe that it extends far; and more practically, not so far as the epineuritis, as is evidenced by the adhesions on the outside of the nerve. It is safe to divide the nerve above the adhesions with every expectation of being above the ascending (endo-) neuritis. When it is impossible to divide the nerve above the epineural adhesions, as happens in some axillary and sciatic cases, only a guarded prognosis can be given.

Clinically there may be distinguished at least five types of pain in amputation stumps, the first of which is universal and the last uncommon.

First.—Early pain, coming on immediately after the amputation, dependent on an endoneuritis set up by the injuries inflicted on the nerves at that operation. When alone present this accession of pain dies away in a few days or weeks.

Secondly.—Compression pain, coming on about two months after the interval and sometimes steadily increasing. For instance, one man had little pain after the operation but after three or four months had elapsed he began to feel pain in his lost limb and the stump grew tender. This increased progressively, until about eight months after the amputation he sought relief. His nerves showed no great amount of inflammation but severe strangulation of the inflamed nerve fibres. When and if bearable this pain may pass off as the nerve fibre dies or the scar tissue ceases to contract.

Thirdly.—Inflammatory pain. The early pain never passes off, or it may become paroxysmal and severe, or it may even be still more clearly defined as in a man recently under my care. A little over three years ago his arm had been amputated. He got through the first and second stages in the healing of nerves very well. About eighteen months after the amputation his nerve ends began to enlarge and he experienced increasing pain and tenderness in them. Finally these enlargements induced him to seek relief and he came as a discharged man to St. Thomas's Hospital. The nerve endings showed inflammatory changes in them, and Professor Marinesco demonstrated the presence of regeneration nerve fibres in a coagulum—i.e., where the inflammation was so intense that an abscess was visibly present.

It is frequent to find a combination of compression of inflamed nerve fibres which cause pain. If the fibres were not inflamed the patient might not be driven to seek relief. As they are inflamed some patients come to the hospital for relief as discharged soldiers. Recently I had no less than four such cases under my care at one time, in each of which the amputation has been done about three years previously.

Clinically these cases may be grouped in a series; pain immediately after the operation is due to the trauma of that operation and the inflammation of the repairing tissues; a little later, after from two to ten months, the pain is due to the compression of the nerve fibres by the contraction of scar tissue; later still, pain is due to active inflammatory changes in the nerve ends.

These three clinical types are distinctive both clinically and pathologically. This is not so in the fourth type; nerve regeneration gives rise to no special clinical symptoms, and consequently the fourth type has no peculiar features. .

The *fourth* clinical type is produced by the regeneration of nerve fibres. It is characterized clinically by more continuous pain and illusions as to the presence of the missing part, for instance the amputated foot. The pain is acute, and first appears within a few days of the amputation. At first it is not great, but increases in severity. At the beginning the new axis-cylinder has no myelin sheath to protect it, and it is easily rendered painful. As the inflammation in the wound subsides the pain becomes less, until the fibrous tissue of the internal scar begins to contract and to squeeze the new nerve fibres. Now the pain may be very severe and may last some months. If there are no foci of inflammation, as around unabsorbable ligatures, and if the fibres are not injured, pain disappears, only to be aroused again by injury or inflammation. The pain is eased considerably by heat, and in some cases by X-rays.

The fourth type has clinical features of both the second and the third type.

The *fifth* type is only recognized clinically by the process of elimination, nerve trunk after nerve trunk is removed by operation and their consideration eliminated by trustworthy surgery. Still the pain and tenderness persist, and are not of neurotic origin. The pain may originate from the irritation and inflammation round a silk ligature or other foreign body. Or the skiagram may show that it is due to disease in the bone; terminal rarefaction of the bone is due to the injuries of operation and the healing of the wound, but when the bony changes are farther afield they are partly due to nervous irritation—dystrophy. Such cases exhibit the usual signs of bone pain, night pain and pain in wet weather, and are very persistent. They are frequently called neurotic patients, but the fault is not theirs but ours—we have not found out the cause of the patient's pain. We should bear the reproach. The stumps of patients of this type frequently twitch and twitter.

In the Museum at the Royal College of Surgeons of England there are two portions of limbs which had to be re-amputated; these I have had the opportunity of dissecting and examining through the kindness of Professor Arthur Keith. Their examination gives a fair idea of the conditions present in the great majority of painful stumps. First, the scar visible on the outside is far smaller than the scar on the inside, and invisible in the undissected limb. It is to this internal scar to which I would specially direct attention. I should like to speak of this internal scar as wound callus. It forms a layer over the bone end from ¼ in. to 1 in. thick. It consists largely of fibrous tissue in which foci of inflammation are present, and in which micro-organisms are imprisoned. Cultures, principally containing staphylococcus, have been made from the scars of all tissues in the stump. The superficial layer of this internal scar is fairly plane, but the deeper surfaces emit prolongations upwards along the lines of least resistance—the planes between structures, and communicate with the general connective tissue of the limb and its lymphatics. Especially does this internal scar transmit processes along the vessels; a most important consideration in passing surgical judgment on a painful stump. Next to vessels the outer sides of nerves would appear to offer most facilities for the upward spread of the infective inflammation. But this infective inflammation spreads to the muscles, and is recognized in the pale brown muscles, dotted with islands of fibrous tissue. It enters the bones, as is shown by the softening of the bone end and the translucency of the terminal part so often seen in skiagrams. This infective inflammation reaches ligatured vessels, causes venous, arterial and lymphatic thrombosis, phlebitis, lymphangitis and arteritis. If the ligature material is of the unabsorbable kind, like silk, it will not be easily encapsulated by this inflammatory infective internal scar, but it is apt to become a source of irritation. Thus silk ligatures turn up again and again, and are responsible for a large number of sinuses in amputation stumps, the sinuses healing at once when the unabsorbable material is removed. But they may persist and cause pain for a long time. Indeed they are one of the causes of pain apart from a nerve, and furnish pathological explanations of those clinical cases which fall into Class 5, the patients suffering persistent and recurring pain when all nerve trunks have been removed or in a situation where there are no nerves. Another factor in this pain is bone-pain, due to some osteomyelitis in the bone and the result of an upward extension of the inflammation from the terminal scar, or if nerves are irritated for a long time, atrophic, and perhaps inflammatory changes are found in the bones, as was shown long ago by Dr. Mott. It would appear then that there are many causes for non-nerve-trunk pain in amputation stumps, and these may be classified according to their source of origin: bone pain, muscle pain, ligature pain, and joint pains. This class of case is one of the most interesting to study. In the exploitation and unravelling of these cases one speedily becomes aware of the fact that this infective inflammatory wound callus has the greatest tendency to spread along vessels and easily envelops the nerves which may accompany them, and, further, that this condition is made worse by the presence of unabsorbable ligatures, irritation produced by which maintains the inflammation. In several cases of persistent pain in a stump resulting from amputation of the thigh, it has been found that the internal scar has filled up Hunter's canal by spreading up it around the femoral artery, involving in its inflammation, fibrous scar and contraction, the internal saphenous especially, when not cut short. Such cases have only been relieved by excision of the artery, vein and nerve as high up as scar tissue extends. It is not the habit, and I speak as an eight years' teacher of operative surgery, to cut this nerve short or to identify it. This war has shown the pain and misery that the neglect to treat it surgically has caused. The nerve can be found and identified at the time of the amputation. It is very doubtful if it can be found in the mass of scar tissue later. It is better to excise the femoral artery, vein, ligatures, and all the scar tissue round them than to attempt to find the nerve at this late stage.

This nerve has turned up in another condition, and in which it is not nearly so often the cause of the painful stump. In amputations below the knee the internal saphenous nerve is raised in the flap and folded over the stump. It may then be responsible for persistent pain about the level of the posterior margin of the tibia, and is strongly suggestive of the posterior tibial nerve. But skiagraph the case and it will often be found that there is a bony outgrowth from the back of the tibia, the result of bony, periosteal and muscular irritation in that region, which is perhaps rendered more severe along the posterior, as compared with the other, margin of the tibia by the presence of an unabsorbable silk ligature on the posterior tibial artery. The removal of this bony outgrowth and perhaps the end of the bones, if translucent, will often cure the case. The internal saphenous nerve is very hard to find in the surgical fields of this region. Sometimes these patients give a very clear account of the distribution of their pain, which suggests a lesion of this nerve.

There is a third condition under which the internal saphenous nerve, and perhaps the obturator nerve, may make its presence felt clinically. Its regenerating fibres may travel with the femoral artery, pierce the adductor magnus muscle and enter the popliteal space, giving rise clinically to "sciatic symptoms." It may mix with the regenerating fibres of the sciatic nerve, and give rise to nervous symptoms which it is difficult to interpret. I would suggest that in Hunter's canal and the internal saphenous nerve may be found an explanation for the persistence of symptoms after the excision of the sciatic in a case of painful stump after an amputation of thigh. These patients usually complain of painful sensations referred to the inner side of their foot or sometimes to the foot as a whole. The pain keeps them from sleeping well and from wearing an artifical limb. On examination the chief objective sign found is a constant and tender point on the inner side of the stump end. A skiagram usually shows the presence of a bony spur growing from the inner and posterior surface of the femur.

The operation usually performed is as follows. Open the upper end of Hunter's canal, or the lower end of Scarpa's triangle, so as to be well above all scar tissue. The parts are not in correct anatomical position owing to the contraction of the scar tissue and the external rotation of the thigh. Consequently it is usual to open the inner side of Hunter's canal and identify the adductor longus by the direction of its fibres. The sartorius is then identified and leads to the fibrous root of Hunter's canal. On opening the canal it is unusual to find any trace of either the internal saphenous nerve or the nerve to the vastus internus. This latter nerve, owing to the proximity of its fixed end, is less displaced than the more easily movable internal saphenous nerve. Consequently, the nerve to the vastus internus is found on the outer side of the artery whilst the internal saphenous nerve is not seen until the femoral artery and the femoral vein have been divided between the two ligatures; the nerve is on the outer and posterior side of the artery. It is pulled down and divided, swing-door like, with two snips. The distal segments of the artery, vein, and nerve are then excised. The scar tissue in the lower part of Hunter's canal is often most dense and certainly must strangulate the structures severely. After washing out the wound with hot saline, the end of the stump is punctured at its lower and inner part and a drainage tube drawn up into the wound, which is completely closed. No deep stitches are needed as bandages approximate the deeper structures; only skin stitches are used. The drainage tube is removed after forty-eight hours.

A specimen removed from Hunter's canal is cordlike. In its upper part it consists of the femoral artery, vein, and the internal saphenous. About its middle the artery and the vein become impervious and the nerve strangulated. This marks a point just above the situation of the ligature. Below this point a part of the adductor muscle becomes adherent to the muscle which at its lower end is attached to a bony outgrowth of the femur.

Consider the position: the thigh is externally rotated and the slip of muscle attached to the femoral artery, vein, and internal saphenous nerve is an adductor. Every forward movement of the stump must pull on these structures and the patient will have a spasm of pain. A truly painful stump indeed!

One of the most successful amputations of the whole war is a supramalleolar amputation of the foot, a Syme's amputation. With it the patient can walk as well as he ever did and follow his ordinary avocation. Unfortunately the Syme stumps are sometimes tender. The stump is ideal to look at. The explanation of the painful stump is almost always the same—the posterior tibial nerve is strangulated by the large internal scar. The anterior tibial nerve has either been retracted or cut short. The posterior tibial nerve is not so easily found and is rarely cut short. Some of these posterior tibial nerves become strangulated; the majority escape. By the contraction of the fibrous tissue they may be tied to the posterior surface of the tibia or pulled under the newly fastened down tendo Achillis.

The swollen and œdematous strangulated end of the nerve is easily demonstrated. In fact for the demonstration of nerve strangulation I know of no better stump. The nerve lies in the heel flap which was brought over the end of the amputated limb covering it entirely, a long flap carrying a nerve. The retraction after the division of the nerve trunk is well shown, but I have never found trouble with this nerve as it lay in the flap, but higher, about the level of the external skin scar.

Theoretically these nerves should be divided at the level of the skin incision, but practically, as teachers of operative surgery know well, the nerve is re-divided higher up in the freeing of the heel flap. So it escapes lower down as it lies in the heel flap.

The anterior tibial nerve always escapes as its end retracts and is not exposed to the irritation of the healing wounds; and the incision is so situated that the nerve end is wholly above the amputation. The anterior tibial nerve always escapes and the posterior tibial nerve always gets strangulated.

The region in the leg above a Syme's amputation is again the great source of pain and trouble, the posterior tibial nerve easily becoming adherent to the back of the tibia over the end of which it drapes its regenerating fibres and leads to pain. In this situation the nerve bulb is small, trianguloid and flattened, so that hardly any bulb can be felt, owing to the firm bony adhesions. The musculo-spiral nerve in the arm is in a similar position. It easily becomes adherent to the back of the humerus and when it does so the terminal bulb of the divided nerve is replaced by an expanding, and perhaps thickened triangle, the contents of the nerve trunk draping themselves over the end of the divided bone, the middle two-thirds of the humerus in this case. In consequence the main objective sign is a spot of constant tenderness—a bulb cannot be felt.

The sciatic nerve is undoubtedly responsible for the majority of painful nerve troubles after amputation of the thigh. It very easily becomes adherent to the hamstring muscles, particularly the semimembranosus; in consequence it is subjected to pulls with each movement of the stump. It forms a well-developed bulb, which is easily felt and demonstrated. Later, the nerve bulb becomes surrounded by a lymphatic space (comparable to the capsule of Tenon round the eye) in which it can minimize the muscular movements of the hamstrings, like a floating compass. A skiagram should always be taken, and if it reveals the presence of any bony growths from the posterior and inner side of the femur, Hunter's canal should be explored.

A very large percentage of patients, a far larger proportion than in any other type of amputation, arm or leg, who have undergone an amputation of the arm suffer from painful and easily palpable nerve bulbs on the inner side of the arm. It is not very apparent why this should be so. The nerves affected are principally three—the median crossing the artery, the ulnar between the artery and the basilic vein, and the internal cutaneous internal to the vein. These, with the musculo-cutaneous and the lesser internal cutaneous, may be caught with the vessels in the clutches of one single big scar stretching from the end of the amputation stump. The musculo-spiral nerve is so far away that it usually escapes being so caught. Consequently for their removal the nerves must be sought for, identified, and removed. Otherwise absurd mistakes will follow; thus I have had to remove many median and musculo-spiral nerves left by careless or inexperienced operators.

But why is there such a high percentage of painful nerve bulbs in the amputation stumps of the upper arm? The nerves and muscles here are included in one big cicatrix whose lower end is at the amputation, forming a neuro-vascular bundle. This formation is only found here, not elsewhere. It is peculiar to this part, principally I believe for two reasons: first, on the inner side of the upper arm there is a huge lymphatic and blood course, enabling the rapid and extensive formation, and subsequently contraction, of scar; secondly, in this situation the nerve endings are only covered by a thin sensitive skin. Bony formations here are unusual, whilst they are plentiful and a distinctive feature on the posterior and inner part of the thigh.

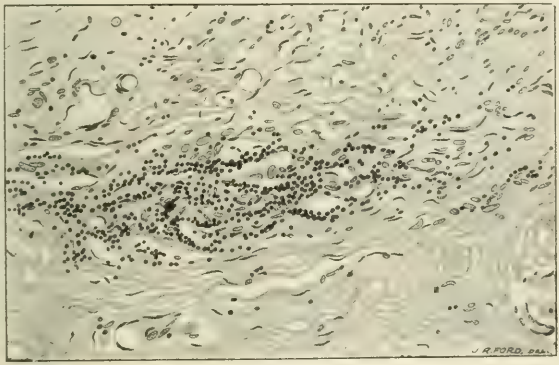

FIG. 4.

E. Wounded, January, 1915. Amputation, day later. Painful stump. Section of ulnar nerve, showing spread out inflammatory nodule in the connective tissue between the nerve bundles. A neuritis which has ascended the nerve.

Bony formations are more indicative of local irritation and nerve lesions than of hypervascularity.

Almost all the amputations through the arm have been done by means of the circular method. In view of the formation of so large a scar involving so many nerves, it is better to convert the circular amputation into a racquet incision, and, through the incision representing the handle of the racquet lying over the vessels and on the inner side of the arm, the nerve can be removed. A great length of nerve can be removed, as the nerves which become painful and bulbous have no branches of importance in the arm. The median nerve, the ulnar nerve and the internal cutaneous nerve can be removed up to the axilla; and the musculo-spiral nerve can be removed as far as its junction with the circumflex nerve. I have seen this modification of the amputation of the upper arm which had been performed by a German surgeon on a British soldier imprisoned in Germany. The amputation was very septic but by his modification the German surgeon had made the operation successful.

Forearm stumps, bearing very little pressure, are very unusually painful. In the neighbourhood of the wrist the median and ulnar nerves would seem to be seldom nipped or inflamed but they regenerate into infective fibrous tissue and into adherent tendons, the regeneration here as elsewhere being a new formation, and invading structures like malignant disease. The pull of the tendons or the inflammation and cicatrization of the scar tissue sometimes makes their excision necessary. The mass of tissue might be compared to a fruit and the nerve to the stalk. Sometimes these are very troublesome cases.

With regard to treatment:—

(1) Tender nerve bulbs should be excised together with a long piece of nerve so as to get above any perineuritis or ascending neuritis there.

(2) The best method of removing a nerve is by means of the epineural sleeve advocated by Major Chapple of the Pavilion Hospital, Brighton, and described in the *British Medical Journal*, August 25, 1917, p. 242. A circular incision is made about 1 in. distal to where the nerve is to be amputated, a cuff of epineural tissue is turned back by means of gauze (doubtless some peripheral nerve fibres are also carried in the cuff), the nerve trunk is divided close to the cuff, which is then drawn forward and its end ligated with catgut. Regeneration is not prevented but the nerve appears later as a pencil, not as a bulb. The difference in shape is due to a less formation of scar tissue, and the advantage is that there is less chance of nerve strangulation by the

scar tissue. The "swinging-door method" is short and easy, leads to a greater formation of fibrous tissue, but practically gives good results. As much nerve as leads to the origin of the next important branch should be removed. This piece of nerve is often long, as the nerve branch comes off above where it leaves the parent trunk. Thus nerves differ from arteries, whose branches come off the parent trunk close to their distribution.

(3) Drainage should always be employed. A good sized tube is used. Its non-use leads to hæmorrhage.

(4) All ligatures and buried sutures should be of absorbable material to minimize sources of irritation.

(5) Injections of alcohol, quinine and urea, &c., should not be used as about three or four-fifths of an injection flows out of the nerve into the surrounding connective tissue, causing later a large formation of scar tissue round the nerve and its subsequent strangulation.

(6) Fixation abscesses are easily produced by tucking gauze or part of a swab into a recess of the wound and leaving it there. They seem to attract a certain amount of the infection to the abscess, which, when the gauze is removed, allows that sepsis to drain away. Perhaps these fixation abscesses are more frequently made unintentionally than intentionally, and are at times most beneficial. The gauze may be left there as long as three or four weeks. They certainly seem to relieve the pain in some cases—infective cases for instance—and are a good counter-irritant.

Another method for relieving pain is the use of an old-fashioned seton of gauze put through the skin of the stump a little above the amputation.

The recommendations that I would make to those surgeons doing the amputations are :—

(1) Let technique be as surgically clean as possible. Much harm results from "lighting a fire" in a wound.

(2) Use only absorbable sutures.

(3) Cut all nerves as short as possible, using the "swing-door method" to close their mouths.

(4) Cut all vessels short, as they form the greatest channel for the spread of infection. By cutting them short "the door is closed."

(5) Drain temporarily all amputation wounds, using a large tube and at least one.

(6) Start the Carrel-Dakin treatment at the time of operation in dirty cases.

(7) In clean cases close the deep wound with a few catgut stitches.

What are the results of attempts to make poor nerve stumps good? The results are not good. They must be improved; for, unless this can be done, re-amputation gives only a slender hope of improvement. The patients frequently complain of pain, and chiefly for three reasons. First, they have fought, been wounded, awaited removal, endured their journey through the dressing stations to the casualty clearing station, have been shorn of a limb, listened to the conversations of friends, fellow-sufferers, nurses, visitors and other frequenters of hospitals, have been compelled to give up their old life and view the prospects of a dismal future, and so on *ad infinitum*. There is hardly a type of patient who shows more plainly the influence of the psychical factor on recovery. The nearer to the trunk the amputation the greater seems to be the psychical depression. I well remember how three men in contiguous beds in the King George Hospital infected each other's mind by their conversations. All three had pains. Not one was improved. Later they were separated, two were operated on again, and all got well. A military ward is bad for these patients. One boy, whose stump began to shake about three weeks after operation in a military ward was cured by a second operation in a civilian ward in which he was next to a cheerful man. Tales of pains and poor prospects render these men unwilling to hurry to get well.

Secondly, these men have often undergone unsuitable operations, leaving them with a generally painful and tender stump. They may have a nerve trunk removed again and again. Some men whom Mr. Muirhead Little has sent me have had twenty and more operations, whilst the real cause of the pain is a ligature which acts like an internal seton, or a bone pain. In point of fact, the surgeons at Roehampton asked for a hospital because others would do one operation when the poor patient only wanted another.

Thirdly, time must be allowed to elapse for stumps to heal and become painless. The stump meanwhile must be used and moved, its muscles made into useful structures, its blood and lymphatic circulations stimulated by heat, massage and movements, and improved by means of electricity and by the early use of a cheap form of artificial leg.

Royal Society of Medicine

EMERGENCY SURGICAL AID CORPS

Annual General Meeting, held at 1, Wimpole Street, W., on Tuesday, November 27th, 1917, at 5.30 p.m.

The President of the Society, Sir Rickman J. Godlee, Bt., K.C.V.O., Chairman of the Corps, in the Chair.

Present: Lieutenant-General Sir Francis Lloyd, K.C.B., C.V.O., D.S.O. (Commanding London District); Surgeon-General Sir William H. Norman, K.C.B., Director-General of Medical Department of the Admiralty; Surgeon-General Sir Thomas Gallwey, K.C.M.G., C.B. (Home Forces), representing the War Office; Sir Edward Henry, K.C.B., G.C.V.O., Commissioner of Metropolitan Police (representing the Home Office); and thirty-five Members of the Corps.

The Chairman called upon the Honorary Secretary, Mr. J. Y. W. MacAlister, to read the Minutes of the Second Annual General Meeting.

The audited Annual Statement of Accounts was submitted by Dr. Moreland McCrea (*see* end of Report).

The Chairman submitted the following—

Report on the Work of the Corps during the Past Year.

During the last year I am sorry to say that I have not been able to take much active part in the work of the Corps, and as I foresaw that this was inevitable I asked to be allowed to resign the Chairmanship of the Committee. But my colleagues seemed to think there was something to be said for keeping the old figure-head even if it was of no great practical use.

The numbers have kept about the same, although there have been many changes. Members go off, but others come on to replace them. The actual members at the present moment are :—

Naval Section	66
Military Section	58
Metropolitan Police Section	104

It says much for the patience and patriotism of the Corps that the numbers should have kept up so well, because, as has often been pointed out, the calls are many, and the majority of them lead to nothing. Still the aspect of things has changed, and the Zeppelins have almost entirely given place to aeroplanes.

The calls come so quickly after the warning is given from the coast, and the machines are so invisible from their small size and the height at which they fly, that it is impossible to locate them with

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searchlights, and, therefore, very difficult to intercept them. The result is that bombs have been dropped in many parts of the town at once, and calls for help have been more numerous.

I will ask one of those who has been on one of these expeditions to tell the meeting what has been done. I know that they now wear steel helmets, a most wise precaution, as even we who sit at home are bombarded with fragments of our own shells in a most unpleasant manner.

The number of calls in the year was twenty-nine, the number of times our men went to the scene of action seven.

I think the way in which we are likely to be of most use in the future, as the staffs of the London hospitals become gradually more and more depleted and more and more exhausted, is to supply teams of one surgeon, one assistant surgeon, and one anæsthetist to any hospital that telephones to say that such help would be acceptable. I know that the arrangement exists, but I think the hospitals might perhaps be reminded that this is the case from time to time. Such a matter is so easily forgotten.

Of course there have been complaints. They are wholesome reminders that we must take stock of our position. One was with regard to apparatus. I believe that everything is now satisfactory in that respect, and we are certainly better provided with light than we were; for we have a good supply of acetylene lamps which should be of much help in first-aid, and perhaps even operations, in the pitch darkness that often results from interference with the light supply.

Complaints of there being nothing to do have my sympathy, but they do not move me; it is the lot of the majority of those who indulge in active warfare. We do not have, or we do not want to have, the very hard times that alternate with the period of repose of our fighting warriors. For though the line has reached our very doors it is after all a mild and miserable fighting that goes on in the "fortress of London."

Some of the complaints have been directed towards the way in which the members are called up. This has led to the Executive Committee thinking out a new plan, the details of which I do not understand, but which I am assured has given much satisfaction. The actual process is for the convener, on receiving a warning, instantly to get together one team, and to warn the remainder of the members on duty to be ready to proceed at once, either to the place where they are wanted—streets or hospital as may be indicated—or to come here as may be required.

Our accounts, which have been carefully prepared and audited, are still in rather an anæmic condition, and I hope members of the Corps will endeavour to induce their friends who cannot give their services to give their "silver and gold, or paper." Of course a large proportion of the deficit is on account of surgical supplies kindly entrusted to us by Messrs. Allen and Hanburys on "sale or return," and it is to be hoped that we shall be able to return the bulk of them; but still it is a debit, and in the event of a large amount of serious work having to be done we should have to pay for these things; but even if these items are taken out of the account you will see that financial help is needed if the work of the organization is to be carried on as efficiently as in the past.

We have repeatedly been urged to go on with our work by the Naval, Military, and Police Authorities, and I trust that the members of the Corps will still continue to devote themselves to this laborious and rather dull task.

LIEUTENANT-GENERAL SIR FRANCIS LLOYD.

MR. CHAIRMAN AND GENTLEMEN,—I have a few words to say, but these few words are, as far as I can possibly make them, of very strong encouragement to this organization. It has been said by our Chairman that waiting is dull work. Well, gentlemen, the Army waited from after the South African War till the day this War came, wondering whether the country would acknowledge their Army at all, and very dull work it was, but fortunately for the country the Army took the business seriously, and those seven divisions to which a decoration has been awarded by the King to-day were probably trained better than any Army in the world has ever been. That was the result of dull work, and if you are having dull work waiting now, I am not at all sure that the day may not come, like a thief in the night, much more quickly than you expect, when your services will be required up to the very hilt.

Up to this moment we have had some very terrible air raids here in London, but the organization of Sir Edward Henry and the police, the organization of the medical service under General Sir Thomas Gallwey and that of the D.D.M.S. of the London District, acting under the Home Forces, have been sufficient to cope with it ; but if we have a really great air raid, as the Germans have promised us, and as possibly that last Zeppelin raid (when the bomb dropped in Piccadilly) would have been but for unforeseen conditions, we shall want every man that we can possibly get to succour the wounded. That is the way I look at it.

But if we have nothing behind what we are at present employing, what will happen if there is a great day of fighting in this country? It is the unexpected that happens in war, as we have been told lately by the greatest authority in this country, and as we have read year in and year out that invasion is always a possibility (I do not say a probability, for we are, thank God, well protected by our Fleet), but there is always a chance of a last throw by the Germans, and there is always the chance of a last successful throw, even if it be only a temporary success. If this were to happen there will be terribly heavy fighting, and although the regular medical service has proved itself equal to every occasion so far, still there may come a moment here when we shall want everything we can put our hands upon.

There is also in my mind always the possibility at any moment of undesirable characters raising troubles in parts of London, troubles which may lead to difficulties and possibly to fighting—that is a possibility. I am quite certain that the police, assisted by the force under myself, will be able to cope with it, but it might mean great loss of life and consequently a great number of wounded, and an organization of this sort under those circumstances would be of the greatest possible use, and indeed prove to be an absolute necessity. Although, therefore, it may be dull work, I do offer the very greatest possible encouragement and thanks from the Army in general for the work that your Corps are doing.

There is one point that has been alluded to, that is your men getting here during a barrage. I certainly think that the early call is a most necessary thing, for I can tell you this, that the barrage is likely to be heavier in the future. We are increasing every means of coping with the raids, and as you know, a heavier barrage means more splinters falling. I had some through my own windows, and, therefore, if it gets heavier and heavier, the sooner you get your men here the better it is.

Well, gentlemen, I do commend most heartily the services which you have given, and the services you may be called upon to give. Preparation is everything, as our German enemies have shown, and I urge upon you to organize even more than you have done. If the day does come yours will be a very great reward in the satisfaction of knowing you have prepared for and performed a great public service.

SURGEON-GENERAL SIR WILLIAM NORMAN.

GENTLEMEN,—I have very little to add to Sir Francis Lloyd's commendation of your work, but I can assure you it is much appreciated in the Navy, and it has been a source of great relief to me to know that such a competent body of medical men is ready at any moment to proceed to any place in case of need. Of course we are waiting too, and I hope we shall wait on till the end, but if the need does come at any time I am sure you will all come up to the scratch, and we shall be most grateful for your help. So far the number of wounded from our actions has been small, as most of them have gone down in their ships. But we must remember that up to the present battleship has not met battleship, and so we really do not know what will happen or what number of wounded we may get at any time or where they will be, and that of course makes it very difficult for us to make special preparation at any particular place, and so you may be called to help us at any time. The only serious event which happened lately, and which you will all no doubt remember, took place at an area where there was a large staff on the spot, but if the latter had not been available we should have been obliged to call for your help. So I am afraid it is dull waiting, as Sir Francis Lloyd says; but the longer we wait the better for the country. I have to thank you in the name of the Navy for what you have done, and all I am sure you will do, and for the time spent and the trouble you have taken.

SURGEON-GENERAL SIR THOMAS GALLWEY.

I can add very little to what Sir Francis Lloyd and Surgeon-General Norman have said, but as the War progresses our reserve resources are lessening a little bit, and I think we can congratulate the Members of this Corps upon being the first to organize themselves for this work.

You have lately seen a great deal in the papers about the man-power of this country in the speeches by Sir Auckland Geddes, and you will notice as we go on that this work of organization is becoming each day more difficult, but I think also more perfect. The medical profession being a scientific profession have anticipated a good deal of this, and your Corps are an example of a wise anticipation of the country's needs. As I told you last year, the waste in the Army is going on, and with it the waste in the medical profession. And more and more we feel the need of your organization, and I want you to understand that we are relying more than ever on your help when the need arises.

As regards my own particular branch, I may inform you that in case of raid or invasion the Emergency Surgical Aid Corps now forms an integral part of our scheme. It is now a part of our scheme for home defence, and particulars are being printed. It will rest with the General Headquarters at the Horse Guards where we shall ask you to go in times of emergency. We do not know where any blow will be inflicted upon us, but if that blow is inflicted we are now in the position that we can rely upon your Surgical Aid Corps to give us skilled experts to go to any dangerous point, and it will be of the greatest assistance to our Forces organized for defensive purposes. The various units will now know when they receive these printed particulars (as I think we have only told them verbally up to the present, as a great deal of the work is secret) that your Corps is going to be part of the scheme. I cannot thank you sufficiently for organizing this Corps and keeping it going.

SIR EDWARD HENRY.

I must apologize for being late, but my car came to grief, so I had to walk. I think it was at the beginning of January this year when I had the pleasure of meeting you here, and I took the opportunity of urging you to maintain your organization, because, as I pointed out, one never could tell when the need for your services might arrive. Well, since then we have had eleven air raids, I think. We had a raid in May, one in June, one in July, five in September, and three in October. But the June raid was really a serious affair—the raid that occurred on June 13th—because about 160 odd people were killed or subsequently died, and about 430 to 440 were injured. It is a heavy casualty bill, and as one raid in the daylight can produce results like that I leave it to you to visualize for yourselves what might be the state of affairs if the enemy determined to raid us in much greater strength than hitherto. On all ordinary occasions surgeons at the various hospitals are probably sufficient in number to deal with the casualties, but, as Sir Francis Lloyd pointed out, nothing but the unexpected happens in war, and you cannot rely merely upon what you have experienced as the sole guide in determining what may occur. The Germans may decide that they will make several strong attacks on London in order to terrorize us. Some of their machines might get through, and they might do a great deal of damage and cause many casualties. If that should happen there will be plenty of work for this Corps, and I am sure that everyone would feel that in maintaining this Corps, and in placing their services at the disposal of the authorities, you are obeying the call of duty and doing national work of the greatest importance.

I have alluded to the figures of casualties in the raid on June 13th because I think there is an inference to be drawn from these. We had as many casualties on that one occasion as on the previous ten occasions, the reason being that we had not learnt how necessary it is to take cover. We have learnt that now and we have provided a great deal of cover for those who want to take it.

We have got bomb-proof cover for 300,000 or 400,000 persons, and splinter proof shelters for about 1,000,000 more; and, therefore, I think you will agree the accommodation is on a scale quite sufficient for all persons likely to be in the streets on the "take-cover" warning being given, as it is on a scale which will provide a sufficient margin for those persons who will leave their houses in order to take refuge. Now most of the public shelters are only splinter proof, and one's own house is splinter proof, and there can be no reason or sense in leaving your own house to seek shelter elsewhere. Very few buildings in London (and I am sure the authorities will tell you the same) are proof against a direct hit. These bombs are terrible engines of destruction—they sheer through everything. What we are trying to do is to persuade householders to remain in their own houses, where they are just as safe as in public shelters, for it is in seeking the public shelters they will be exposed to most danger.

Well, gentlemen, the present situation is, that in response to a public demand we have provided shelters on a very extensive scale. When one talks about providing shelter for 1,000,000 people it means a great deal of spade work, and assistance in regulating the crowds admitted to them. That we have done, and have provided in addition bomb-proof shelter for from 300,000 to 400,000 persons. So now if people will only heed the warning which is given them, and take cover immediately, we hope that even if the Germans do attack us in greater force than in the past, the casualty list will not be so very heavy. What we feel satisfied about is that should there be unhappily another serious raid there is sufficient surgical aid to cope with the cases, for we have now perfected our relief parties, ambulances, and nursing organization, and so far on every occasion those who have been wounded have been taken to hospital with great promptitude.

So, gentlemen, you may be assured that your Emergency Corps have contributed to the success of our arrangements. You have done your share, and we are very grateful to you for all the assistance you have given us.

DR. MCCREA

MR. PRESIDENT AND GENTLEMEN,—I have very little to add to the very lucid report of the President. I have had the experience of doing practical work in connexion with this Corps, and it proved to me that we must keep together and ready for action, for I can see that occasion may arise when this Corps will be of great use. If the bomb which was dropped in Piccadilly on the last occasion had dropped twenty minutes sooner the whole of this Corps—both rotas—would have been required.

As regards work done, I have been out on two or three occasions; on one, I think we had about six cases, and it is satisfactory to know that we found we were properly equipped and had everything that would be wanted in the case of a big thing. There were no complaints by any of the parties that had been out. We had altogether twenty-nine calls in the year, and we sent out seven parties. One party, which I do not know, should certainly get the V.C. They were finally driven into the cellars of a club owing to the heavy barrage and later they came back here ready to perform their duties should another bomb drop.

At Seven Sisters Road, we were unfortunately late in arriving, owing to some telephone difficulties, and we only got the call half an hour after Scotland Yard wanted us, but we were of some use, and I think this was the first occasion that the acetylene lamps were used and they proved a great success. The only complaint against them is made by those men who have to stay here waiting to be called out, and who object to the smell when the lamps are brought back.

About the team system, as the President was telling you, we have cut it down to a really fine point, because of the alteration in the nature of the raids. Before, when a Zeppelin came, he did give us warning, and we had plenty of time to get ready; but aeroplanes come along before we know where we are.

But the authorities have altered things considerably for us, and we get the warning now from the coast, in fact we get three warnings—viz., we get the coast warning, which sometimes gives us time to call the conveners, and one party, and to warn the remainder of the rota they may be wanted. Then we have one second warning—viz., "Take air raid action."

Formerly we only got the "take action" call, and the men arrived here in the middle of the barrage. Now we get the early call which enables the men to hold themselves in readiness, and to have one party here ready for action. If more are required, we can send them on direct from their homes, or bring them here as seems best, but we have now arranged to send cars for the later parties to convey them whenever they are wanted. There was a good deal of grumbling about the calling up under the old system, and having to come through the barrage. The curious thing is that, notwithstanding the complaints, the first night the new system was instituted we had more men here than at any raid before, so evidently the complaint was one that should not have been taken too much to heart. As regards funds, I think we are very well off, notwithstanding Mr. MacAlister's remarks about a deficit. A successful show was never run with a credit balance and I think we are to be complimented on it.

Mr. ERNEST CLARKE: We are greatly indebted, sir, to these gentlemen, beside you, who have spared time from their grave and responsible duties to come here and encourage us as they have done; and I think we can tell them, now we are assured by their presence here and by their admirable addresses, that they appreciate our effort, that not a single member of our Corps is likely to drop out unless called to still more important duties. Permit me to move—

> That the cordial thanks of the Emergency Surgical Aid Corps of the Royal Society of Medicine be offered to Sir Francis Lloyd, Surgeon-General Sir W. H. Norman, Surgeon-General Sir Thomas Gallwey, and Sir Edward Henry, for their presence here, and for the addresses they have delivered, and that with their permission their speeches be published in our Report.

Mr. MCADAM ECCLES: I rise to second the resolution of thanks to Surgeon-General Sir W. H. Norman, the representative of our first line of defence; Surgeon-General Sir Thomas Gallwey, who stands behind him on the shore; Sir Francis Lloyd, to whom our metropolis owes more than she can ever repay; and to Sir Edward Henry, who watches and cares for every street and alley. Their words of commendation have cheered us, and will make us steadfast in our work until the end of the War makes it no longer necessary.

The resolution was carried by acclamation, and Sir FRANCIS LLOYD briefly acknowledged it.

Executive Committee.—The following were unanimously re-elected: Sir Rickman J. Godlee (Chairman), F. F. Burghard, A. H. Cheatle, Ernest Clarke, W. H. Clayton-Greene, Edred Moss Corner, W. McAdam Eccles, G. J. Jenkins, J. P. Lockhart-Mummery, H. Moreland McCrea, M. S. Mayou, J. W. Thomson Walker, Andrew Wylie, J. Y. W. MacAlister (Hon. Secretary).

Royal Automobile Club.—Mr. MAYOU moved—

> That the best thanks of the Corps be conveyed to the Royal Automobile Club for its generous help in supplying a fleet of motor cars upon every occasion upon which the Corps has been called up.

This was seconded by Mr. WYLIE and carried unanimously.

Dr. MORELAND MCCREA: I would like, sir, to move, with your permission—

> That a cordial vote of thanks be offered to Miss Williamson and her staff for the able assistance rendered to the Corps in the office of the Society.

Without that assistance it would be impossible to carry on our work, which involves a heavy amount of clerical work and correspondence. It involves daily revising of lists, correction of rotas to keep them up to date, and arrangements for telephoning, and many other things.

Mr. MACALISTER (Honorary Secretary to the Corps): I think it is my right, sir, as well as pleasant duty, very cordially to second Dr. Moreland McCrea's proposal, for not even he knows as well as I do the amount of work that falls upon Miss Williamson and her staff in connexion with the Corps work. When I look through the list of subscriptions to the expense of the Corps I see, in my mind's eye, the biggest subscription of all opposite her name, representing the value of the work done by her since the formation of the Corps.

The work done by these ladies, and it is well and cheerfully done, is really a fine bit of patriotic work.

The CHAIRMAN briefly supported the motion, and it was carried by acclamation.

Mr. MCADAM ECCLES: I rise to propose a vote of thanks to the Hon. Secretary, Mr. MacAlister, and in doing so no one can gainsay that his initiative, fostering care, and great energy have not only brought about the existence of the Emergency Surgical Aid Corps, but have fixed it on a firm foundation, and raised it to that position of which we have had evidence from the excellent addresses which have been delivered this afternoon. So long as we have him as our Hon. Secretary to guide us through what are at times troubled waters, difficult fronts, and dangerous roads, it is unlikely that anything will go amiss.

Mr. ERNEST CLARKE: I have special pleasure in seconding Mr. McAdam Eccles's motion, for it is only those behind the scenes who know the work that Mr. MacAlister has done. It is work that no one else could do, for he is more than our Secretary, he is the "moving spirit."

The motion was carried unanimously.

On the motion of Mr. SWINFORD EDWARDS a vote of thanks to the Chairman, Sir Rickman Godlee, for presiding was carried by acclamation and the meeting adjourned.

THE ROYAL SOCIETY OF MEDICINE.

EMERGENCY SURGICAL AID CORPS.

Dr. INCOME AND EXPENDITURE ACCOUNT FROM 22ND JANUARY, 1915, TO 27TH NOVEMBER, 1917. **Cr.**

RECEIPTS.	£	s.	d.
Donations (for list see over)	262	18	2
Balance (being excess of Expenditure over Income)	3	0	1
	£265	18	3

[N.B.—We have obtained from Messrs. Allen and Hanburys Surgical Appliances to the value of £77 11s., on "Sale or Return." If these are used or deteriorate, our deficit balance will be increased by that amount.]

PAYMENTS.	£	s.	d.
Postages and Telegrams	20	18	9
Wages of Telephone Attendants . ..	183	8	10
National Health Insurance Stamps for Telephone Attendants	3	4	6
Miscellaneous Disbursements	12	9	7
Printing and Stationery	8	0	6
Mouth Gags and Forceps	4	9	1
Field Dressings	8	14	6
Lamps and Carbide	3	11	9
Brassards and Gas Masks	16	13	3
Tourniquets	1	10	0
Travelling Expenses	2	17	6
	£265	18	3
Balance brought down	£3	0	1

27th November, 1917.

Audited and found correct,
A. J. FOSTER, F.C.A.

List of Donors to the Emergency Surgical Aid Corps Fund.

	£	s.	d.
Dr. Peter H. Abercrombie	12	12	0
Dr. H. Russell Andrews	2	2	0
Mr. R. C. Bailey	3	3	0
Mr. Aslett Baldwin	2	2	0
Mr. C. A. Ballance, M.V.O., A.M.S.	2	2	0
Mr. Harold Barwell	4	2	0
Mr. F. D. Bennett	2	0	0
Dr. Norman G. Bennett	1	1	0
Mr. H. Blakeway	1	1	0
Dr. J. Charlton Briscoe	2	2	0
Dr. Dudley W. Buxton	1	1	0
Dr. N. Whitelaw Bourns	1	1	0
Sir Lauder Brunton, Bart. (the late)	2	2	0
Lt.-Col. E. Farquhar Buzzard (R.A.M.C., T.)	3	3	0
Dr. G. Cathcart	4	4	0
Sir Francis Champneys, Bart.	2	2	0
Mr. Arthur H. Cheatle	5	0	0
Mr. Charles C. Choyce	2	2	0
Mr. Ernest Clarke	7	7	0
Mr. J. Jackson Clarke	1	1	0
Mr. W. H. Clayton-Greene	8	8	0
Mr. E. Treacher Collins	2	2	0
Mr. J. F. Colyer	3	3	0
Mr. V. Zachary Cope	2	2	0
Mr. Edred M. Corner	6	6	0
Mr. F. Richardson Cross	2	2	0
Mr. Alban Doran	2	2	0
Dr. S. Ernest Dore	2	2	0
Dr. John J. Douglas	1	1	0
Dr. E. Vincent Dunkley	1	1	0
Dr. Frank E. Easton	2	0	0
Major W. McAdam Eccles (R.A.M.C., T.)	5	5	0
Mr. Percy Edgelow	2	0	0
Sir Frederic Eve (the late)	2	2	0
Lt.-Col. C. H. Fagge	2	2	0
Dr. John Fawcett	2	2	0
Captain J. Herbert Fisher (R.A.M.C., T.)	2	2	0
Dr. Edward Furber	2	2	0
Mr. P. L. Giuseppi	1	1	0
Mr. Bryden Glendining	1	1	0
Sir Rickman Godlee, Bart.	7	7	0
Dr. Albert A. Gray	1	1	0
Mr. H. Tyrrell Gray	5	5	0
Dr. W. S. A. Griffith	2	2	0
Dr. de Havilland Hall	2	2	0
Dr. K. R. Hay	1	1	0
Dr. E. W. Hedley	2	2	0
Mr. Harvey Hilliard	1	1	0
Dr. Eardley Holland	2	2	0
Mr. W. W. James	2	0	0
Mr. Thomas H. Kellock	2	2	0
Dr. D. Barty King	2	2	0
Dr. A. B. Kingsford	1	1	0
Dr. J. T. C. Laing	2	2	0
Mr. T. W. Lambert	1	1	0
Mr. W. Lang	2	2	0
Mr. John B. Lawford	5	0	0
Mr. Arnold Lawson	2	2	0
Dr. J. Howell Lloyd	1	0	0
Dr. G. B. Longstaff	2	0	0
Mr. J. Y. W. MacAlister	5	0	0
Dr. H. Moreland McCrea	2	2	0
Mr. L. H. McGavin	2	2	0
Dr. Alastair MacGregor	2	2	0
Mr. John D. Malcolm	6	6	0
Mr. M. S. Mayou	3	3	0
Mr. W. M. Mollison	2	2	0
Mr. J. P. Lockhart Mummery	5	0	0
Dr. Vivian B. Orr	2	2	0
Dr. H. M. Page	2	0	0
Mr. J. Herbert Parsons	5	0	0
Lieut.-Col. D'Arcy Power (R.A.M.C., T.)	2	2	0
Dr. May Rathbone	2	2	0
Mr. Charles Ryall	5	5	0
Mr. H. Betham Robinson	2	2	0
Dr. G. Drummond Robinson	2	2	0
Mr. G. Winfield Roll	1	1	0
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Section of Anæsthetics.

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(*November* 2, 1917.)

The Administration of Anæsthetics in Home Military Hospitals.

By J. F. W. SILK, M.D., Lieutenant-Colonel R.A.M.C.

FROM the beginning of August, 1915, to the end of July, 1916, I occupied the position of "Consulting Anæsthetist" to the Malta Command, that is, during the period of the Gallipoli activities, when the greater part of the beds that the island contained were occupied by surgical cases. For the last fourteen months I have been in a similar position at home, and in that capacity I have visited all, or nearly all, the principal military surgical hospitals in England. In Malta, of course, all the "deaths under anæsthetics" were notified to me, and since the middle of January of this year I have received detailed reports of all the fatalities in England, Scotland, and Ireland.

Most of you will recollect, I expect, that in the pre-war days I always deprecated the endeavour to use any one anæsthetic, or any one method in every case, and insisted upon the advantages of varying one's procedure in accordance with certain elemental circumstances such as sex, age, physical condition, and nature of the operation. While my views upon this point remain unchanged, I appreciate the fact that the relative values of these respective factors have altered materially under the stress of war. Questions of age and sex have practically disappeared, and the main determining factors are those of the previous condition and the nature of the operation, and even these have very much altered.

First of all with regard to the factor of the previous condition, it seems to me that except for the secondary results of his injury, the soldier may be described as an average healthy male. But these secondary results are of much importance, and introduce factors which are comparatively speaking unknown in civilian practice. For instance, with the acute shock of the battle-field we have but little to do in the home hospitals, but, on the other hand, very many of the men are suffering from a condition of "chronic shock" which renders them rather troublesome from the anæsthetist's point of view. Then again, many of the soldiers have been the subject of a somewhat prolonged suppuration of varying intensity. This may not perhaps produce all or any of the characteristic objective symptoms of acute sepsis, but I am sure that it has a very bad effect upon the heart muscle. It is rather more than a mere coincidence that of forty post-mortems at home the heart muscle was found more or less degenerate in twenty-six, and of these twenty-six there were definite records of prolonged sepsis in twenty. A somewhat similar degeneration, too, results from malarial poisoning, to which the troops are exposed upon some of the fronts. In Malta the question of the effects of malaria upon the heart muscle and its relation to death under anæsthetics was the subject of an exhaustive inquiry by the authorities in that island.

We do not, nowadays, hear very much about the so-called "soldier's heart," which was the subject of so much controversy about the time of the Crimean War and after. I am myself inclined to believe, however, that the dilated heart of over-training and under-feeding is of fairly frequent occurrence, and when by a rare chance such a patient comes under the anæsthetist, trouble is very likely to arise.

As regards the nature of the operation, the differences entailed by war conditions are so obvious that one need do no more than enumerate them. Intracranial injuries are usually treated at the base hospitals abroad, and we see but few at home. At any rate, I know that while I was in Malta such operations were of almost daily occurrence, but I do not think that I have seen a single one since I have been doing home work. Injuries involving the pleuræ are relatively numerous, severe, and complicated. Abdominal surgery, except in point of view of numbers, does not seem to me to differ from similar cases in civil life. There are, however, at least three classes of case in which the work of the anæsthetist has been most distinctly altered and severely tried. I allude to spinal injuries, orthopædic surgery in general, and plastic operations upon the face.

Looking at the whole subject from a broad general point of view, I should be inclined to say that in military surgery the exceptional cases call more insistently for variations in methods of anæsthetization than in civil life. But, on the other hand, the number of normal or straightforward cases is relatively larger. This means of course that our first consideration is the selection of an anæsthetic and method which may be employed almost as a matter of routine.

In deciding this question there are two very important points which must not be lost sight of. The first is the personality of the anæsthetist. The additional military and war hospitals scattered throughout the country are to be numbered by hundreds, and the difficulty of supplying them with skilled or even efficient anæsthetists is very great. The patriotism and willingness with which medical men and women have come forward to staff these hospitals are quite above any praises of mine, but I am sure that none of them will mind my saying that the task of administering anæsthetics has not always been one with which they have been most pleased or for which they have considered themselves the best fitted.

The second point is that the large number of military hospitals taken in conjunction with the frequent and necessary changes in the anæsthetists who work them, renders it very desirable to standardize the equipment provided, and to render that equipment as simple as is compatible with efficiency. The standard at present is :—

(1) Nitrous oxide apparatus.
(2) Schimmelbusch's frame.
(3) Clover's inhaler.
(4) Junker's bottle (Rigby's safety pattern is now issued).
(5) Tongue forceps.
(6) Mouth gag.

One or more of each is provided in accordance with the size of the hospital and the number of operating theatres in use. For special hospitals, and where special cases are treated by special anæsthetists, special instruments are obtainable. To my mind the War Office is quite generous in this respect, but naturally the authorities do not wish to lend themselves to the exploitation of cranks and instrument makers.

On the whole, therefore, one is bound to select as a routine anæsthetic a method which is the least liable to abuse, and in the use of which the anæsthetist, even if rather below the average, is least likely to go wrong. Amid conflicting opinions the task is not so easy as it seems. There is one negative point, however, upon which I hope my opinion may be largely supported—i.e., that in military surgery at home the routine use of pure, unadulterated, or as our American colleagues call it, "straight" chloroform, as a first choice is to be avoided as much as possible. Briefly stated, my chief reasons are the following:—

(1) In the hands of the most skilled the mortality is relatively high. I believe that it is something approaching 1 in 2,000. In military work I do not know exactly what the proportion may be, but I will give you some figures: Of the deaths under anæsthetics which have been reported to me since January, 1917, at least 55 per cent., and probably more, were due to or occurred under chloroform used in an undiluted form, and in many other cases the chloroform was by far the predominating drug. One may admit, of course, that in many of these cases the use of chloroform was almost imperative, and that in many the condition of the patient was such, that in all probability he would have died under any other anæsthetic, but such admissions do not really affect the figures. I would further point out that of that 55 per cent., in fully 28 per cent. death occurred before any operation had commenced.

(2) In the course of my wanderings, I have been much struck by the fact that of the most earnest advocates of pure chloroform, only a comparatively small number really know how to give the drug properly. As an example of this, let me instance the gentleman whom I found pouring drachm doses of chloroform upon a lint-covered Schimmelbusch's mask, over which, and his retaining hand, he had thrown another sheet of lint about the size of a pocket handkerchief. From the same hospital I received complaint of the impurity of the chloroform which, I was told, gave rise to a great deal of lividity.

Although I object to the use of undiluted chloroform as a first choice, for routine work, it must not be supposed that my condemnation extends further than this. I fully recognize the value of this drug for many cases, and I would even say that when sufficiently diluted down with ether in the shape of one or other of the mixtures, and administered carefully by the open method, it forms a very simple and admirable method of inducing anæsthesia previous to the continuous use of ether.

For quite short cases some form of local anæsthetic is, I believe, often employed, but these naturally do not come under my ken, and

I have no means of knowing exactly to what extent this method is really employed. I am inclined to think, however, that it is not employed nearly to the extent that its most earnest advocates could wish, and for this I have no doubt that the surgeons could give sufficient reason.

For similar short cases an alternative is nitrous oxide. This is not used nearly so often as I should like to see it used. Two difficulties arise with regard to it. First of all the supply is somewhat scanty and irregular, and in the second place the average anæsthetist in the military hospitals rather fights shy of it.

Failing local drugs and nitrous oxide, the anæsthetic of my choice for continuous work is ether, and with certain reservations and precautions I prefer the open method. The details of the particular method that I advocate do not, I expect, differ very materially from those employed by most of you, but in order to make myself quite clear, I will briefly refer to the points upon which I like to insist :—

(1) A preliminary injection of morphine $\frac{1}{4}$ gr., and atropine $\frac{1}{100}$ gr. For military work I think that this should be given rather less than half an hour before the operation. In my opinion the orthodox one hour is too long. See, too, that the *patient* gets the full dose, not the bedclothes.

(2) A small pad of Gamgee tissue with a hole in it round the nose and mouth.

(3) A Schimmelbusch's mask covered with from twelve to twenty layers of gauze. This may be clipped to the pad by means of tongue forceps, and a useful hinge is thus formed.

(4) Although when time is no object and the surgeon is good natured, one can start with ether from the very beginning, I think that it is better in most cases partly to induce with a mixture, or even with a Clover's inhaler. In many cases I employ chloride of ethyl on the open mask in the manner suggested by Dr. Barton.

(5) As a drop-bottle I recommend the metal flask devised by my friend, Dr. McCardie, of Birmingham. This enables one partially to regulate the flow of ether, and is therefore economical; it also possesses the great advantage of being practically unbreakable.

(6) The late Sir Frederic Hewitt introduced a very useful open airway tube passed into the mouth over the base of the tongue. I prefer to pass a smaller and softer tube about 5 in. down the nose, a device which I have found most useful, and now employ in almost every case.

This is the plan that I have advocated at most of the military hospitals which I have visited, and I believe that it is very widely adopted. The equipment of all military and war hospitals includes one or more Clover's inhalers, and at some of the hospitals I have visited I find that the anæsthetist is rather wedded to that instrument and will not succumb to my blandishments in favour of the open method. Under these circumstances I generally suggest to him to try the plan proposed by Dr. McCardie of putting 1 dr. of chloroform into each 2 oz. charge of ether. I have myself had much success with this method, and believe it to be capable of extension in various directions, for instance, I am inclined to think that the same degree of dilution might be used by the open method all through, and would to some extent meet the objection so often raised as to the large quantity of ether required in long cases. In this connexion I may perhaps say that from a large number of figures I have obtained I estimate that the quantity of ether required for open administration works out at 6 to 8 oz. per hour (in Malta it was 8 to 10 oz.). Whichever plan has been adopted, I am very certain that less pure chloroform and more ether is being given in the home hospitals now than was the case twelve months ago. I think, too, that I can point to some definitely good results which have followed the change, for the death-rate for the quarter ending September 30 has been rather less than one half of the average rate for the two quarters preceding.

I now come to the question of the anæsthetic to be used in the various special cases to which I have alluded. In the cases of modified or chronic shock to which I have referred I doubt whether one can give too much morphia, but in cases of sepsis, malaria and other heart trouble, I would avoid the use of morphia. In other respects I would base my anæsthetic treatment of all these cases upon three fundamental lines, viz. :—

(1) Avoid all but the most dilute doses of chloroform.

(2) Give as little anæsthetic of any sort as possible.

(3) Avoid anything like asphyxia, by diluting the vapour very freely. For which purpose oxygen given either by itself or bubbled through your anæsthetizing agent is very useful.

It is in such cases as these that the use of the continuous "gas and oxygen" methods of the American surgeons seems likely to be of considerable value. Others who are here to-night will probably speak to you more in detail as to the particular advantages of this method. All I will say at present about it is that the present forms of apparatus seem to me to militate considerably against its general employment, nor am I quite convinced as to the quality of the anæsthesia produced.

In discussing the question of a routine anæsthetic I purposely avoided any allusion to the warm ether method, mainly because Dr. Shipway will probably speak upon that method, but partly because it does not appear to be used to any great extent at home, partly, also, because the method seems to me to lack that element of simplicity which I believe to be so essential when dealing with a large number of administrators of varying skill and capacity. I think, however, that in the class of cases with which I am now dealing—i.e., chronic shock and sepsis, warm ether would very likely be of considerable advantage. In intracranial cases, morphia is usually unnecessary, as the patient is often in a semi-comatose or dazed condition. For the same reason, too, very little anæsthetic is required. In these cases I prefer chloroform because it is desirable to limit the venous congestion as much as possible. I usually administer it by means of a Junker's bottle, through which I pass a very small stream of oxygen, leading the mixture of chloroform vapour and oxygen down the nose by means of a soft tube.

The mortality arising from operations of the empyema type is rather heavy. Most of the cases dealt with in the home hospitals are of course of the chronic type, and have been the subject of prolonged and severe suppuration. They are, therefore, particularly bad subjects for anæsthetics of any sort. There is no doubt that these cases ought all to have some form of artificial respiration as is typified in the intratracheal method. Unfortunately, however, my experience is that neither the anæsthetist nor the surgeon seem always to grasp the seriousness of the problem which is before them when they decide to operate. The intratracheal apparatus is so cumbersome, costly, and difficult to manipulate that but very few establishments are equipped with it. Not only so, but the anæsthetists are constantly changing, so that even when one man has learnt to use it the probabilities are that he gives place to another in the course of a few weeks who does not know how to use the machine. I can only think that these cases ought to be concentrated in a few of the better equipped hospitals where this method is available. Failing this, chloroform and oxygen should be given very gently from a Junker throughout the operation.

In cases of spinal injury the position is often complicated by the fact that the patient is turned wholly or partly upon his face: this not only entails compression of the chest but obliges one to pump the anæsthetic into him. Then again, avoidance of venous congestion is desirable. Taking all these points into consideration I am inclined to give these patients oxygen and mixture from a Junker's bottle. But these are cases in which the care and vigilance of the anæsthetist are more important than the actual method employed.

One would imagine that orthopædic surgery was the special field for the use of the intraspinal methods, but I am surprised to find that, as far as my experience goes, this is not the case; nor is any explanation forthcoming. Nitrous oxide is frequently used, and it requires a good deal of skill in the manipulation of the administration to hit off the precise moment at which the fullest possible degree of relaxation is obtainable. The average orthopædic case does well enough with ether, but it must be ether given without a trace of asphyxia so that there is as little congestion of the veins as possible. At one of the orthopædic hospitals visited I received complaints about the amount of venous congestion, which in this instance was attributed to the use of warm ether, and unless the warmth of the ether vapour is very carefully regulated it does seem to produce considerable venous congestion.

The question of the anæsthetic in plastic face surgery and allied conditions deserves a whole evening to itself. It is one in which I have taken a great deal of personal interest, and one of the most difficult questions with which we have to deal. The chief troubles arise partly from the presence of large quantities of blood in the mouth and pharynx, partly from the necessity of getting out of the way of the operator, and partly also from the difficulty in manipulating the anæsthetic when the jaw is wholly or partly fixed, or even perhaps unduly mobile. There is no operation in which the anæsthetist can be of so much real assistance to the surgeon, or where the value of a good anæsthetic is so conspicuous. For this reason I do not favour the view sometimes taken that one particular method should alone be employed. Not only do the cases vary very much in themselves, but one should take into account the capabilities of the anæsthetist. In some few instances the ordinary routine methods are possible, but these are rather in the minority. In most cases exceptional methods will have to be employed. Of these exceptional methods I recognize four varieties.

(1) The intratracheal method. I place this first on the list, for just now it is very much in fashion. I acknowledge that in some cases it is almost indispensable, but I do *not* admit that it is necessary or even advisable in all or even in the majority of cases. Time, however, will not permit me to give my reasons for this opinion.

(2) Allied to this plan is what I would call the intubation method,

by the means of a Kühn's tube. This method is little known in this country and I believe that it has many advantages, for the whole of the pharynx can be fully packed and the blood and mucus are then absorbed.

(3) Another plan which I also think is not as much practised as it should be, is the rectal oil-ether method of Gwathmey. I have used it myself many times and seen it used by others, and always with satisfaction and advantage. It is probably the simplest known method of giving an anæsthetic.

(4) The fourth plan—the method of position—is one I naturally esteem highly, as I am mainly responsible for its use in this type of case; but for that very reason I am not disposed to say much about it now. Anæsthesia is induced with the patient sitting bolt upright on the table, any routine plan being adopted. The anæsthesia is maintained by means of a nose-tube conveying oxygen and chloroform vapour; or the vapour of mixture or ether may be used. This plan has been sharply criticized, but at least I can say of it that it has been in use now for over a year at one of the principal "facial" hospitals in the kingdom, and that no fatalities have attended its use.

Whichever plan be employed, it may be supplemented by the use of some form of suction apparatus on the principle of similar tubes used by dental surgeons, and with the same object—viz., the removal of blood and mucus from the buccal cavity.

Before concluding what I have to say on the methods of anæsthetization there are, however, two minor points to which I should like to draw your attention. The first of these is the occurrence of the so-called ether bronchitis. In the pre-war days one was very much inclined to put this down off-hand to simple exposure, either in draughty passages or in the wards themselves. In my visits to hospitals of very varied type throughout the country, however, I have come across cases of bronchitis under such different conditions and surroundings that I am not at all sure that the theory of "exposure" will hold good. In some places it almost appears to be endemic: in others it partakes of the nature of an epidemic; while in other places again where it ought to be rampant it hardly ever occurs. The second point is the question of records. In the original army scheme the only provision made for recording the work of the anæsthetist was in the shape of one, or at most two, rather narrow columns in the "operation book." In Malta I drew up a form for this purpose, and from it I obtained some valuable information, the hospitals being all within reach and the operations not over numerous. A somewhat similar form is now in use too, in most of the English Commands. But I am not satisfied with the results; the figures do not appear so reliable. But at any rate I think that they have served one useful purpose—viz., that of increasing the interest of the men in their anæsthetic work. Something of the sort is very desirable, and I should be glad to have your views upon the subject.

DISCUSSION.

Mr. BELLAMY GARDNER: The introduction of a nasal tube to act as an artificial airway in cases of partial or complete obstruction to respiration is quite new, and I wish to draw attention to this invention, for which we owe Colonel Silk our thanks, as an addition to our resources in these emergencies. In certain military hospitals to which I am attached the quality of the chloroform has been extremely bad. I hope Colonel Silk may be able to remedy this defect.

Dr. J. BLOMFIELD: I have learnt to appreciate the intratracheal method in plastic surgery.

Mr. PERCIVAL P. COLE: I can only deal with the question from the surgeon's standpoint, and particularly from that of a surgeon engaged in the treatment of jaw and face injuries. Colonel Silk has put forward a plea for uniformity, and has suggested that it is necessary to adjust administration of anæsthetics to the calibre of the anæsthetists employed in routine military surgery. I should have thought it better to adapt the calibre of the anæsthetist to the cases with which he has to deal—to retain, and make a point of retaining the highly skilled administrator to cope with the most difficult cases. Colonel Silk has suggested that the Kühn's tube is a good working substitute for the intratracheal method. It is certainly an improvement on the ordinary methods of administration, but has distinct disadvantages when compared with insufflation by the intratracheal tube. First, Kühn's tube is bulky, and in intra-oral operation impedes the operator. Secondly, the long tube is the only channel whereby air can enter. The effort of breathing through this for the space perhaps of two hours is a physical strain that cannot be neglected. Colonel Silk kindly undertook one such case for me. For myself, as operator, nothing could have been more perfect. The patient's condition, however, for three or four hours after the operation was such as to cause considerable anxiety. Kuhn's tube has no conceivable advantage over the intratracheal method except that it is easier to pass. The intratracheal method, if uniformity be necessary. is a method which fulfills all requirements in every case, and the mere question of technical difficulty should not be allowed to weigh against its undoubted superiority. The number of cases in which the tube cannot be passed is very small, and if it cannot be passed at the first administration, steps, necessary in the course of treatment, can be taken to render its passage possible on the next occasion. Oil-ether *per rectum* has a definite claim to consideration. Although Colonel Silk has demonstrated the utility of the method, I should not, however, care to adventure it except in extra-buccal operations, for the depth of anæsthesia apparently cannot be varied and controlled, and therefore bleeding would hamper the action and distract the attention of the surgeon.

Captain H. EDMUND G. BOYLE: It is both wrong and dangerous to induce with chloroform; a mixture of ether, or of gas and ether should always be used. I do not agree that the apparatus militates against the gas and oxygen method. In respect of plastic surgery, I have done twenty-three cases with oil-ether *per rectum* and have tried to change the dosage. I now use paraldehyde 1 dr., olive oil 2 oz., and ether 4 oz. I have not had any cases of irritation of the rectum. The mouth prop should always be used, and I consider there is no need for the nasal tube.

Major H. D. GILLIES: By Colonel Silk's advice I have adopted the sitting position, and also intratracheal methods. In plastic surgery an anæsthetist is needed who can give every known form of anæsthesia. It is often impossible to use the intratracheal method owing to the form of injury. The sitting position has the advantage that the tongue and jaw tend to fall forward. Nitrous oxide and oxygen have given good results. Each case has to be considered from the anæsthetic point of view. The intratracheal is the ideal method if it can be used. Kuhn's tube is a good substitute, but the large apparatus in the mouth is a disadvantage. Chloroform and oxygen given through the nasal tube, with the patient in the sitting position, have yielded good results.

Dr. A. H. COOK: Since Colonel Silk's visit to the Hampstead Military Hospital I have used a rubber tube, pointed bluntly at one end, and passed through the nasal cavity into the pharynx, in over 100 cases, and have found it of great service. Whereas, before using these, I had very frequently to resort to the tongue forceps, or stitch, and had occasionally to contend with severe spasm, or trismus, due, doubtless, to the irritation caused by the cheap anæsthetics of war time, necessitating the use of the screw-gag or Mason's. With the help of the nasal tube the tongue forceps are scarcely ever necessary. I have used a rubber tube two sizes larger than that recommended by Colonel Silk, several of these being sterilized by the orderly before the operations begin, kept in a glass tube to avoid sepsis, and touched with vaseline before introduction. Only on one occasion was there bleeding which gave some trouble. I should like also to ask Colonel Silk if he has met with cases of general tremor or "dithering," which I have occasionally seen, due, perhaps, to partial recovery from the anæsthesia during the operation, and which have proved very embarrassing to the operator: and what is the best treatment for that condition.

Dr. F. E. **Shipway**: I agree with Colonel Silk that chloroform should be condemned as a routine anæsthetic in military work. Ethyl chloride, on the open mask, or C.E. should be used for induction, to be followed by ether—preferably warm ether. As this is not always to be obtained in military hospitals open ether can be used. In shock, morphia must not be given freely; morphia is a depressant, it diminishes nervous excitability, lowers body temperature, reduces blood-pressure, and diminishes the secretions. Thus it produces conditions similar to those existing in shock. Not more than $\frac{1}{6}$ gr., or $\frac{1}{4}$ gr. at the outside, should be given. Intratracheal insufflation of ether should not be used in empyema. Deep anæsthesia is usually necessary for passing the catheter, and this should be avoided. I prefer warm ether or C.E., and always give oxygen. In plastic surgery intratracheal ether is the best method. These cases are frequently very difficult for the anæsthetist, owing to malunion and contraction, and sometimes fixation of the tongue, and there is no method which gives such good results; but the difficulty of passing the catheter is sometimes great. I have found this impossible in six cases out of over 100 cases, so that the percentage is under 6. This is a small number. Vomiting is rare, a point of considerable importance to a patient who may have to undergo many operations in succession; some patients have had as many as twenty. If they have not been sick they face the next operation with much less trepidation, and take the anæsthetic better. Sickness is a disadvantage of the sitting-up chloroform method. It is a little difficult to defend this method, and at the same time to condemn chloroform as a routine. Ether-oil is valuable occasionally, but I am not quite sure that it is free from danger of irritation of the lower bowel. Mr. Page says that a two to one mixture is safe, and prefers it on this account to Gwathmey's 75 per cent. mixture, but I believe that Major Gillies has found that the strength that Gwathmey recommends is necessary to keep his patients quiet.

Dr. A. G. **Levy**: Further evidence is afforded, if further evidence is needed, that the routine injection of atropine before operation does not prevent death under chloroform. I have reason to believe that atropine will even foster cardiac syncope under chloroform.

Lieutenant-Colonel **Silk** (in reply): I am very pleased to find that the meeting is so thoroughly in accord with my condemnation of undiluted chloroform for routine use in military hospitals. I have some difficulty in dealing with the death record, as it is almost impossible to draw just conclusions from the figures until I am able to obtain an approximate idea of the total number of administrations. At present I am engaged in an endeavour to obtain these figures. I am glad to have the independent opinion of Dr. Cook, of the Hampstead Military Hospital as to the value of the nasal air-tube, and to find that the suggestion meets with the approval of Mr. Bellamy Gardner and others. I adhere to my opinion that for shock cases as seen in the home hospitals morphia is required. With regard to the use of intratracheal methods in empyema, I refer of course to cases of the chronic type, and not to acute cases. My first case of rectal oil-ether was done. I think, about four years ago, and since then I have employed that method in suitable cases many times, both in civilian work and in the military hospitals in Malta and at home. I have never seen any rectal irritation following its use, but of course it requires care not only in the selection of cases, but particularly in the direction of washing out the rectum at the end of the operation. This should be done by the anæsthetist himself with a double tube, and should not be left to the nurse or the orderly. The patient should be put back to bed on the left side. I quite agree with Captain Boyle and others, that the difficulties of introducing the intratracheal catheter are sometimes very considerable, and as Major Gillies has pointed out, these difficulties are sometimes insuperable, so that it is always as well in these cases to have a second string to one's bow.

(*November* 2, 1917.)

Apparatus for Administering Oxygen with Ether and Chloroform.

Demonstrated by HUGH R. PHILLIPS, M.D.

THE apparatus consists of an oxygen cylinder connected with a water tank made of copper, as used by Boyle in his intratracheal apparatus; this is connected by a tube with a three-bottle apparatus, as used in the American autogenor apparatus. The Boyle's tank is three-quarters filled with boiling water, and the three tubes contain respectively chloroform, ether, and plain water. These are connected with a supply tube by means of taps, and so by means of oxygen bubbling through all the tubes one can give oxygen-chloroform, oxygen-ether, oxygen-chloroform-ether, or pure oxygen. In cold weather these three tubes are surrounded by two small hot-water bottles. The supply tube is connected with a mask covered with a single layer of flannel, or a nasal or oral tube, or a mouth prop with tube attached, or with an intratracheal catheter. For intratracheal work a mercury gauge is used.

I have used this apparatus for over two years in nearly 1,100 cases of all kinds, and it seems to have the following advantages :—

(1) Simplicity.

(2) Economy, as one uses only about 3 dr. of chloroform, or 2 to 3 oz. of ether, and 50 to 60 gal. of oxygen an hour.

(3) The patient invariably keeps a good colour, provided there is a good air-way.

(4) In about 70 per cent. of cases there is no vomiting.

The chief disadvantage is its bulk.

Section of Anæsthetics.

President—Mr. George Rowell, F.R.C.S.

Some Considerations on the Physiology of Anæsthesia by Chloroform.[1]

By George A. Buckmaster, M.D.

It is not the purpose of this paper to formulate a general theory of narcosis. Many attempts in this direction have been made, among others, by Max Verworn and Ralph Lillie. In recent papers Winterstein considers that a process of adsorption takes place between the narcotic substance and the cell-proteins or cell-colloids. This causes a reversible decrease in the permeability of the cell-surfaces for water and soluble cell-constituents, producing a diminution of the excitability which is associated with the conditions existing in the normal permeability of cells. The experimental facts are then re-stated as an explanation, or rather, they are put forward as a theory of narcosis.

It is known that the most varied types of living animal and vegetable cells respond in an identical manner to the action of chloroform, a drug which is seven to eight times as toxic towards protoplasm as ether. Like many toxic substances, exceedingly minute doses of chloroform act as stimuli, thus Lemoine and Leblanc have shown that the time of inflorescence of the lilac and azalea is quickened by chloroform vapour. Both individual cells and organisms differ in the degree of their re-action to anæsthetics, but, given a sufficient concentration of the drug, the final result is death. Paul Bert in 1883, and more recently B. J. Collingwood, carried out experiments which proved that dogs and other animals are killed by amounts of chloroform which are incapable of producing anæsthesia—the whole metabolism of the body is profoundly affected, the temperature steadily falls, and death occurs. The following table is constructed from experiments carried out on dogs by Paul Bert.

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[1] At a meeting of the Section, held May 7, 1918.

Percentage of chloroform vapour in air	Results	Hours from commencement of administration till death
0·8	No anæsthesia	9 to 10
1·2	Diminished sensibility	6 to 7
1·6	Anæsthesia slowly induced	4
2·0	Anæsthesia in a few minutes	2 to 3
2·4	,, ,,	1·7
3·0	,, ,,	0·66
4·0	,, ,,	0·5
6·0	,, ,,	0·005

When a low percentage is inhaled, an enormous quantity of chloroform can pass into and out of the body without producing any obvious effect except a fall of temperature. In this respect its action closely resembles alcohol, which was demonstrated by Tarchanoff to impede growth and check the development of the excitable sensori-motor regions of the cerebral cortex. Certain experiments were carried out for a period of five months on kittens of the same litter and of equal vigour. Some of the animals formed a control while the others were anæsthetized twice a day up to the stage where the corneal reflexes disappeared; these showed a definite retardation of growth which was estimated by weighing. Subsequent to this treatment the animals exhibited increased metabolic activity, and their rate of growth during this period more than compensated for the retardation of growth during anæsthetization (Schapiro).

Since in the higher animals arterial blood is the vehicle by which chloroform is taken up from the surrounding medium, and a definite solution pressure of the drug must be attained before anæsthesia occurs, the relations which may exist between blood and chloroform have been the subject of much investigation. The proteins of the blood, both those of plasma and the red corpuscles, have been proved to form definite unstable compounds or physical aggregations with chloroform, and, since analyses of the blood gases during chloroform anæsthesia, made by J. A. Gardner and myself, showed that during narcosis arterial blood becomes progressively richer in reduced hæmoglobin, it is probable that the transport of oxygen by the red corpuscles is interfered with by the presence of chloroform. Evidence for the relations which may exist between blood and chloroform was first definitely given by Benjamin Moore and Roaf, though numerous observers, such as Pohl, Waller, Wells and Vernon Harcourt, among others, had recognized with what tenacity chloroform is held by the blood, and how difficult it is to recover the drug from the blood. In Waller's experiments only 0·014 grm. of chloroform could be recovered out of 0·108 grm. which the blood contained. We shall subsequently show, however, that, provided the extraction of the blood gases in chloroform anæsthesia is carried out in a very perfect vacuum, the entire quantity of the drug can be recovered as vapour with the evacuated blood gases.

It is well known that of the three gases which exist in blood the amounts of oxygen and carbon dioxide taken up are not proportional to the pressures exerted by these gases; in other words, their absorption does not follow the Dalton-Henry law. The question whether chloroform is or is not taken up by blood according to this law has never been experimentally tested. Since in any given mixture of air and chloroform there exists a definite chloroform vapour pressure, the value of this in air and chloroform at the anæsthetizing value of 2 per cent. is about 8 to 10 mm. of mercury, and this was the value observed by Paul Bert. The evidence given by the experiments of Moore and Roaf, who measured the vapour pressures of chloroform in water, serum, and hæmoglobin solutions of equal strength to the percentage of this pigment in the blood, indicates that the curves of pressures and concentrations in the case of serum and hæmoglobin are such that chloroform cannot be absorbed according to the Dalton-Henry law, but that, like oxygen and carbon dioxide, the drug is associated with some substance, probably the blood-proteins. However, this fact is not easy to determine except at vapour pressures of chloroform which were much in excess of the vapour pressure of a mixture of air and chloroform at the anæsthetizing value. Unlike blood, the absorption of chloroform by water or solutions of sodium chloride does, as might have been expected, follow Henry's law, so that when the experimental results are expressed graphically the absorption curve is a straight line.

The experimental work of J. Addyman Gardner and myself was carried out in 1905-12. This was undertaken subsequent to the researches which are incorporated in the Report of the Special Chloroform Committee of the British Medical Association. Our work exactly corresponds in time to the numerous publications of Maurice Nicloux in the *Comptes rendus*.

None of our experiments are in accord with the view that chloroform is decomposed in the blood. That this should occur is extremely improbable. However Nicloux considers that as much as 50 per cent. of the quantity inhaled is decomposed within the body. Theories have been advanced that the anæsthetic state is due to the abstraction of oxygen from the blood by chloroform. But, since the actual amount of chloroform in the blood can be accurately measured, the amount of oxygen which might be abstracted in this way can be calculated, and the amount could produce no symptoms at all, except in so far as the oxidation products might produce narcosis. Of this there is no experimental evidence.

The destiny of chloroform in the blood is to be almost entirely exhaled. If any amount of it is oxidized in the tissues this might explain the increased output of chlorides in the urine after chloroform narcosis; but these are increased, though not to the same extent, after anæsthesia by ether. Some observers, Wagener, Thiem and Fischer, state that chloroform as such can be recognized in the urine. This amount therefore must be unoxidized. In our opinion chloroform remains undecomposed within the body.

We have repeated the work of Desgrez and Nicloux. These observers found a combustible gas in the blood of normal dogs. This was augmented in chloroform narcosis; as much as 6·9 c.c. of carbon monoxide was found in 1,000 c.c. of blood, and considered to arise from the decomposition of chloroform within the body. We have never found any carbon monoxide in the blood of normal or anæsthetized animals, though the chloroform when present is easily obtained as vapour by the blood-pump. As much as 4·1 c.c. chloroform vapour at 0° and 760° pressure, equal to 41·9 mgr. of chloroform, is frequently found. This vapour mixed with the blood gases *yields* carbon monoxide during the analysis of the gases with potash according to the following equation:—

$$2\,KHO + CHCl_3 = 2\,KCl + H_2O + HCl + CO.$$

In the experiments of Desgrez and Nicloux the blood gases and chloroform vapour were passed *first* through a U-tube containing potash and then analysed for carbon monoxide by the method of Rabourdin, in which solid iodic anhydride at 150° C. is decomposed with the liberation of iodine by carbon monoxide. In their experiments we are convinced that the carbon monoxide found to be present is actually manufactured by their method of procedure. In no single case have we found a trace of carbon monoxide, though this gas is easily recognizable in much smaller quantities than what is stated to be present as the result of a decomposition of chloroform within the body.

Our determinations of the anæsthetic and lethal quantities of chloroform in narcosis were carried out on cats and dogs. The experiments in all reach several hundreds and the results can only be fairly criticized when full details of the experiments are given, but for obvious reasons it is not possible to do so in this paper. It is sufficient to say that the initial experimental procedure was done under ether or nitrous oxide. The animals were kept warm at the normal temperature. One important and essential detail must be given. We realized, what is in fact true for a very large number of physiological experiments on the circulation and respiration, that the dorsal position of an animal is a highly abnormal one; in our experiments the animal was frequently changed from side to side. We are satisfied that an experiment may give absolutely fallacious results if this precaution is not taken. The blood corpuscles of cats easily sink in plasma, and there is a large area of the lung which is completely choked with a sediment of corpuscles if the dorsal position is long maintained. Such an animal respires with a diminished lung surface which may be estimated at a quarter to even a half of the normal.

In all our experiments we never lost a single animal by accident. This result we attribute to the precautions taken, and to the fact that the administration of the anæsthetic was continuous throughout at a definitely known percentage, though at the time we were unacquainted with the views held by A. Goodman Levy on this matter.

The amount of chloroform in the blood was estimated by the method of Carius, which is the one generally employed for the determination of a halogen in organic compounds. The method yields results of great accuracy. The destiny of the inhaled chloroform is traced by its chlorine. The blood during anæsthesia therefore contains an excess of this element. When ether is used it was found in numerous control experiments that the percentage of chlorine in the blood remained sufficiently constant throughout a prolonged experiment. Therefore our method of research, an entirely new one, possesses the required accuracy.

The Anæsthetic and Lethal Quantities of Chloroform in the Blood.—When the different methods which have been used to determine the lethal dose of chloroform are considered, there is fair uniformity in the results. We took two points in the narcotic state: loss of conjunctival reflexes, this is the anæsthetic quantity, and the convulsive respirations, which is the lethal amount. Our conclusions are given in the following table, showing milligrams of chloroform in 100 c.c. of blood :—

Observer	Animal	Anæsthetic amount of $CHCl_3$	Lethal amount of $CHCl_3$
Gréhant and Quinquaud, 1883	Dogs	50 mg.	—
Pohl, 1890	,,	18-50 ,,	35 mg.
Nicloux, 1906	,,	50 ,,	41-70 ,,
Tissot, 1906	,,	34-40 ,,	60-105 ,,
J. Mansion and J. Tissot, 1906	,,	29 ,, 32-43 ,,	— —
Buckmaster and Gardner, 1906	,,	16-31 ,, (reflexes just gone)	61-69 ,,
Buckmaster and Gardner, 1906	Cats	14-27·5 mg. (reflexes just gone)	40 ,,

From these figures, which are in fair agreement, especially for the lethal dose, it would seem that the quantity of chloroform required to produce various stages of anæsthesia are somewhat higher in dogs than in cats. In this series of experiments we noticed that the body-weight of an animal is without influence on the percentage of chloroform in the blood necessary to produce anæsthesia. Also an examination of the curves in our paper shows that the rate of induction of anæsthesia varies for different individuals. Lastly, a narrow margin exists between the quantity of chloroform in blood during complete surgical anæsthesia, and that required to abolish respiration. In all our experiments, without exception, the cessation of respiration preceded cessation of the heart-beat. The death was an asphyxial process.

The Rate of Assumption of Chloroform by the Blood.—To investigate this question ten or twelve small samples of blood were removed from an artery during the progress of anæsthesia. From the data of the chloroform content curves were constructed. In all of these the same general features were seen. In the initial stages of anæsthesia at a time when the individual is conscious, the chloroform content of the blood rises with great rapidity to a value which approaches a maximum. During this period, which occupies the first few minutes, the first danger point in anæsthesia occurs, because the quantity of chloroform in the blood directly or indirectly affects the respiratory nerve-centres of the brain. In consequence of the respirations being slower and shallower, the amount of chloroform in the blood falls, but the fall is also due to the exit of the drug from the blood into the tissue-cells of the body. When the animal has passed this first stage, the amount of chloroform again quickly rises towards a maximum value, and an equilibrium between the factors which determine the amount of chloroform in the blood appears to be obtained, the processes of intake and output of the anæsthetic at the surface of the lung going on side by side. In other words, a state of equilibrium is reached, which persists for a considerable period, and throughout this period the difference between the amount of chloroform present in the blood and what is found at the lethal point is very minute. The state is far from one of safety, and the animal may suddenly die at any moment should any disturbing factors come into play. The whole of this period is a danger period. In the experiments quoted the percentage of inspired chloroform was maintained constant, but it is probable that the contention of those anæsthetists who have insisted that after anæsthesia has been established this can be and should be maintained with a much smaller amount of chloroform vapour, rests upon a sound basis.

The Rate at which Chloroform is eliminated from the Blood after Anæsthesia.—These experiments fall into several groups. In some, samples of arterial blood were taken; in others, of venous blood. The most interesting were those in which samples of arterial and venous blood were taken simultaneously. The main conclusion which can be drawn from these experiments is that the rate of elimination of the drug from the body, viâ the blood, depends upon the physiological state of the individual animal. The rate of loss is at first comparatively rapid, and subsequently becomes slower. But the initial rates of elimination are much less rapid than the initial rates of the intake of chloroform, and, on the whole, elimination is a much slower process than assumption, a view which is supported not only by the actual determinations of chloroform in the blood but by a comparison of the times at which various reflexes disappear and reappear. From our curves the initial falls are not quite so rapid as the work of other observers had led us to suppose. The chloroform-content of the blood was only reduced by 50 per cent. in fifteen to twenty minutes; three-quarters of the chloroform was eliminated in about half an hour. These statements hold good when the animal is breathing naturally, but, as might have been expected, the extent to which the lung is being ventilated during any period is the chief, if not the only, factor which determines the rate of elimination. We may mention than an animal may walk about after anæsthetization with as much as 10 mgr. of chloroform in 100 c.c. of blood. It appears in all respects as a normal animal.

Function of the Red Corpuscles in Anæsthesia.—It was originally suggested by Schmiedeberg that the red corpuscles were the chief vehicle in the transport of chloroform from the lungs. This view was confirmed by Pohl's experiments in 1890. Nicloux, in 1906, used centrifugalized oxalated blood removed from a vein, and recovered 64·4 per cent. of the chloroform in blood from the red corpuscles and 13·3 per cent. from the plasma. His figures for the distribution of chloroform among the constituents of the blood are much below those of Pohl, who had stated that 87 to 90 per cent. of the total chloroform was held by the red corpuscles; in other words, the chloroform-holding power of the corpuscles is seven to eight times that of the plasma. Our experiments in connexion with this question proved to be exceptionally difficult. Our conclusions, that chloroform is very firmly held by the blood of an anæsthetized animal, are in accord with all other observers. But it was also found that chloroform primarily associates itself with the red corpuscles and does not enter the plasma to any marked extent unless the anæsthesia is pushed to an extreme degree, or a chloroform and air mixture is administered with a high percentage of the drug. At the period when the reflexes disappeared the percentage of chloroform in the red corpuscles was found to be 64·3 per cent.; in other cases 71·1 per cent., and after the inhalation of 2 per cent. chloroform for three-quarters of an hour no less than 98·5 per cent. of the anæsthetic was held by the red corpuscles.

In order to obtain additional proof that the red corpuscles, as would seem to be the case from the above experiments, are the essential vehicles for the carriage of chloroform, we argued that if the view was correct that the transport of chloroform was a function of the red corpuscles, then, though the absolute amount of chloroform present in the blood might be modified by abstracting or adding blood from or to an animal, the *percentage of chloroform* ought to remain constant.

The general plan of the experiments was to anæsthetize an animal with ether or nitrous oxide, allow the anæsthetic to be disengaged from the body, and then administer a known percentage of chloroform. The determination of the amount of the drug in the blood was made at the asphyxial point. In one hour or an hour and a half all the anæsthetic was eliminated, and during this period a measured amount of blood was abstracted and the experiment repeated. In other experiments the amount of chloroform present was determined in the same animal before hæmorrhage, after hæmorrhage, and after the replacement of the blood which was furnished from another animal in such quantity as to augment the original volume of the blood by one-third to one-half.

The general results of all the experiments will now be detailed.

	AVERAGE AMOUNT OF $CHCl_3$ PER 100 GRAMMES OF BLOOD		
		Before bleeding	After bleeding
Experiments in which the asphyxial state was rapidly reached, in about 3 to 9 min.		0·043 mg.	0·045 mg.
Experiments in which the asphyxial state occurred in about 30 to 80 min.		0·048 ,,	0·051 ,,
		After hæmorrhage	After replacement of the blood
Experiments made at the asphyxial point		0·0421 mg.	0·0426 mg.
		0·0768 ,,	0·0768 ,,
	Before hæmorrhage	After hæmorrhage	After replacement of the blood
Experiments made at the asphyxial point	0·061 mg.	0·059 mg.	0·049 mg.

From the above it would appear that the conclusion is justified that the percentage of chloroform in the blood does not undergo any variation corresponding to differences in the volume of the circulating blood, and this is what was to have been expected if the red corpuscles were the essential agents for the transport of chloroform. It may be considered in the highest degree probable that the anæsthetic is associated with the proteins of the corpuscle, but the experiments gave no clue as to whether it is the hæmoglobin or other cell-proteins which actually pick up the drug.

Composition of the Blood Gases in Chloroform Anæsthesia.—Little attention has been paid to the changes in composition of the blood gases in anæsthesia, in spite of the well-known fact that, as the various stages of anæsthesia by chloroform which are clinically recognized succeed one another, the aspect of arterial blood to the naked eye becomes progressively venous in character. This change in colour must be due either to an altered ratio of oxyhæmoglobin to reduced hæmoglobin, or to changes in form of the red corpuscles, which in consequence reflect different amounts of light to the eye, as the observations of Harless in 1846 showed to be the case when blood outside the body was saturated with oxygen or with carbon dioxide gas. In our experiments a specially constructed blood-gas pump was used. This was made without taps, and a very perfect vacuum obtained by employing a side-chamber containing cocoa-nut charcoal, which was surrounded with liquid air. A number of experiments on the gas-content of the blood of normal cats served as a control. In the evacuation of the blood gases of an anæsthetized animal the chloroform can be recovered as vapour with an efficient pump, and its amount determined. A few examples may be given of arterial blood :—

Percentage of $CHCl_3$ inhaled	Time in minutes of duration of anæsthesia	Total gas	$CHCl_3$ in cubic centimetres of vapour	CO_2	O	N
2	60	53·99	5·95	36·79	9·90	1·34
2	55	56·69	5·27	41·66	8·58	1·16
2	100	58·48	6·51	39·73	10·89	1·38

The composition of normal cat's blood compared with that of other animals in different stages of anæsthesia : (1) when the reflexes re-appear after anæsthesia ; (2) when the reflexes just disappear ; (3) during the anæsthetic state ; is given in the following table :—

	Average volume in cubic centimetres per 100 c.c. of blood			Average composition percentage of gas			Ratio of O : CO_2
	CO_2	O	N	CO_2	O	N	
Normal cats	25·07	13·60	1·00	63·20	34·28	2·52	1 : 1·84
Reflexes first reappear	29·02	11·49	1·33	65·06	25·44	2·87	1 : 2·55
Reflexes first disappear	29·57	7·78	2·15	69·14	18·17	5·09	1 : 3·80
Anæsthesia	36·00	8·14	1·49	71·27	16·12	2·95	1 : 4·40

We found that in slightly anæsthetized animals, and in those completely anæsthetized, that the gas-content of the blood was augmented by an amount of 10·2 and 26·2 per cent. respectively. This increase is almost entirely due to an augmented amount of carbon dioxide gas. But apart from this, the darkened colour of blood in chloroform narcosis is due to an actual diminution in the amount of oxyhæmoglobin. The fall in the oxygen content of the blood is well marked, even at the point where the reflexes disappear. During the anæsthetic state the reduction is 40 per cent. The hæmoglobin is therefore only partially saturated with oxygen during narcosis, indeed to the extent of 60 per cent. The blood of the normal cat, on the assumption that the alveolar air contains 14 per cent. of oxygen, should be saturated to the extent of 90 per cent., the oxygen tension being equal to 99·49 mm. Hg. But during narcosis the hæmoglobin is in the same state of partial saturation that it would be with an oxygen tension of only 45·5 mm. Hg. It is known that chloroform is associated with the red corpuscles, and outside the body aggregations of hæmoglobin and chloroform have been described by Moore and Roaf, and by Edie. Some of these are said to possess distinctive spectra. It is therefore probable that the fall in oxygen content of the blood in chloroform narcosis is due to direct interference with the oxygen-transporting property of the red corpuscles.

Ventilation of the Lung during Chloroform Narcosis.—This subject stands in close relation to the experiments which have just been described. Based on experiments made by J. Tissot, the view is expressed in the Report of the Chloroform Committee of the British Medical Association in 1910, that the blood retains unimpaired up to the time of death its normal capacity by absorbing oxygen, and that if the amount of this gas diminishes in the blood, the decrease is solely due to the slowing of the respiration. From our experiments we were unable to agree with this view. As a matter of fact no single experiment had ever been made on the ventilation of the lungs. The ordinary methods of recording respiratory movements are quite useless for this purpose.

We have used a plethysmograph similar in principle to that employed by Haldane and Boycott in their studies of the lung ventilation in man. The ventilation of the lung was investigated during narcosis with chloroform and during narcosis with ether.

With unimpeded respiration under anæsthesia by chloroform, we found the ventilation of the lung takes place at a lowered level. The total exchange of gases between the animal and the atmosphere is diminished in amount and this continues throughout the whole period of anæsthesia. During narcosis the lung-ventilation is diminished in the first three minutes by an amount of from 30 to 80 per cent.; the average is almost 60 per cent. of its original value. This lowered level of respiration, as our blood-gases analyses have shown, results in a marked diminution of oxygen in the blood. Whether this is due to the chloroform or the diminution of respiration was decided by plethysmographic records taken under ether. It was found that a change in the ventilation of the lung took place, which fell from 1,058 c.c. per minute to 657 c.c. This is a diminution of 62 per cent., but it had no effect whatever on the oxygen content of the blood. The experimental evidence therefore enables us to conclude that the diminution of oxygen-content in the blood in chloroform narcosis is dependent upon the action of the drug upon the red corpuscles. These, therefore, are carriers for oxygen and carriers of chloroform vapour. Recently, evidence has been given that these also play an essential part in the transport of carbon dioxide from the tissue-cells of the body.

LIST OF PAPERS CONTAINING THE EXPERIMENTAL WORK ON WHICH THE ABOVE SUMMARY IS BASED.

BUCKMASTER, G. A., and GARDNER, J. A.

"The Anæsthetic and Lethal Quantities of Chloroform in the Blood of Animals," *Proc. Roy. Soc. Lond.*, 1906, B, lxxviii, pp. 414-454.

"The Estimation of Chloroform in the Blood of Anæsthetized Animals," *Proc. Roy. Soc. Lond.*, 1907, B, lxxix, pp. 309-315.

"The Rate of Assumption of Chloroform by the Blood during Anæsthesia," *Proc. Roy. Soc. Lond.*, 1907, B, lxxix, pp. 555-565.

"Function of the Red Corpuscles in Chloroform Anæsthesia," *Proc. Roy. Soc. Lond.*, 1907, B, lxxix, pp. 566-579.

"On the Rate of Elimination of Chloroform from the Blood after Anæsthesia," *Proc. Roy. Soc. Lond.*, 1907, B, lxxix, pp. 579-589.

"On the Supposed Presence of Carbon Monoxide in Normal Blood and in the Blood of Animals anæsthetized with Chloroform," *Proc. Roy. Soc. Lond.*, 1909, B, lxxxi, pp. 515-528.

"A New Form of Tapless Blood Gas Pump," *Journ. of Physiol.*, 1910, xl, pp. 373-380.

"The Gases of the Arterial and Venous Blood of the Cat," *Journ. of Physiol.*, 1910, lxi, pp. 60-63.

"The Composition of the Gases of the Blood in Chloroform Anæsthesia," *Journ. of Physiol.*, 1910, xli, pp. 246-262.

"Composition of the Blood Gases during the Respiration of Oxygen," *Proc. Roy. Soc. Lond.*, 1912, B, lxxv, pp. 56-64.

"Ventilation of the Lung during Chloroform Narcosis," *Proc. Roy. Soc. Lond.*, 1911, B, lxxiv, pp. 347-372.

"The Nitrogen Content of Blood," *Journ. of Physiol.*, 1912. xliii, pp. 401-416.

BUCKMASTER, G. A.

"The Relations of Carbon Dioxide in the Blood," *Journ. of Physiol.*, 1917.

"The Capacity of Blood and Hæmoglobin to unite with Carbon Dioxide," *Journ. of Physiol.*, 1917.

DISCUSSION.

Captain C. T. W. Hirsch: I should like to have heard from Dr. Buckmaster a plea for the greater use of chloroform as an anæsthetic. The one point which struck me was his observation that oxygen given with that anæsthetic did not improve the percentage of oxygen in the blood. I have given considerably over 8,000 chloroform anæsthetics at the Royal Herbert, King George, and Military Orthopædic Hospitals. In all it was given with the percentage inhaler I showed at a meeting here some two years ago, and in most oxygen was given at the same time, with the result that the patient was always of a good colour and not in any way cyanosed. If there was bleeding it was distinctly "republican," and not in any way "patrician." I attribute this to the oxygen I used with the anæsthetic.

Dr. A. G. Levy: A number of researches have been carried out on this subject, but the late Dr. J. H. Wells[1] has alone made observations upon the blood of patients who died under the influence of chloroform. His method was not so accurate as that of Buckmaster and Gardner, but his results in this series were striking. Out of samples of blood from seven patients only three contained chloroform in estimable quantities, and the average quantity in these three estimations was less than one-fourth of the average found in the blood of dogs fully anæsthetized, and less than one-sixth of the average found in the blood of dogs at death. These experiments were not at the time considered sufficiently numerous to yield any very definite data, but I wish to point out that they are in entire agreement with my conclusions as to the conditions of death under chloroform, that is, that death occurs during light anæsthesia, and is not due to an overdose. I am afraid I cannot accept Dr. Buckmaster's interpretation of a tracing he shows as tending to confirm Yandell Henderson's theory of respiratory failure under chloroform. It will be observed that the suspension of respiration occurs in the inspiration, and therefore cannot be due to a failure of the respiratory centre. In my opinion the temporary arrest of respiration is simply an expression of that holding of the breath which is such a common clinical experience during the induction of chloroform anæsthesia. Dr. Buckmaster has reviewed many interesting matters, but there is one subject to which he has not referred, and in which I am deeply interested. He has made no mention of the cause of death under chloroform, although we know from his writings he at one time took this question into consideration. I should very much like to know what his views are on this subject now.

Dr. Buckmaster: In answer to Dr. Levy's questions, I am familiar with the fact that animals voluntarily hold their breath during anæsthetization. Respiratory records have always been taken in our experiments.

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[1] Report of Special Chloroform Committee of the British Medical Association, *Brit. Med. Journ.*, 1902, ii, p. 126.

but the tracings have shown that it is exceedingly rare; and it is possible that the preliminary narcosis with nitrous oxide may account for it. A marked slowing of respiration in the early minutes is a feature of our curves, and, with the knowledge of the exceeding sensitiveness of the respiratory centre to minute changes in the blood, we have regarded this slowing as due to a direct effect upon the energy discharges of this centre. As this is the simplest, so is it the most probable, explanation. I am unwilling to say much about matters of which I have little experimental knowledge, and dislike guessing at causes for phenomena, since this leads not to knowledge but to error. As already stated, though we have lost no single animal accidentally, I have seen many deaths during the early stages of induction of anæsthesia by chloroform. Without exception, the heart in such cases is in a state of fibrillation. This is the essential cause of death. Fibrillation of the cardiac muscle resembles in many ways the idiomuscular contractions of voluntary muscle which were first described by Schiff. When once established, cardiac fibrillation rarely or never disappears in the hearts of grown animals. In the small hearts of rats and guinea-pigs fibrillation may entirely disappear, and the animal recovers. Some observers appear to have confused the state of fibrillation, which is useless for sending blood into the arteries, with feeble cardiac beats. I believe that deaths in early chloroform narcosis are due to cardiac fibrillation as a primary cause.

New Foot-key for Gas Cylinders.

Shown by O. Millauro.

Having often experienced a good deal of difficulty in turning the stop valve of gas cylinders with the ordinary metal foot-key (the spikes of which, when new, stick into the boot, and, when worn down, fail to grip), it occurred to me that much trouble and annoyance might be saved by making the key a good deal larger and by covering it with rubber so as to give a good grip to the foot. After trying several patterns, I have selected this shown as the best for simplicity and efficiency. It consists of a wooden disk, the upper surface of which has a concavity deep enough to fit the ball of the foot. This surface is covered with rubber, so as to cause a strong grip with little pressure. To the lower surface of the disk is fitted an iron plate with a cylindrical projection, in the centre of which is a square opening large enough to fit the turning pin of the valve. The entrance of the square opening is made conical to facilitate the speedy application of the foot-key, so that when the pin enters the conical cavity the least turn to the right or left will cause the key to slip into position.

The advantage of this apparatus is that, with very little pressure, the anæsthetist secures a powerful grip on the key, which results in the absolute control of the supply of gas.

The only disadvantage that this apparatus possesses is its increased size as compared with the old foot-key. To obviate this as much as possible, I have made two sizes, the larger of which is suitable for stationary use, the smaller for the anæsthetist's bag.

In conclusion, I wish to thank the Section for allowing me to show the apparatus to them, and in particular, Dr. Shipway for the kindly interest which he has taken in the matter.

Combined Gas, Oxygen, Ether and Warmed Ether Apparatus.

Shown by C. T. W. HIRSCH.

(ABSTRACT.)

[The demonstration of this apparatus is published *in extenso* in the *Lancet*, July 13, 1918, p. 48 (with illustration).]

THIS consisted of a 350-gal. nitrous oxide cylinder and a 30-ft. oxygen cylinder, each fitted with fine adjustment valves, a glass mixing chamber for the gases with visible feed, an ether bottle through which the gases can be directed at will, and a jacketed worm for warming if such is desired. The main features consist in the rubber supports for the water and ether bottles, and the method of attachment of the metallic tops to these, a perfect gas joint being obtained by a similar device to that employed on an acetylene generator. The gases are finally administered with an ordinary gas bag fitted with a Barth face-piece and a Colman valve. With this apparatus gas-oxygen anæsthesia can be maintained for long periods, rebreathing being employed to a great extent and ether when necessary.

Nitrous Oxide-Oxygen-Ether Outfit.

Shown by H. E. G. BOYLE, Captain R.A.M.C.(T.).

THIS nitrous oxide-oxygen-ether outfit was shown in three sizes. The largest of these, the size that has been selected for active service, is capable of taking, when full, four cylinders of 200-gal. capacity, together with two of 500-gal. and one 40-ft. cylinder of oxygen. The case is as compact as is possible considering the large amount of gases that are carried, and it moves easily on wheels. The second case, the one designed for hospital work at home, carries four 200-gal. of nitrous oxide and either a 20-ft. or 40-ft. cylinder of oxygen; this case is also on wheels and moves quite easily. The third case, the one meant for use in private practice, takes two 100-gal. of nitrous oxide, and two 30-gal. of oxygen. The machine is provided with a watersight feed and ether bottle, as well as a rebreathing bag and face-piece with the ordinary three-way valves; there is also a spirit lamp for warming the reducing valves of the nitrous oxide cylinders.

One of the main features of the machine is, that each cylinder is provided with a fine adjustment valve and tap, and these valves are so made that they can be easily adjusted to any ordinary English cylinder, so that the machine is capable of taking any cylinder from 25 to 200, and in some cases 500-gal. capacity.

In this machine it is quite easy, when one cylinder is empty to change to another; all that has to be done is to pull the rubber tubing off from one nozzle, put it on another, and turn on the reducing valve. It is assumed that the anæsthetist will have anticipated the change, and will have previously opened the main valve on the cylinder itself.

The exhibitor has found that with this machine the average amount of nitrous oxide required for one hour's anæsthesia is 80-gal. If it became necessary to use ether, he found that as a rule a very small quantity was required; it was hardly ever necessary to run the ether for longer than two or three minutes at a time.

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VOLUME THE ELEVENTH

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1918

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Section of Balneology and Climatology.

President—Dr. PRESTON KING.

Cheltenham: Its Mineral Waters and Climate.[1]

By R. KIRKLAND, M.B.

AFTER referring to the period from 1789 to 1850, when Cheltenham was one of the great spas of Europe to which George III, the Duke of Wellington, and Queen Adelaide resorted, Dr. Kirkland described the recent movement for its revival. The Corporation had now brought the various waters to the Central Spa at the new Town Hall, and in May of last year it took an important step in inviting five medical men of the town to form a Spa Medical Advisory Committee. Their function was to give advice to the Town Council on all matters pertaining to the spa and its successful development.

It was hoped at once to instal new baths in the Montpellier Baths, such as the Bergonié, peat, Vichy, besides the baths already in use—viz., Aix douche, brine, electric radiant heat, vapour and sulphur. The Promenade, with its avenues of chestnut trees, was the outstanding feature of Cheltenham, and in the early summer with wisteria, lilac, laburnum, red and white hawthorn in bloom, and later when the roses took their place, the appellation of "garden town" was well justified. The golf course on Cleeve Hill, at a height of nearly 1,000 ft., was easily reached by electric tram, with a common of some 1,500 acres in extent free to the public. Here patients might enjoy an air as bracing and pure as any in England, and fine views along the Severn Valley to the Malvern Hills and to the Welsh mountains.

Cheltenham might claim to take the place of Wiesbaden, but with this difference. The German spa[2] "has turned this gift (her waters) so splendidly to account without detriment to her educational institutions or residential amenities that, with a population of 100,000, she had before the War a visitors' list of 150,000, and buildings unrivalled in many a metropolis."

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[1] At a meeting of the Section, held March 14, 1918.

[2] "Health Resorts of the British Islands," edited by Neville Wood, M.D., 1912.

MINERAL WATERS.

The actual site of Cheltenham was on the lower lias overlaid in certain parts by variable depths of sand and gravel. The mineral springs were derived from the underlying beds of the new red sandstone from which the water, laden with chlorides, found its way through cracks and fissures in the various marls of the lias, deriving in its upward course various medicinal properties from the salts with which it is brought in contact. The waters of the springs which had been acquired by the Corporation had been analysed by Professor Thorpe, whose results coincided with those made by Sir Humphry Davy in 1811, and those made by Messrs. Abel and Rowney in 1847. The constancy of the chemical constituents of the waters was therefore assured. The table shows the composition of the different springs in comparison with the waters of the principal spas of Europe. Dr. Kirkland classified the waters as:—

(1) The muriated, or those which contained much chloride of sodium, such as the Pittville and Lansdown.

(2) The magnesium, or those which contained much sulphate of magnesium, such as the Chadnor and Fieldholme.

THE COMPOSITION OF THE CHELTENHAM WATERS COMPARED WITH THAT OF THE PRINCIPAL SPAS OF EUROPE, EXPRESSED IN GRAINS PER PINT.

	Sodium chloride	Sodium sulphate	Sodium bi-carbonate	Calcium carbonate	Calcium sulphate	Magnesium sulphate	Magnesium carbonate
Cheltenham—							
Fieldholme	4·622	15·980	..	5·101	5·705	17·720	...
Lansdown Terrace ...	48·875	19·465	...	3·004	0·306	2·833	1·764
Chadnor Villa	3·005	7·612	...	4·548	7·9325	14·707	...
Pittville ...	58·003	14·386	4·87	0·573	...	..	1·358
Carlsbad	10·009	22·005	13·06	2·075	...	...	1·004
Marienbad	16·032	47·055	11·35	4·098	...	...	4·016
Tarasp ...	36·070	20·067	...	22·036	3·77	...	9·060
Kissingen	55·088		...	10·017	3·73	5·062	...
Baden	20·000	2·065		1·063	7·06	...	...
Homburg	94·006		..	14·005	0·12	...	0·025
Wiesbaden	65·061	...	..	4·001	0·86	...	...
Brides les Bains	10·069	9·002	...	2·084	20·56	6·012	...

All the waters contained sulphate of soda in varying proportions, and the Pittville contained also bicarbonate of soda, and was the only alkaline water in England. The Chadnor contained carbonate and sulphate of calcium in sufficient quantity for therapeutic efficiency. The Pittville water, containing bicarbonate of soda and sulphate of soda, might therefore be termed an alkaline sulphated water resembling Marienbad more than Carlsbad, inasmuch as it was cold and contained more chloride of sodium. The Lansdown water resembled Kissingen in its sodium chloride content. It was difficult, however, to attempt a classification of spas upon the chemical constituents of their waters. For example, Brides-les-Bains was called the French Carlsbad, for it was apparently well adapted to the treatment of the same class of patients, yet the composition of its springs was not very similar. Brides-les-Bains springs contained no sodium bicarbonate, a very important distinction between them and Carlsbad. It was certain that water produced in Nature's laboratory could not be imitated by man. The effect of a synthetic production was comparatively *nil* when compared with the natural mineral waters, especially in their nascent condition, fresh from the spring.

The maladies suitable for treatment by Cheltenham waters belonged to the large class of chronic disorders caused by imperfect or perverted metabolism of the nitrogenous, carbohydrate or fatty constituents of our food. These included the functional dyspepsias, hepatic and renal affections, glycosuria and diabetes, obesity and abdominal plethora, and the ailments comprised in the generic terms goutiness and rheumatism. In the ab-articular form of goutiness or so-called rheumatism, as lumbago, sciatica, torticollis, &c., the toxins produced by this faulty metabolism were absorbed into the general circulation and deposited in the white fibrous tissue. In the articular forms we seem to have a sub-infection by attenuated yet effective bacteria. The intestinal tract was the chief potential source of fibrositis, myositis, neuritis and all non-traumatic articular disorders, and patients so affected generally drank too little water.

Excluding all dyspepsias having a basis of organic disease, some of the secondary dyspepsias were greatly benefited by treatment at Cheltenham, especially those dependent on certain affections of the liver and bile-ducts, as biliary calculi and gravel, catarrh of the bile ducts, hepatic congestion due to alcoholic or dietetic excesses. Functional dyspepsias were of two kinds: (1) The sthenic, hyperacid or hyperchlorhydric; and (2) the asthenic, hypoacid or hypochlorhydric.

The type of the sthenic dyspeptic was the man about, or a little over, middle age, strong, energetic and capable, a captain of industry or one having the burden of a large business or may be a stockbroker. His appetite was excellent, but he hurried over his meals, which he consequently bolted. He stoked for 25 knots an hour and went every knot. He was irritable and impatient, and often suffered from palpitation. About four hours after a meal he might complain of a feeling of hunger and uncomfortable distension, sometimes of such a distressing nature as to simulate angina pectoris. As a rule, heartburn and pyrosis were characteristic symptoms. This was the patient eminently suitable for spa treatment. For the danger looming ahead was the development of arteriosclerosis. The first thing to do with such people was to clear the *primæ viæ* with blue pill for one night and then begin a course of the waters. As prolonged use of waters containing a large quantity of sodium chloride increased the gastric secretion of hydrochloric acid and as they suffered already from excessive acidity, a water almost free from sodium chloride was chosen, such as the Fieldholme, given perhaps warm in the early morning and at noon. This water contained nearly equal quantities of sulphate of soda and sulphate of magnesia, and in suitable doses was an efficient purgative. With a carefully arranged diet and exercises it might be hoped that a course of these waters and the sedative climate would avert or at least delay the processes tending to arteriosclerosis. To prolong the working activities of able business men, even for a few years, was an asset to the nation.

The Fieldholme water was equally useful in obesity, abdominal plethora and hæmorrhoidal conditions.

With reference to the other type of dyspepsia—in which there was a diminution in the hydrochloric acid of the gastric juice: the subject had often been overworked or overworried, or was a convalescent from acute illness, or had resided in hot climates. The signs were a large, flabby, indented tongue, poor appetite, weak and quick pulse, pain and discomfort shortly after meals, constipation and depression. It was in this chronic form of dyspepsia that the chloride of sodium springs proved beneficial, the prolonged use increasing the gastric secretion of hydrochloric acid. The Lansdown water closely resembled Kissengen, and in asthenic conditions was best given cold. Its laxative tendency increased its usefulness. The small amount of iron it contained might also aid as a blood-restoring agent. This water should also be prescribed in chronic gastro-intestinal catarrh, which was so frequently a prelude to an attack of appendicitis.

The Pittville and the Chadnor springs might be regarded as a hepatic and a renal water respectively. The Pittville, as already stated, was the only alkaline water in Great Britain. It resembled in every particular Carlsbad or Marienbad, with more chloride of sodium, and, like the latter water, was cold. A course of Pittville water was adapted for the treatment of the same class of cases as it was customary to send to Marienbad. Such cases belonged to the chaotic category of disorders variously termed congestion of the liver, torpid liver, biliary catarrh, gastro-duodenal catarrh, tropical liver, &c. They were, however, all dependent on a common cause—viz., the absorption of toxins due to microbial agency and perverted or defective metabolism and their action on the hepatic cells and ducts. These poisons, and possibly the micro-organisms themselves, were taken up by the radicles of the portal vein, thence conveyed to the great toxin filter of the body, the liver, where the hepatic cells either destroyed or arrested them, and returned them in the bile to the bowel. But the greater part of this bile was re-absorbed again and again. At last a stage was reached when the portal blood contained so much toxic material that the hepatic cells were overtaxed and the bile excreted by them produced a low grade catarrhal inflammation of the surfaces over which it passed and upon which it rested. The catarrhal changes in the gall-bladder thus caused might eventuate in cholecystitis or the formation of gall-stones.

Should the toxins be still more virulent, or the hepatic cells more paralysed, some might pass into the systemic circulation causing headache, mental confusion, dullness, depression, gastric disturbance, and all the other phenomena comprised in the much used term "biliousness." The administration of the Pittville water in full doses washed away the poisons in the bowels and thereby lessened their absorption into the portal system. At the same time it stimulated the flow of bile and pancreatic juice and thus cleansed the biliary channels of the products of the catarrh. The evening before beginning the water it was well to give a blue pill. This water also had its uses in bronchial catarrh, glycosuria and diabetes, but in the case of obese glycosurics, Dr. Kirkland preferred the Fieldholme.

The Chadnor or renal water contained a large quantity of sulphate of magnesia, 14·707 gr. to the pint, and a very fair amount of carbonate and sulphate of calcium—viz., 4·548 gr. of the former and 7·9325 gr. of the latter. Both the magnesium and calcium salts were valuable diuretics. Why the calcium or earthy salts should act so efficiently in renal and vesical disorders it was difficult to say. But that they did so was proven by the reputation the Contrexéville Spa enjoyed in such maladies, and the principal ingredients of its waters were carbonate and sulphate of calcium. It was not claimed for the Chadnor water that it would dissolve renal or vesical calculi, but it did rid the kidney of gravel and relieve the conditions on which stone depended. A course of this water might also be advised after operations on the urinary tract. Many highly intelligent patients had assured him of the beneficial results of small doses of sulphate of magnesia in rheumatic troubles. Sir James Barr's advocacy of calcium in myalgia and kindred disorders had also led him to try the Chadnor water for rheumatic cases. As a result of the experience not only of others but of himself, he could speak of the good effects of this water in inflammation of white fibrous tissues, such as lumbago, myalgia, torticollis, and other forms of fibrositis.

These four waters therefore, differing as they did in composition, supplied weapons to the physician the value of which was only limited by the skill with which he could wield them.

In the matter of climate, Cheltenham owed much to its hills. Situated in a valley at an altitude of 206 ft. above sea-level, the hills were not so near as to cast shadows or to interfere with the amount of bright sunshine or to break the even flatness of its plain. On the other hand they were near enough effectively to shield the town from the east and north-east winds, for an escarpment of the Cotswolds partly encircled it on that side. The Welsh mountains to the South-west, and the nearer ridge of the Forest of Dean, drew off some of the moisture of the prevailing Atlantic winds. The combined results of these factors were a low rainfall, a satisfactory amount of bright sunshine and a moderate humidity. For the years 1914 and 1915, according to the reports of Mr. Saxby, the Borough meteorologist, the rainfall was 27·38 and 32·79 in., the number of rainy days being 167 and 154, and the amount of bright sunshine 1,576 and 1,562 hours per annum. The mean humidity was 81 and 82, and the mean air temperature 50·8° F. and 48·8 F., with maxima of 58 and 56, and minima of 43·7 and 41·7. The percentages of the prevailing winds for a year, taken at random, were: Calms 6·3, N. 3·8, N.E. 8·2, E. 4·8, S.E. 10·3, S. 11·6, S.W. 34·4, W. 9·3, and N.W. 11·5.

The climate of Cheltenham was therefore sedative, equable and temperate, suitable for a winter as well as a summer course, whereas the winter in most continental health resorts was of so rigorous a nature as to prevent them remaining open for health seekers for more than six months in the year. In summer the heat of the sun was mitigated by the foliage of the fine trees which lined the Promenade and other avenues. In the early morning when invalids resorted to the Spa, the air was pure and refreshing.

Throughout the year, then, the climate was well suited for cases of bronchial catarrh with scanty secretion and emphysema, for cardiac trouble, valvular or myocardial with maintained compensation, for hyperpiesis, for those suffering from degenerative affections of the blood-vessels, for chronic renal disorders, and to counteract the deleterious effects of residence in hot climates and the after effects of malaria. For those who in the present war have had their nervous systems strained and racked either in the trenches, in the heat of the fray, or at home while they only work and watch and wait, a sojourn at Cheltenham was of inestimable benefit.

It was unsuitable for cases of catarrhal or spasmodic asthma, neuralgia, simple anæmia, pulmonary tuberculosis, and for those of a fairly robust constitution convalescing from influenza, typhoid or pneumonia, or those aged under 50 who were simply run down and required a very bracing air. If such should come they must seek the bracing plateau of Cleeve Hill, access to which is easily attained by electric tram.

Whether it was that the climate led, as Dr. Kirkland believed, to a lowering of vascular tension, or that the risk of chills was minimized, people in Cheltenham lived to a great age.

If the late Sir James Goodhart's maxim were true that "plain living, high thinking and water drinking form the best protection against the ills that flesh is heir to," then the motto on the borough arms of Cheltenham, "Salubritas et Eruditio" was well justified.

Dr. FORTESCUE FOX: The new sciences of medical hydrology and climatology which grew up in Europe in the Victorian era are rapidly changing the empirical practice which formerly ruled at health resorts, and it is now incumbent upon every spa which claims to be of medical value to develop its own particular health-promoting resources (*materies therapeutica*) on a scientific or experimental basis. Whilst the duty of promoting the general amenities of a health resort belong primarily to the local authorities, the duty of taking charge of the evolution of the spa as a place of health naturallyfalls to the medical profession in each locality. Some of the guiding principles in this development at the present time may be stated as follows:—

(1) *The Care of the Springs* (*Captage* of the French).—Many waters have been allowed to deteriorate, disappear or run to waste from lack of expert knowledge and handling.

(2) *Examination of the waters* by recent methods, so as to determine for example the presence of colloidal metals and ferment actions, together with a full experimental investigation into their general actions and uses.

(3) *The Choice of Surface Treatments.*—The treatment of disabled soldiers has shown the value of external treatments, and it is desirable that a selection should be made in each locality of the procedures that are not only most useful at the present time but such as will best reinforce the action of the waters and climate.

(4) *Individuality of the Spa.*—Every health resort has its own definite indications, from natural endowment—some of the smaller and restful spas being, for example, of much value in nervous disorders. The cultivation of individuality and the limitation of indications are especially needed among the British spas.

Researches of this kind may be laborious and require prolonged observation, but it is a branch of medicine which will repay cultivation, and it is no longer possible for any spa to enjoy a permanent reputation without a well-founded scientific basis.

PRESIDENTIAL ADDRESS ON SPA TREATMENT. By PRESTON KING, M.D.

Delivered at a meeting of the Section, October 25, 1917, and published *in extenso* in the *Medical Press*, October 31, 1917, p. 328.

THE PHYSIOLOGICAL ACTION OF WHIRLPOOL AND MANIPULATION BATHS, WITH INDICATIONS FOR TREATMENT BASED THEREON. By Dr. C. F. SONNTAG.

Read at a meeting of the Section and published *in extenso* in the *Medical Press*, March 20, 1918, p. 219.

THE MENTAL FACTOR IN DISEASES TREATED AT SPAS. By PERCY G. LEWIS, M.D.

Read at a meeting of the Section, January 31, 1918, and published *in extenso* in the *Medical Press*, March 13, 1918, p. 200.

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SECTION FOR THE STUDY OF DISEASE IN CHILDREN.

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Section for the Study of Disease in Children.

President—Dr. H. MORLEY FLETCHER.

(*October* 26, 1917.)

(Dr. HUTCHISON, and later, the PRESIDENT in the Chair.)

Case of Amaurosis following Violent Convulsions.

By ERIC PRITCHARD, M.D.

D. D., AGED 2 years, was admitted into hospital on August 29, in a fit. The previous day she had been quite well, but towards evening seemed drowsy and feverish.

Past history: No previous history of fits. Not had scarlet fever; but earlier in the year had been under treatment for "some inflammation of the bladder."

On admission: Temperature 102° F. The child was unconscious. The body and limbs were extended and rigid. Mouth firmly clenched, and three teeth were taken out as she seemed to be in danger of swallowing her tongue. Eyes widely open and fixed, pupils dilated and equal. Reflexes all +. The fit lasted all the evening until 10 o'clock, when the child commenced screaming, and the temperature rose to 104° F. Heart, lungs, and abdomen apparently normal. Liver not enlarged. Spleen not felt. There seemed to be a little tenderness over the region of the kidneys posteriorly, rather more marked on the right side. Lumbar puncture negative.

Progress: After two hot-air baths (at 115° F.) the child seemed to show a little improvement, but did not show the slightest sign of recognition of her parents. Very drowsy and irritable. Temperature fluctuated for a week, and gradually fell to normal. Ten days after admission the child showed more improvement, and was able to sit up in bed; when it was noticed for the first time that she seemed blind. Ophthalmoscopic examination showed the fundus normal but very pale. Temporal side of disk pale. Edges clean cut. Vessels normal, somewhat congested. Macula normal. Retina appeared healthy. She has since made good progress, and does not seem mentally affected. Takes more notice when spoken to, and repeats her name. Examination of the eyes shows the same condition as that noted above. Wassermann reaction negative.

The urine contained albumin, pus, a few granular casts, and coliform bacilli. Owing to the condition of the urine it was at first thought that the case might be one of uræmic convulsions. The cause of the convulsions is doubtful.

DISCUSSION.

Dr. Porter Parkinson: Very little can be said against Dr. Pritchard's view, that this case is one of encephalitis of the occipital lobe, causing amaurosis. Of course, that does not explain any alteration there may be in the fundus of the eye, but, as far as I can understand, that slight difference from the normal is very slight indeed, and appears to be clearing up, whereas the amaurosis is not clearing up. If the fundus were to become fairly normal and the amaurosis still to continue, it would confirm the diagnosis of encephalitis. This condition is extremely rare, and I am certain we have never had a similar case before this Section. Neither have I read of a similar case. Still, there is every ground for thinking this is a case of encephalitis.

Dr. F. Parkes Weber: The only other possible explanation is that the loss of vision is due to a retro-bulbar neuritis of the same infective origin as the polio-encephalitis which Dr. Pritchard suggests was the cause of the whole condition.

The Chairman (Dr. Hutchison): This interesting case would be still more interesting if we could have the opportunity of seeing the case again, or hear of its subsequent progress, especially as to whether the vision returns. If the vision is restored it will constitute a strong confirmation of the polio-encephalitis view.

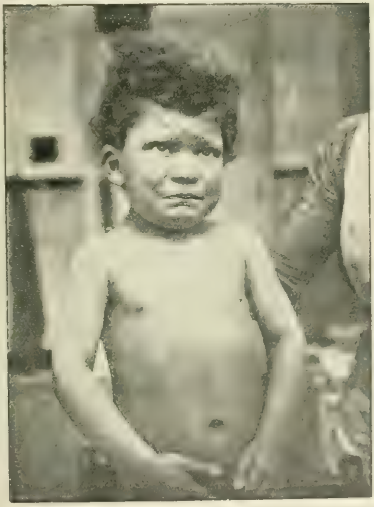
Dr. Eric Pritchard (in reply): Although I thought it extremely likely that this case was the result of an infection, I did not venture to suggest what the nature of the infection was: it might possibly be an encephalitis, due to a severe *Bacillus coli* infection, but there is no proof of this.

(*October* 26, 1917.)

Congenital Heart Disease in a Mentally Defective Child, with a Negroid-Mongoloid Facies.

By F. Parkes Weber, M.D.

The patient (S. K.) is a girl, aged 4 years, whose mother is English (a native of London) and whose father is German. Her head is brachycephalic, but large (capacious). The photograph (*see* figure) shows the general appearance of her face. The slight upward slant of the palpebral fissures outwards is, however, unfortunately not well seen in the photograph. There is no epicanthus nor epicanthus-like fold present. The broad nose, with low flat bridge, and the lips, cheeks



and soft parts above the eyes, look thick and coarse, and the face is highly coloured (at times) with red. The external parts of the ears are likewise large and thick, and the upper edges of the pinnæ are excessively folded over. The hair of the scalp is curly, but it is rather scanty (this is not well shown in the photograph); there is no hair whatever on the eyebrows and lower eyelids; and there is hardly anything in the way of upper eyelashes. The tongue is natural, that is to say it is not large, nor kept lolled out, and is not specially rough, nor of the so-called "cleft" or "fissured" or "scrotal" kind. On the whole, the child is well-grown for her age; and there is no umbilical or other hernia. There is no special curvature of the little fingers, nor is there any abnormality of the hands and feet, excepting a decided bilateral condition of flat foot (talipes valgus). Neither palm shows the single transverse "line," or "crease-mark," as described by Reginald Langdon-Down and F. G. Crookshank in some Mongoloid idiots. There are no signs of rickets. A striking feature is a condition of xerodermia, involving more or less the whole skin of the trunk and extremities. The skin is dry, rough, and in parts feels like sand-paper (owing to follicular projections). The thyroid gland is certainly not a big one, it cannot be felt distinctly.

On examination of the heart, which is not decidedly enlarged (or, according to Röntgen-ray examination, only very slightly so), there is found to be a definite systolic murmur always present, especially well heard over the base, to the left of the sternum (the so-called "pulmonary area"). Nothing else abnormal can be detected in regard to the thoracic and abdominal viscera. I think that the child must have some form of congenital heart disease, perhaps congenital pulmonary stenosis; and it is just possible that the occasional high colouring of the face may be partly connected with it. There is no history of rheumatism, and no "rheumatic nodules" can be detected. The only acute illness from which the child is known to have suffered is an attack of whooping-cough.

The patient's mother is a healthy-looking woman, aged 35 years, whose Wassermann reaction (Dr. H. Schmidt) is negative. She has, she says, only once been pregnant, the present patient being the fruit of that pregnancy. The child's father, whom I have not seen, is said to be a temperate man, in fairly good health. The child was born by the help of instruments (under chloroform); for the first month the mother suckled it, but after that it was bottle-fed.

A great difficulty is to decide on the child's mental development, for she is almost, or completely, deaf, and consequently cannot speak, and is of course "backward." She is certainly not quite clean in her habits. She is not mischievous or bad-tempered. She is said to find out things by observation, and knows that a hairbrush is meant for the hair and that a bootbrush is used for boots, and so on. She can to some extent be made to do things by gesture.

On the whole, I think it probable that a certain amount of the child's mental backwardness is not dependent on the deaf-mutism, and that there is likewise slight imbecility (more or less of the Mongolism kind) present, associated perhaps with an element of cretinism, or infantile myxœdema. The presence of hypothyroidism is suggested by the child's rough (xerodermatous) skin, scanty hair, and puffy facies. Mongolism is known to be occasionally associated with hypothyroidism, but it must be admitted that, in the present case, thyroid treatment has as yet led to no decided change of any kind; it has, however, been insufficiently tried, and will therefore be continued for a time.

There certainly seems to be a "Negroid" appearance in the child's face, and this reminds one that Dr. J. Langdon-Down,[1] in his paper of 1866, which led to the separation of a Mongoloid group of feeble-minded children, referred also to other imbecile European children as sometimes having a Negroid, a Malayan, or a North American Indian physiognomy. In the present case the facies might perhaps be termed Negroid-Mongoloid. Congenital heart disease (valvular, or due to other structural malformation) in Mongoloid idiots has been recorded by A. E. Garrod[2] and others.[3] W. Bertram Hill,[4] several years ago, summarized the various congenital abnormalities which have been met with in cases of Mongolism and thought that congenital heart disease was present in 10 to 20 per cent. of all cases. Dr. H. Morley Fletcher, however, tells me that congenital heart disease is not rare in imbecile children other than those of the Mongoloid group.

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[1] J. Langdon-Down, *London Hospital Reports*, Lond., 1866, iii, pp. 259-262.

[2] A. E. Garrod, *Trans. Clin. Soc. Lond.*, 1898, xxxi, p. 316; *Lancet*, Lond., 1898, ii, p. 1258.

[3] F. Langmead, *Reports Soc. Study Dis. Child.*, Lond., 1908, viii, p. 276; George Carpenter, *ibid.*, 1908, viii, p. 278; and others.

[4] W. Bertram Hill, *Quart. Journ. Med.*, Oxford, 1908, ii, pp. 49-68.

DISCUSSION.

Dr. CROOKSHANK: What are the points on which Dr. Weber lays stress as indicating the child's Mongoloid character, apart from the upward slant of the palpebral fissures? The father is stated to be a German: can Dr. Weber tell us from what part of Germany he comes? I ask that because I have myself seen, in the last few years, children of alleged European parentage with a distinctly Mongoloid type of feature, but on investigation have found that the parents came from the Eastern parts of Europe, and were people having racial Mongolism in their blood. It is sometimes a point calling for decision, whether the Mongoloid condition of a child is an immediate racial trait accentuated by backwardness, or whether it is that kind of Mongolism which we recognize as pathological. Has Dr. Weber noted the markings on the palms of this child? Dr. Reginald Langdon-Down has pointed out that in many "Mongoloids" the palm lines exhibit a notable difference from the normal, either the "line of the head" or "the line of the heart"—to use the language of the palmist—has become suppressed, and there is only one transverse line instead of two. That appears to be the case in 30 to 40 per cent. of marked cases. It will be interesting to know whether that is so in this child. Also, is there any epicanthus?

Dr. PORTER PARKINSON: Does Dr. Parkes Weber think that the absence of any effect from the administration of thyroid extract is not rather against there being any hypothyroidism? Nothing is more striking than the effect of thyroid treatment in a case of slight hypothyroidism. I have a very striking example in a patient who is now in hospital. The child was of very backward mental and physical development, without obvious cause. There was no evidence of ordinary cretinism: he did not look like a cretin, and had none of the characteristics of that disease. The thyroid gland could not be felt. I gave him thyroid treatment ($\frac{1}{2}$ gr. t.d.s.) on the chance that there might be some good result. Within a fortnight there was a most striking improvement. The Sister told me he did not seem to be the same child at all after that interval. And in many of the milder cases which were described by Dr. Hertoghe, of Antwerp, the result of the thyroid treatment appears to have been very striking. If Dr. Weber's case has not improved after thyroid treatment, I should doubt whether there is any hypothyroidism.

Dr. PARKES WEBER (in reply): With regard to hypothyroidism, we have only just begun the administration of thyroid extract in the dosage of 1 gr. three times a day. Before that we gave only 1 gr. every day, which I do not think is enough. With regard to the question of real so-called Mongolism being present in this child, my position is this: Here is a child who is mentally defective, unclean in its habits, and so on, so that the mental backwardness can scarcely be attributed merely to deaf-mutism. The case, therefore, falls into the large class of imbecile European children. There are two things to be noticed in the child's face, the upward outward slant of the palpebral fissures, and the rather negroid aspect of the face in general. She has also congenital heart disease and flat-foot (talipes valgus). With regard to the question of epicanthus and palmar lines (crease lines) I do not think any abnormality is present, but I will make certain and revise my description if necessary.

(*October* 26, 1917.)

Note on Three Cases of Melæna Neonatorum successfully treated by the Injection of Whole Blood or Blood Serum.

By Robert Hutchison, M.D.

Case I.—A female child, aged 2 days. The day after birth blood began to pass from the bowel; no hæmatemesis nor bleeding elsewhere. On admission the child was blanched and profuse melæna was present. The same evening 5 c.c. of human serum, obtained from the blood of a patient with erythræmia, was injected *sub cutem.* Next day there was a small attack of epistaxis but no further bleeding from the bowel, and the child was discharged a week later apparently well.

Case II.—A male child, aged 4 days. Two days after birth several hæmorrhages from the bowel took place, the last just before admission. The child was well nourished but pale and slightly icteric. Two hours after admission 5 c.c. of serum obtained from the same patient with erythræmia was injected under the skin of the back, as in the first case. Six hours later the child looked much better and only one further slight attack of bleeding had occurred. Next day the stools were normal, and three days later the child was discharged.

Case III.—A male child, aged 30 hours. Blood was noticed in the first action of the bowels after birth and four tarry stools had been passed since. There had not been any hæmatemesis. A healthy looking well-developed child of normal colour. Napkins showed stains of typical melæna of considerable amount. On afternoon of admission 8 c.c. of whole blood, furnished by the Ward Sister, were injected into the subscapular region. In the next twenty-four hours there were four more small tarry stools, after which the hæmorrhage ceased and the child was sent home well three days later.

There is, of course, nothing new in the treatment of the hæmorrhagic disease of the newly born by the injection of human blood or blood serum. It was introduced, I believe, by J. E. Welch in the year 1910, and many successful cases have been recorded since, especially in America. In this country, however, it does not appear to be very generally known yet, so it seemed worth while to put on record the three cases described above, which occurred consecutively. How the serum acts it is impossible to say with certainty, but it seems probable that it supplies some constituent which is lacking in the infant's blood and which is essential for clotting. I used human blood or blood serum in all the cases described, but normal horse serum or anti-diphtheritic serum would probably do as well.

DISCUSSION.

The PRESIDENT: The results of treatment of these cases of melæna neonatorum are most interesting. To what class of case did these children belong? Were they congenital syphilitic cases? Some cases of melæna neonatorum may be associated with familial jaundice. Did Dr. Hutchison ascertain, either before or after treatment, whether there was any evidence of fragility of corpuscles? I had a case of this description under my care about five years ago, which was treated with serum most successfully. We ought to hear the experience of members with both serum and normal blood. I should like to hear from Dr. Hutchison what method of administering the blood or serum he has found to be the most successful.

Mr. A. S. BLUNDELL BANKART: I have a number of times used coagulose for bleeding during operations, and I have found it very effective. It is an amorphous powder said to be precipitated from normal horse serum and put up by Parke, Davis and Co. I have applied it locally in solution to bleeding surfaces, and sometimes the effect has been quite astonishing. Coagulose has also been used subcutaneously at the Queen's Hospital for Children in cases of melæna neonatorum, hæmophilia, and other persistent bleeding, and it has proved so successful that it has practically become the routine treatment there for all such cases. Such a preparation always available has obvious advantages over serum, which must be freshly prepared for injection.

Dr. ERIC PRITCHARD: Is there any advantage in giving human serum rather than human blood? because there must be some delay in the preparation of the serum, and there is some danger of it becoming contaminated. I have had for some time in hospital a case of purpura, with constant hæmorrhages, which have been treated by a variety of methods. Coagulose has had a most striking effect in stopping the hæmorrhages temporarily. I have tried injection of blood from the father, hypodermically, and this also seemed to have an equally good effect, but the method is troublesome, and if the coagulose is always as effective as the serum or the blood, it will prove a very valuable substitute.

Dr. HUMPHREYS: I have tried coagulose in three cases, one of them a case of hæmophilia. The boy had been bleeding for three weeks before he came into the hospital, and the coagulose stopped the bleeding in less than half an hour. The second patient, a boy with purpura, had several attacks of epistaxis, and they have all been successfully treated with coagulose. The third case, one of hæmophilia, was admitted into hospital with a violent attack of hæmatemesis. I gave him a teaspoonful of coagulose by the mouth, and it stopped the bleeding within an hour, and he had no further attack. I have tried it locally as well, for the gums. A boy came into the Casualty for severe epistaxis, with a plug soaked with adrenalin in the nostril. I removed that plug, and put in one soaked with coagulose, and in half an hour the bleeding had ceased and the boy left the hospital.

Dr. HUTCHISON (in reply): I cannot say whether these children had congenital syphilis or no: the Wassermann test was not done on any of them. But none of them presented the obvious outward appearances of congenital syphilis. As regards dosage I have given 5 to 8 c.c., but in America, I believe, they give as much as 10 c.c. and repeat it. I have not tried coagulose. As Dr. Pritchard said, it has an enormous advantage from its being always available. I wonder whether antidiphtheritic serum would do as well. I agree it is a disadvantage to use human serum, because one has to wait for the blood to clot, and as these patients are extremely ill, time is of the greatest importance. I do not claim that these cases were cured by the treatment, but the mortality of melæna neonatorum has been put down as at 80 per cent., and here were three consecutive cases, two of them very bad, who got well immediately after the treatment, so it is reasonable to suppose there is something in it.

Section for the Study of Disease in Children.

President—Dr. Morley Fletcher.

(*November* 23, 1917.)

Case for Diagnosis.

By J. Porter Parkinson, M.D.

The patient, a boy, aged 11 years, was admitted under my care on September 24. His father and mother and four other children are alive and healthy. He had been well till a year ago when he had epistaxis and headache almost daily and is said to have become yellow in colour. Three weeks ago a swelling was noticed in the neck.

On admission he looked a well-nourished but somewhat anæmic boy. On the right side of the neck, just above the clavicle, was a tense elastic swelling resembling a mass of enlarged glands. There was very slight jaundice. The liver was somewhat enlarged, the left lobe apparently more than the right, and it felt harder than normal. The spleen could be felt extending three fingers' breadths below the costal margin. The kidneys could not be felt, and the urine was normal, except for a trace of bile pigment. No enlarged glands could be felt in the abdomen, nor elsewhere except those mentioned in the neck. The heart, lungs and other systems appeared normal. There was no evidence of congenital syphilis, and the Wassermann test was negative.

The blood count was practically normal—viz., red corpuscles, 4,080,000; leucocytes, 7,800; hæmoglobin, 62 per cent.; colour index, 7; polymorphs, 69·5 per cent.; lymphocytes, 28 per cent.; eosinophils, 2·5 per cent. No variation in size or shape of red cells; no nucleated forms; no myelocytes. The fragility of the red cells was normal.

The temperature has generally varied between 97° and 100° F. daily, with occasional rises to 101° or 102° F., and has sometimes been nearly normal for a day or two. The glands in the neck became softer and fluctuating, and on October 17 an incision was made and two or three teaspoonfuls of pus let out. The abscess has since been drained and the lump has now almost disappeared. Examination of the pus "showed very degenerate pus." No tubercle bacilli were found, and no organisms of any sort.

During his stay in the hospital the patient has seemed remarkably well, and his condition has not varied, except for a slight increase in weight. There has been no epistaxis and no jaundice after the first day or two. The urine was tested for urobilin, but with a negative result. Two days ago there was slight fluctuation in a gland in the right groin which was enlarged to the size of a haricot bean. This was opened and a few drops of purulent fluid escaped. Its appearance was the same as that removed from the neck. The glandular affection appears to be tubercular, but that does not explain the symptoms and the asymmetrical enlargement of the liver, &c.

DISCUSSION.

Dr. ERIC PRITCHARD: I have a case at present under observation which is very much like this, and I had difficulty in deciding on its group. One feature of my case was, that although the Wassermann reaction was definitely negative in the child—the test has not been applied to the parents—the mother's history suggested that the child was probably syphilitic, for there were three or four miscarriages, and then a stillbirth, and following the stillbirth this child was born. The child is now about six years old, and its general appearance is distinctly syphilitic in almost every respect; the cranium shows Parrot's nodes, the spleen is enlarged to about the same size as in Dr. Parkinson's case, and the liver is enlarged, and the teeth typical. The child has been under medical care ever since birth, but the doctor who has had charge of it tells me he regards it as a case of ordinary rickets, and he believes there is no syphilitic taint in it. I do not feel satisfied with the result of only one Wassermann test, therefore I have asked that it be repeated, and the fragility of the corpuscles ascertained.

Dr. E. C. WILLIAMS: Four or five years ago I had an apparently similar case. The patient was a boy 9 or 10 years old, and there was a history of illness sometime before he came into the hospital. He had no enlarged glands. He had been admitted for attacks of a purpuric eruption, which periodically cleared up. He also had dilated bronchial tubes. There was a distinct enlargement of both spleen and liver, the latter being enlarged chiefly in the left lobe, as in Dr. Parkinson's case. There seemed to be a tongue of tissue on the liver which could be moved about. The boy became worse, gradually developed ascites, for which he had to be tapped every fortnight, and his death occurred after eight or nine months' illness. There was no history of syphilis. I saw the parents, and they were healthy. The Wassermann reaction in the child was negative. There was no suggestion of tubercle. At the post-mortem examination we found there was tremendous thickening of the capsule of the spleen. On section, the liver cells appeared to be not perfectly healthy, but nearly so, except that there were large striæ running from the capsule into the liver substance. The pathologist who examined the sections could give no explanation as to the cause : he could only say there was a capsular cirrhosis, with thickening of the capsule of the spleen, which may have been due to some infection from the dilated tubes.

The PRESIDENT : This may be a case of some localized lesion of the liver, such as hydatid, abscess, or gumma, associated with tuberculous glands, which do not bear any connexion with the hepatic disease, such as hydatid of the liver; the left lobe is prominent and is more enlarged than the right. An enlargement which is not uniform is unusual when the case is congenital syphilis, and that is why I think the case is most probably one of associated syphilitic and tuberculous infection, and that, in spite of the blood having given a negative Wassermann reaction, the patient is suffering from congenital syphilis. I agree with Dr. Pritchard that one Wassermann test is not sufficient to exclude the diagnosis of syphilis, and I think it should be repeated. The boy does not show obvious stigmata of congenital syphilis, except the spacing of the teeth. With regard to the association of tuberculosis and syphilis in the same patient, a point I have been greatly interested in for several years past, one not infrequently finds, if one looks out for them, cases of tuberculous glands in subjects of congenital syphilis, with enlargement of the liver and spleen. I have seen several examples of this association, and if the syphilitic condition be treated, the glands become reduced in size, and the tuberculous process seems to be inhibited, or to clear up much more rapidly than would tuberculosis not so associated.

Dr. PORTER PARKINSON (in reply) : I mentioned that the urine showed no urobilin, which, in acholuric jaundice would probably be present, sometimes at any rate. There was no eosinophilia, the eosinophils were 2·5 per cent. In fact the blood picture was quite normal for a London boy. There was, perhaps, a slight deficit in the number of red cells, but they were well over 4 millions. The greater enlargement of the left lobe of the liver was very definite from the first, in fact it was more definite when the child came into the hospital than it is now : the abdomen projected, and one could feel the liver at the level of, or lower than, the limit of the right lobe, and this made me think of the possibility that the case was syphilitic. But the family history, and the result of the Wassermann test were against that, though I agree one test is not sufficient, and I intend to have one or two more done before I give up that view. I shall be interested in the effect of anti-syphilitic remedies, and will report later, and will show the boy again if there is any alteration in his condition. It has been suggested to me, privately, that it is dangerous to use mercury and iodide in ordinary doses for such a case. I did not know of such danger, though I knew of the danger from arsenic compounds. If members know of such a risk, I shall be glad to know before I start treatment. Otherwise, I propose to give 20 to 30 drops of liquor hydrarg. perchlor., with 5 gr. of iodide of potassium at first. I think there is some mixed infection, of some kind, in the case.

The PRESIDENT: With regard to treatment, I have given mercury and iodide of potassium in considerable doses to cases who have had the liver and spleen as much enlarged as this patient's, and without any ill-effects.

(*November* 23, 1917.)

Case of Amaurosis following Violent Convulsions (shown also at last Meeting).

By ERIC PRITCHARD, M.D.

IT was suggested that I should report on this case again, as to whether the amaurosis was temporary or permanent. Shortly after showing the child at the last meeting, the sight gradually began to improve, and I understand from the mother that the child can now see its way about and can protect itself.

Since the last meeting, I have been able to find the reference to certain other cases which were shown or described before the Society for the Study of Disease in Children, by Mr. Sydney Stephenson and Dr. Ashby.[2] Those authors give the literature of such cases, and provide notes of cases of their own. At the meeting there was a very good discussion. The conclusions they drew were these: (1) That there is a form of amaurosis which occurs in infants or young children which is post-eclamptic, due to anæsthesia of the visual centres. (2) That the convulsions, which may be due to various causes, are apt to be severe, and accompanied by coma; but that the amaurosis may be associated with aphasia and paresis of hemiplegic distribution; the hemiplegia may be permanent. Finally, that the amaurosis is, for the most part, transient. It is possible that in some instances there is hemianopia. In

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[1] *Proceedings*, p. 1.

[2] *Reports Soc. Study Dis. Child.*, 1903, iii, pp. 197-209.

1903 members were invited to give particulars of other cases, but I have been through all the reports since then, and, as far as I can see, not a single case has since been brought before either that society or this Section. But I understand there are certain members of this Section who have seen somewhat similar cases that seemed to fit in with this group, which may be anything from a transient eclamptic fit, leaving no sequelæ, to a very violent eclamptic fit, leaving behind a complete hemiplegia. In some of these there are sensory symptoms, amaurosis, which may be temporary or permanent, or aphasia, which, as far as I can gather, is nearly always temporary, unless accompanied by very severe general paralysis. Stephenson and Ashby say that whatever the origin the convulsions themselves may be, the actual cause of the amaurosis is exhaustion of the visual centres by the explosive character of the nerve disturbance. That seems to explain my case, for the convulsions were very serious, and lasted three or four days, and the amount of exhaustion inflicted on the nerve-cells might suffice to account for the somewhat prolonged amaurosis of six or seven weeks' duration. The total number of such cases, apparently, before the paper I refer to was written, was five, and no doubt the authors searched the literature for records. Most of them were from the *Transactions* of the Ophthalmological Society. As I explained at the last meeting, the diagnosis was obscured by the presence of very obvious urinary symptoms; there was albumin, and a distinct coli infection. I think the convulsions and the bacilluria were independent conditions, that there was a *Bacillus coli* infection of the bladder, and a violent epileptic attack, which may have been predisposed to by the bladder condition; but I also think there must have been some other reason, and that the severe disturbance in the cortical cells resulted in exhaustion and amaurosis.

DISCUSSION.

The PRESIDENT: I am afraid I cannot quite agree with what Stephenson and Ashby and Dr. Pritchard brought forward as an explanation of the blindness in these cases, that is to say, that the eclamptic fits caused exhaustion of the centres. It is like bringing forward one symptom to explain another, instead of trying to find a definite cause for the condition. It seems to me much more probable that in these cases, in which there has been a prolonged series of fits—as long as three days continuously in this instance—there must be some grave cerebral condition, of which the fits are merely the manifestation. In most of these cases in which children have prolonged epileptiform convulsions the cause is some cerebral irritation, set up by a meningitis, or by an encephalitis, with or without meningitis. In this case we do not know, for certain, the nature of the encephalitis, but there was definite evidence of a severe *Bacillus coli* infection of the urinary tract. I regard the convulsions as a manifestation of encephalitis, and as a consequence part of the visual apparatus has suffered. One point in favour of this view is the fact that Dr. Pritchard told me he considered the child's mental condition after the illness was not as good as it was before it. I regard the condition in this case as the result of an obscure form of encephalitis, and as the only clue available was the presence of the *Bacillus coli* infection, I do not see why this should not be associated with the cerebral condition. I would like to ask what is the prognosis with regard to vision.

Dr. PRITCHARD (in reply): With regard to prognosis, I do not think the authors of that paper are very definite in their views, because they admit that the epileptic condition may be due to so many different causes, and I suppose they take into consideration the hereditary quality of the nerve-cells. In the group of cases they give—a considerable number—the prognosis of the amaurosis is favourable. But they quote only five cases, and these, with the five they give themselves, only total ten, and I have been unable to find others. Some of the patients were permanently hemiplegic, those in which the Rolandic area was involved. Where I disagree with the President is, I cannot help thinking that the disposition of the brain itself is an important factor as to whether there is an eclamptic fit or not. There may be severe irritations of the brain substance without causing an eclamptic condition. I still regard the epileptic condition as an entity, and not due entirely to the causative factor, the infection with *Bacillus coli*, or with the organism of polio-encephalitis or polio-myelitis. I think for this to occur there must be both suitable soil and suitable seed.

(*November* 23, 1917.)

Congenital Malformation of the Left Temporo-maxillary Joint and both Hip Joints.

By A. S. BLUNDELL BANKART, M.C.

GIRL, aged 5 years. The condylar process on the left side was found (at operation) fused with the base of the skull and the zygoma. The left hip showed appearances typical of Calvé's disease (osteochondritis). The right hip showed an advanced stage of the same condition, with complete destruction of the head of the femur and coxa vara.

Section for the Study of Disease in Children.

President—Dr. H. Morley Fletcher.

Vitiligo Patches with Central Pigmented Mole.[1]

By J. L. Bunch, M.D.

A girl, aged 12 years, had the hair of the scalp removed by X-rays six years ago for ringworm. The hair grew again and was normally pigmented. One year ago she developed two well-marked patches of canities and the hair over these two areas is still quite white. Associated with this hair change she began to show patches of vitiligo and there are now thirty-five such patches, varying in size from a five-shilling piece to a sixpence, some quite irregular in outline, some more or less circular. In the centre of three of these white patches is a raised, deeply pigmented lesion, apparently a mole. Two of these patches, with a central mole, are situated on the body, and one on the neck. This abnormal condition of a vitiligo I believe to be extraordinarily rare. With regard to the white patches of hair, it is difficult to assume any direct connexion between the previous epilation by X-rays and the subsequent development of canities.

DISCUSSION.

Dr. F. Parkes Weber: I regard the case as one of true vitiligo, and I think it extremely unlikely that the application of X-rays to the scalp had anything to do with the development of so many patches of vitiligo over various parts of the body. I believe that in a certain number of cases of vitiligo, when there are congenital moles present, there is a tendency for the development of circular or oval leucodermic patches around these moles, with the moles as centres. As far as I know, only one paper has been written directly bearing on the subject.[1] But several years ago, in the Clinical Section, an adult patient with jaundice was shown on whose skin there were scattered spots, which I do not doubt were congenital pigment moles, like those present in Dr. Bunch's case. They were surrounded by pale areas, and it looked as if these "halos" round the pigment moles were comparatively free from pigment.[2] Those are the only two cases I know which resemble the condition in Dr. Bunch's case. But I believe that when attention is drawn to the subject, it will be found that in ordinary cases of vitiligo when moles happen to be present, there is a tendency for the leucodermic patches of the vitiligo disorder to form around the moles like "halos" with the moles as centres.

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[1] At a meeting of the Section, January 17, 1918.

Dr. VINCENT DICKINSON: Did Dr. Bunch have the Wassermann test done in this case, because my impression from the perusal of French and Italian literature during the last few years is that there is a tendency in France and Italy to attribute vitiligo to congenital syphilis, owing to some degeneration of the posterior nerve roots or nerve ganglia?

Dr. F. PARKES WEBER: Many French authorities are against the theory that vitiligo is necessarily connected with syphilis.[3]

Dr. BUNCH (in reply): I do not look upon vitiligo as being always due to congenital syphilis: the matter was threshed out some time ago at the Section of Dermatology, and few of the speakers there thought so. However I will have a Wassermann test done. With regard to these pigmented moles, if they are congenital they must have preceded the leucodermic patches, and I would like to ask Dr. Weber why the pigment should have disappeared round the pigmented moles while the moles themselves retain most of their original pigment. If it were a phagocytic process, one would have thought the moles would have been attacked as well.

Dr. F. PARKES WEBER: If I may make a guess, what I think most probably happens is as follows. In certain cases of congenital moles (especially pigmented moles) there is a latent congenital deficiency of pigment-production in the skin immediately around the mole. And I suggest that this remains latent until some excess of pigmentation occurs in the neighbourhood, such as may happen if the patient develops vitiligo. Then the leucodermic patches of the vitiligo tend to form "halos" around the moles, and become themselves surrounded by abnormally dark-coloured skin.

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[1] R. L. Sutton, "An Unusual Variety of Vitiligo (Leucoderma Acquisitum Centrifugum)," *Journ. Cutan. Dis.*, 1916, xxxiv, p. 797 (two cases).

[2] H. D. Rolleston, "Jaundice, with Areas of Unpigmented Skin," *Proc. Roy. Soc. Med.*, 1910, iii (Clin. Sect.), p. 195; cf. remarks by F. P. Weber in the discussion. A good illustration was given.

[3] Cf. the more recent opinions, expressed by Brocq and by Pierre Marie, *Bull. Soc. Méd. des Hôp. de Paris*, 1917, 3 sér., xli, p. 771; and Vignolo-Lutati (Turin), *Giorn. ital. delle mal. vener.*, 1916, lvii, pp. 317-351.

Case of Primary Sarcoma of Lung, with Specimens.

By J. PORTER PARKINSON, M.D.

THE patient, a boy aged 5 years, was admitted into the Queen's Hospital on October 2, 1917. He had just been discharged from the surgical wards after an operation for inguinal hernia. He complained of cough and difficulty of breathing, only noticed since his discharge. His temperature varied between 101° and 102° F., pulse from 130 to 160, and breathing 40 to 60 a minute. He had orthopnœa and much dyspnœa on any movement. The whole of the left side of the chest, front and back, was dull, especially below, the movement very limited and the breath sounds very weak and harsh. There was some ægophony at the angle of the scapula. The heart was displaced to the right, its deep dullness reaching to halfway between the right sternal edge and the right nipple. On the second rib, close to its junction with the cartilage, was a small raised lump the size of half a walnut, immobile and elastic, but not fluctuating. The chest was explored and blood-stained fluid withdrawn; later 4 oz. of blood-stained fluid were removed. Microscopically this fluid consisted almost entirely of blood and no other cells could be distinguished and no organisms were found. A day or two later a large hæmorrhagic swelling appeared over the sternum, which soon disappeared. The child developed a severe paroxysmal cough, and gradually the dullness over the left lung extended across the sternum to the right and the displacement of the heart became much more marked. Wasting now became pronounced. In December he began to suffer from occasional attacks of dyspnœa with cyanosis, relieved by oxygen. These became more frequent and he died on December 14, ten weeks after his admission.

At the necropsy the whole of the left lung was a mass of soft, pliable, hæmorrhagic growth, very firmly adherent to the chest wall, diaphragm and pericardium; hardly any lung tissue remained. This growth extended well beyond the right edge of the sternum, compressing and causing collapse of the right lung, which was otherwise normal except for the presence of two small nodules of growth in the lower lobe. The glands at the bifurcation of the trachea were not enlarged; the bronchial glands were involved in the mass of the growth. Heart and pericardium normal, but displaced so as to lie on the right of the sternum. The nodule on the rib was about the size of a plum and was continuous with the intrathoracic growth. All the other organs were normal.

The microscopical appearance was that of a small round-celled sarcoma.

A case almost identical with the above was shown before the Society for the Study of Disease in Children by Dr. Langmead in 1906, who stated that since 1860 there had been only one previous post-mortem on such a case at the Great Ormond Street Hospital. The signs, as in Dr. Langmead's case, were most suggestive of pleural effusion, and curiously enough a tumour on the second rib was present in both cases.

For the post-mortem notes of the case I am indebted to Miss Stogden, the pathologist.

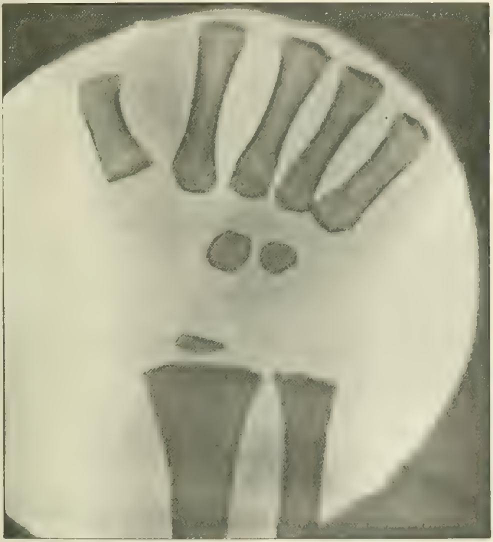
Cases of Cretinism.

By H. C. Cameron, M.D.

Case I: Cretinism showing Delay of Epiphyseal Development.—D. B., aged 6½ years; height, 3 ft. 4 in.; weight, 34 lb. Attends school. Is very quiet and "has not much to say." Was brought to me because she was thought to have "a growth at the back of the nose." The symptoms of cretinism were not marked, but the skin was dry and the eyelids were a little puffy. The eyebrows were poorly developed. A skiagram of the wrist (*see* fig., p. 21) shows the epiphyses have reached the stage of development of a normal child of less than 3 years. The os magnum and the unciform have begun to ossify, but the centre for the pyramidal and the proximal epiphysis of the first and the distal epiphyses of the other four metacarpal bones, all of which should have appeared by the third year, are absent.

Case II: Almost untreated Cretinism in a Girl, aged 28 years.—G. M. H., 4 ft. high. She is said still to be growing. The epiphyses, however, appear all to have united. She is said not to have grown at all till the age of 7 years when for two years she was treated from time to time. During these two years "teeth were cut," and she became able to walk with difficulty. Since then there has been no treatment. She shows most of the features of cretinism. The skin is harsh and dry, and there is infiltration of the subcutaneous tissue, especially of face and hands. Menstruation is said to be normal. She can speak a few words. She still plays with dolls.

I meant to bring more of these cases, but at the last I could not find those I wanted. The patient who best illustrates my point shows a mild degree of cretinism. Indeed, there was some doubt at first as to whether she was a true cretin. Whenever I have had indubitably good effects from thyroid extract, I have found epiphyseal delay. In this child, aged 6½ years, who looks perhaps 5 years old, the epiphyses are those of a child of less than 3 years—that is, the infantilism is more marked in the epiphyses than in the general appearance of the child. I have never found any condition which I thought was due to a subthyroid state without very marked delay in the development of the epiphyses. There are those who see, in a whole multitude of conditions, evidence of thyroid insufficiency — enuresis for example, or xerodermia, or undescended testicles, and no doubt these do occur in subthyroid conditions. But these conditions only acquire weight as evidence of a subthyroid condition if there is at the same time evidence in the body of infantilism and delay in the appearance of the epiphyses. This delay has not, I think, been sufficiently used in practice



to test the value of the suggestion, sometimes made on very indefinite grounds, that hypothyroidism is present in a given case.

Dr. CAMERON (in reply to questions): The child has been under treatment four months, without marked change in the epiphyseal centres, but with steady improvement in other ways. Other forms of infantilism in the experience of any physician are rare. Cretinism is, after all, the common form of infantilism. But I think that delay in epiphyseal development is not nearly so marked in other forms of infantilism. For instance, in the girl aged 17 years, whom I show to-day (p. 23), so far as I can see, the epiphyseal development is not seriously backward.

Case of Osteomalacia and Infantilism, with Horseshoe Kidney and Interstitial Nephritis.

By H. C. CAMERON, M.D.

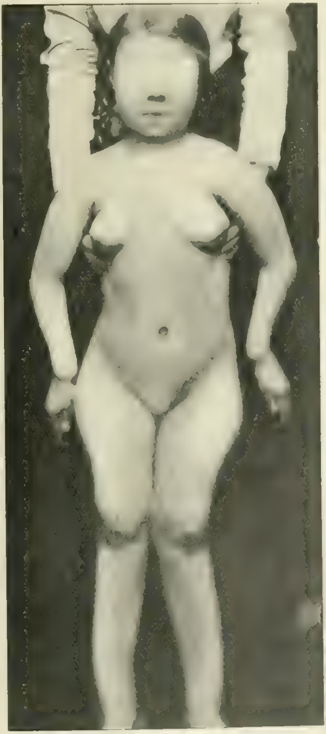
L. M., AGED 15 years, was admitted to Guy's Hospital on July 7, 1911, for extreme deformity of the wrists and knees, which had appeared some eighteen months before. For the last six months she had been unable to walk, complaining of pains in the legs. For some years she had not grown in height, but had become fatter. She was dwarfed and infantile in appearance. Before admission she had been treated with thyroid extract. Osteotomy was performed to reduce the curvature at wrists and ankles. After two days, complete suppression of urine was noted with increasing drowsiness. Death followed on the fourth day after the operation. The urine was said to be free from albumin.

At the autopsy I removed the radius and femur. The bones were very slender, and so softened that in places they could be cut readily with a penknife. The lower end of the radius was bent outwards, forming a complete right angle with the shaft. The neck of the femur showed an angle with the shaft of less than a right angle. The skiagrams show great irregularity of the epiphyseal line with extensive unossified areas in the surrounding bone. The kidneys were represented by a single horseshoe structure situated low down over the third and fourth lumbar vertebræ; the capsule was thick and adherent, and there were numerous cysts. The microscopical changes were those of chronic interstitial nephritis. The left ventricle was somewhat hypertrophied. There were atheromatous changes in the mitral valve. No abnormality was detected in the thyroid or other ductless glands.

Case of Osteomalacia and Infantilism, with Renal Deficiency.

By H. C. CAMERON, M.D.

L. S., AGED 17 years, was admitted to Guy's Hospital for deformities of wrist, knees and ankle. She was said not to have grown since the age of 9 years, when her legs and arms first began to bend and to be



painful. Since April, 1917, she has been unable to walk at all. Her height is 3 ft. 8 in. The radius and ulna are bent sharply outward at their lower ends. The condyles of the femur are displaced inwards and backwards and the lower end of the tibia is acutely curved as in rickets. The intelligence and general health is good. The urine is dilute, and the specific gravity is 1005 to 1008; it contains albumin but no casts; the daily amount passed averages about 70 oz. The blood contains urea 2·07 grm. per 1,000 c.c., or about seven times the normal. Ambard's constant is 0·798, or about ten times the normal. The skiagrams show similar changes to the first case of cretinism. The Wassermann test is negative.

Museum Specimen.—The bones of the upper and lower extremities from Mr. Davies-Colley's case, *Transactions of the Pathological Society*, xxx—shown for comparison.

DISCUSSION.

The PRESIDENT: These cases form a very important group. Dr. Cameron has not mentioned his opinion on the relationship of these cases to so-called delayed rickets. Many cases have been recorded of severe curvature of a bone or of bones occurring in children and classed as delayed rickets. Nearly all the cases I have seen had albuminuria. I think the case I showed before the Section in 1911, as a case of infantilism associated with renal disease, was one of the first exhibited here. That patient was a small boy who exhibited the deformity of the bones often present in congenital nephritic cases. The commonest type of bone deformity is genu valgum. Another case, which I saw with Mr. Elmslie, was a girl aged 10 or 12 years, who very rapidly developed great curvature of one femur. There was associated nephritis. It belonged undoubtedly to the same group as the present cases. Has Dr. Cameron seen cases in which only one bone has been involved? I do not think we really know anything as to the pathology of these cases. I consider that the lesion which occurs first is the renal one, and the bony manifestations occur later.

Dr. F. PARKES WEBER: One of the chief points of interest is the nature of the kidney change in these cases in which infantilism is associated with fibrotic kidneys in children. It has been suggested that these cases are due to congenital syphilis. I feel inclined to take another view, and to regard the kidney disease, in this group of cases, as a congenital or developmental dysplasia (mal-development) of the kidneys, related to congenital malformations and other congenital abnormalities; and I would specially draw attention to the fact that in one of Dr. Cameron's cases (the one in which the bones obtained from the necropsy were shown) to-day there was also a horseshoe kidney, which is a congenital developmental abnormality. Our President, Dr. H. Morley Fletcher, was one of the first to draw attention to cases of chronic fibrosis of the kidneys associated with infantilism in children. It seems to me that in such cases, when there is definite evidence of the renal condition being associated with a chronic inflammatory change, as there was in Dr. Morley Fletcher's case, one may with great probability regard the kidney disease as a chronic interstitial nephritis on a basis of congenital dysplasia of the kidney substance (with congenital deficiency of the true glandular tissue).

Dr. PORTER PARKINSON: Some time ago I showed a case of infantilism in association with diabetes, the last instance of infantilism I showed. The patient has since died, and the pancreas is being sectioned now. I also had a case some years ago, in which there was kidney trouble and infantilism.

Dr. CAMERON (in reply): With regard to the question as to whether the kidney disease is congenital, or whether it is inflammatory in origin, casts have been very carefully looked for in this very severe case, but have not been found. The character of the urine has suggested that there may be a polycystic congenital abnormality, but we cannot feel any enlarged cystic kidney, neither does X-ray examination show anything of the sort.

Case of Erythromelalgia.

By H. C. CAMERON, M.D.

THE patient is a Jewish girl, aged 10 years, who last year suffered from a very extreme swelling of the hands and feet. This year the feet and the right hand, though cold and cyanosed, have not shown the condition so distinctly, but the left hand is very swollen and also very painful, and that in much greater degree than is apparent to-day. It gets much worse in cold weather, and it was in the recent severe weather that matters reached a crisis. On her holding the hands up for some minutes the swelling completely subsides, and there remains only some stiffness and atrophy of muscles. Clearly, the condition is not Raynaud's disease. It involves the whole hand, not the individual fingers. The swelling has been so great that if it were a case of Raynaud's disease I should have expected some necrosis to have appeared. I have called it a case of erythromelalgia, although against this diagnosis one may place the fact that the hand seems distinctly too cold, rather than too warm. Some years ago I showed a case of extreme cyanosis of the extremities, which was definitely associated with chronic constipation and severe attacks of abdominal pain. Ultimately a highly diseased colon was removed, following on suppurative inflammation around the colon. That was a case of acrocyanosis associated with a definite toxæmia of alimentary origin. In this child there have been no other symptoms.

DISCUSSION.

Dr. F. PARKES WEBER: I am familiar with this kind of "acrocyanosis" (I prefer this term to "erythromelalgia") as a rather rare condition in young (mostly female) adults, and I regard the condition as a functional nervous disease, although I am certain that some of the cases are connected with a chronic toxæmia of intestinal origin (with coprostasis in the colon). Still, the condition is essentially a functional nervous disease, and it is only very few of the patients suffering from chronic toxæmia of intestinal origin who develop such an acrocyanosis of the hand. The ultimate prognosis is good on the whole, in regard to the hand. I suggest that in the present case the paresis of the hand is not a separate condition and is not due to the turgid congestion (acrocyanotic swelling), but is part of what one might call a functional muscular (motor) and vasomotor paresis of the extremity, that it is in fact a variety of so-called "hysterical" monoplegia, associated with the turgid swollen condition (due to passive congestion and vasomotor paresis) which is often associated with organic or functional paralysis of an extremity (especially an upper extremity).

Dr. CAMERON (in reply): What Dr. Parkes Weber has said corresponds with what Weir Mitchell stated in his first description of erythromelalgia. He regarded it as a vasomotor neurosis, and that is the view in this case of Dr. Jessie Campbell, who sent the child to me. The girl attends a Jewish school, and Dr. Campbell tells me they have had a good deal of neurosis, of one sort or another, in these young Jewish girls. Recently there has been an epidemic of dermatitis artefacta. All the treatment suggested for erythromelalgia is treatment of the nervous symptoms, such as the prescription of bromides. I think it is not a chilblain because it involves the whole hand. Chilblains are usually on the fingers, isolated, discrete lesions. The skin of the hand in this case has never been broken, even under the most extreme distension.

Congenital Obliteration of the Bile-ducts.

By W. E. Carnegie Dickson, M.D.

(ABSTRACT.)

The case is of interest owing to the rapidly fatal issue, supervening on the third day after birth, the jaundice itself appearing on the second day. The birth was normal, and the child appeared healthy, but a rapidly increasing jaundice appeared on the second day; it became comatose, with spasmodic breathing, and it died in just under three days from birth. At the autopsy, complete obliteration of the cystic, common, and right and left hepatic ducts was found, with marked dilatation of the intrahepatic bile-ducts, and deep bile staining and congestion of the liver. Except for the general jaundice, the only other point of interest was the extreme congestion and hæmorrhages in the suprarenals, especially the medulla. Microscopical examination of the liver did not show any of the biliary cirrhosis usually described in such cases—the bile-ducts showing marked dilatation and some surrounding "small-celled infiltration." The liver cells showed marked cloudy swelling and fatty degeneration, and, especially towards the periphery of the lobules, were packed with bile pigment. There did not appear to be any septic cause, and the case on the whole is suggestive of the non-development of the lumen of the extrahepatic bile-ducts, rather than of their obliteration by some inflammatory process such as a cholangitis.

DISCUSSION.

Dr. Eric Pritchard: If, as stated, there was one normal motion, it is to be presumed that it was a meconium action, containing bile pigment, which would suggest that the obliteration of the bile-ducts found post mortem must have occurred at a very late pre-natal period, possibly even at the time of birth, or afterwards. Is there anything which contra-indicates the view that all this might be a post-natal condition, due possibly to some septic infection of the child? It is said that the cord itself was healthy, but I believe there are cases on record in which there has been absolutely nothing abnormal, to the naked eye, either at the navel or in its obliterated continuation, and yet on microscopical examination almost a solid strand of bacteria has been found in these structures, without causing an abscess or any indication of a septic infection. So possibly this might be due to some such septic infection occurring through the cord and tracking up to the liver, thus causing cholangitis, which in this early stage of life might have caused blocking and the subsequent jaundice and other symptoms. The sudden death may not have been necessarily due to the jaundice but to the septic condition. We have not heard whether there was any marked rise of temperature. [Dr. SIMPSON: There was no rise of temperature.]

Dr. SIMPSON: I asked the nurse about the motions: she was an experienced nurse, and the event took place in a nursing home in Welbeck Street. At 11 o'clock at night she told me no water had passed since midday. There was a stain which, by electric light, seemed to be like meconium: its area was not so large as that of a two-shilling piece. There was no reason at all why the child should be septic, because the dressings had been sterilized from the moment the child was born. I do not know whether the child passed normal meconium.

Dr. CARNEGIE DICKSON (in reply): With regard to the meconium being normal, there is always the danger that some of the bile stained urine may get mixed with the motions from the bowel, and this mixture might give the appearance of ordinary meconium. In the majority of these cases, unless there is supervening pneumonia, or some other complication, one does not find a raised temperature. I felt confident there was no sepsis present, but naked-eye appearances may deceive. My concern was with finding the cause of death, and I was satisfied as to the cause of death. I have had some experience in following out bile-ducts in children, even though very small ducts, as I have dissected large numbers for various purposes. I am confident there was no lumen here, only a mere fibrous thread; a history of three days would not account for a fibrosis to that extent: in an ordinary infant such a fibrosis would have been a matter of months. In other words, I think fibrosis had been proceeding in the uterus a considerable time, and the question is whether the bile-ducts were developed at all, or not. I suppose originally they are solid buds, and that the lumen of the duct only appears later; so that in these cases there is a possibility that it does not get developed.

Section for the Study of Disease in Children.

President—Dr. H. MORLEY FLETCHER.

Case of Hæmophilia with Effusion into Knee-joints.[1]

By PAUL BERNARD ROTH, F.R.C.S.

A BOY, aged 6½ years, was admitted into hospital on January 26, 1918, with greatly swollen knee-joints, his mother stating he was a hæmophilic. The family history is negative. The patient is an only child; previous to his birth the mother had one miscarriage at the fifth month, preceded by repeated severe hæmorrhages. When she has had teeth removed there has been considerable bleeding for several days after. Of the mother's relations, two sisters are alive and well; both are married; one has two boys, the other a girl, all of whom are healthy; father died at 70 years, of old age, he was not a bleeder; mother died at the age of 60 years of cancer of stomach; this mother had two brothers, who are both alive and well and are not bleeders.

History of Case.—The patient was quite well till 2 years old, when he fell down and tore the upper lip at the frenum; bled for three days; was treated with electric cautery. Soon after he bled from his tongue for three days. At the age of 3 years the right knee swelled up suddenly; after some time the swelling subsided. Since then the swelling has appeared and subsided repeatedly and the knee has never been quite well. He also used to bruise very easily: thus the whole of his back, or the whole of one arm from the elbow to the shoulder would become bruised. This tendency has not been noticed for some time. When 6 years old his left knee suddenly swelled up and then subsided; it has done this several times since. Only six weeks ago he started to bleed profusely from the socket of a milk molar; the bleeding lasted for a week: it was always much worse at night. *On admission*, he was seen to be an intelligent, fresh complexioned, well nourished boy with brown hair and eyes. His tonsils were hypertrophied as large as cherries, blocking up his throat. His knees were enormously enlarged, especially the right, which was hot and glazed in appearance. A few days after admission he scratched his left index finger one afternoon and the next morning his shirt and bedclothes were covered with blood; the scratch bled for two days. The swelling of the left knee gradually disappeared, but that of the right has only slightly lessened.

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[1] At a meeting of the Section, held February 22, 1918.

Radiograms of the right knee (figs. 1 and 2), by Dr. W. Ironside Bruce, show extensive wearing away and absorption of the cartilage and bone of the articular surfaces of the femur, tibia and patella; in addition there is to be noticed the formation or building up of new bone at the edges, that is to say, lipping, and a dark shadow surrounding the bones, an appearance evidently due to distension of the synovial cavity with blood. These appearances, taken together and with the age of the patient, are unlike those in any other condition, and I suggest that they are pathognomonic of hæmophilia.

I believe this to be true hæmophilia, in spite of the absence of a family history. I have no doubt that the joint effusions are due to blood, but some writers say definitely that in these cases the effusion is not due to blood, but serum. Some state that the effusion consists of mixed serum and blood, and others that in every case the effusion is blood. So there does not seem to be any certainty on this point. In this case, the radiogram shows a marked shadow, and I do not doubt that the swelling is due to blood. The mother mentioned that several times when the child bled the bleeding was worse at night, a fact which has been noted by all writers on hæmophilia. The family history, in this case, as far as it goes, does not show anything, but presumably he had ancestors who did bleed unduly, or this may be the start of a new hæmophilic stock.

Another point of interest is the diagnosis. When first seen, this case was, without a proper examination, diagnosed as tuberculous knee, and in the literature cases are described as having been diagnosed as acute tuberculosis, even by good observers, and some have been operated upon and have died. In this case the mother arrived with the patient and said he was a hæmophilic, and in such an event the confirmation, when the idea is present, becomes easy: but I think that if there were any difficulty, the X-ray examination would clear up the doubt.

With regard to prognosis, the chief feature in this case, to my mind, is the damage to the knees, which are practically sure to become more or less ankylosed. I want to obviate that as much as possible by endeavouring to prevent other attacks of effusion into the joint by limiting the movements of the joint with splints. I shall therefore apply two "knee cages," and give regular doses of thymus gland and Ca and Mg salts. His two large tonsils are seriously interfering with his breathing; they seem completely to block up the back of his throat.

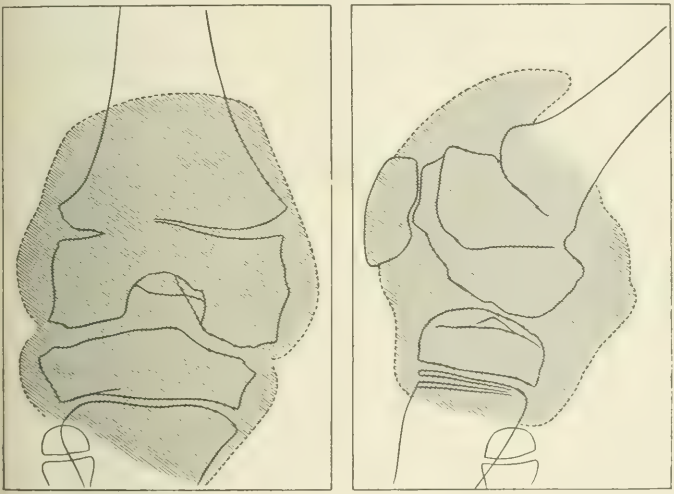

FIG. 1. FIG. 2.

Case of hæmophilia, with effusion into knee-joints. Drawings of radiograms of right of knee by Dr. W. Ironside Bruce.

Fig. 1.—Antero-posterior view. Note the wearing away of the articular surfaces, the lipping, and the shadow due to effused blood.

Fig. 2.—Lateral view. Note the wearing away; and the shadow due to effused blood, especially in subcrural pouch.

He has also got a very high-arched palate. But for the hæmophilia, those tonsils would have been removed at once. In order to get him better, we want to improve the quality of his blood, and the best way to do that is to give him a free air-passage. So that here we have a vicious circle: we cannot remove the tonsils because of the peculiarity of his blood, and the enlarged tonsils stand in the way of efforts to improve the blood. In the literature dealing with operations on tonsils in hæmophilic subjects, I find that an American surgeon, Packard, did remove the tonsils of a known hæmophilic, and the case bled afterwards for six days. At the end of that time, the bleeding was stopped by injecting 20 c.c. of rabbit serum into the buttock. I should like to meet a laryngologist who is willing to remove these tonsils: I think it ought to be done.

DISCUSSION.

Dr. ERIC PRITCHARD: This boy has an exceedingly small expansion of the chest, and the large tonsils almost block up the airway; therefore every effort should be made to remove the tonsils. I think that it would be a feasible operation, if after calcium treatment the coagulability of the blood were ascertained before such operation. I should have no hesitation, under such conditions, in recommending an operation in the present case, provided that coagulose were ready at hand for use immediately after the operation.

Mr. T. P. BEDDOES: These cases cause considerable anxiety, and the question always arises as to what to do suddenly when one comes across them unknowingly. I knew a lad who had no family history, and had been under observation for hæmophilia about twelve years. He was quite comfortable up to last summer. He looked fairly robust, but was very under-sized: his age was 16 years, but he looked to be about 12 years. Suddenly he had a hæmorrhage, and that was the end of him. Another case which came under my notice was a hæmophilic who had diphtheria, and for that disease was injected with antitoxin. He had a subcutaneous hæmorrhage at the site of injection, which broke down and caused considerable anxiety; but it was not opened, because at the time the swelling appeared the boy was known to have hæmophilia. A little of the fluid was drawn out, and a small opening left for drainage, and he did well. Hæmophilia is particularly important because of its family history. We know that the Duke of Albany was of mature age when he died. His sister, Princess Alice, had two children, one of whom is the present ex-Czarina of Russia. According to Dr. C. G. Cumston,[1] the ex-Czarevitch, who is the subject of hæmophilia, has twice nearly died from hæmorrhage.

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[1] *New York Med. Journ.*, January 12, 1918, p. 77.

Specimens of Case of Enlarged Liver and Spleen, shown at the Meeting in November, 1917.

By J. PORTER PARKINSON, M.D.

THE patient died in the middle of January. For a fortnight previously the temperature had been raised to about 102° F. each day, but no fresh physical signs were observed. The liver and spleen were unchanged in size. Post mortem both lungs were found to be fibrous at the roots with enlargement but no caseation of the bronchial glands. Miliary tubercles were scattered throughout both upper lobes, and there was a small amount of greenish lymph on the surface of the right lung. The pericardium contained about a pint of thick greenish-yellow pus, in which pneumococci were found, and it was densely adherent to the left lung. The heart was covered with a ragged deposit of lymph, and its surface was distinctly fatty. The valves were competent. There was no free fluid in the abdominal cavity. The liver was only slightly enlarged and weighed 38 oz. It was typically hobnailed, and the capsule slightly thickened. The cut surface showed a coarse multilobular cirrhosis, and microscopically there was much exudation of leucocytes round the portal canals. Tubercles were scattered throughout. The spleen was much enlarged and weighed 15 oz., the capsule was slightly thickened. It contained numerous tubercular masses, many of which were caseating. Microscopically there was a marked general infiltration with lymphocytes. The kidneys weighed 4¾ oz. and 5 oz. respectively, and were much encysted. The capsule stripped with difficulty and was slightly thickened. There was marked cloudy swelling of the convoluted tubules, with infiltration of the intertubular tissue with leucocytes, and a few scattered miliary tubercles. The mesenteric glands were enlarged, but not caseous. No tubercles nor ulceration were seen in any part of the intestine or peritoneum.

Clinically this boy had had occasional rises of temperature for a week or so, with apyrexial intervals. There were breaking down of glands in the neck and signs of enlargement of the liver and especially of the spleen.

I should like to ask the Section whether they consider the cirrhotic liver and greatly enlarged spleen can be due to tuberculosis alone. I believe the splenic enlargement might be due solely to tuberculosis, though the enlargement was greatly in excess of what is usually found, but the distinctly cirrhotic liver cannot, I think, be due to tuberculosis alone. At a previous meeting syphilis was suggested as the cause, but the Wassermann reaction was definitely negative on three separate occasions. There is no reason to suspect alcohol as a cause, the child's parents being of a class unlikely to allow this. The pronounced enlargement of the left lobe of the liver during life—which one or two members of the Section thought might be due to a tumour—was more apparent than real, the projection of it being caused by the deformity of the organ due to the cirrhosis.

DISCUSSION.

Dr. ERIC PRITCHARD: We are very lucky to have had an opportunity of seeing, post mortem, these organs, which a considerable number of members of the Section examined ante mortem. Those who were present at the meeting will remember the very good discussion we had on that subject. I recall that our President, Dr. Morley Fletcher, thought it was a case of a combination of tuberculosis and syphilis. I do not quite agree with what Dr. Parkinson has said as to the tumour in the left lobe of the liver. I must say that the post-mortem finding is exactly what I expected. There was a hard mass, almost like a tumour, just below the centre of the sternum, very much in the position of the gall-bladder. It was suggested it might be a hydatid, but it seemed very hard for that. Those who examined it post mortem will note what a hard mass it is. Feeling it through the abdominal wall, one might think it was all in one lobe, whereas it is in the other lobe as well. It is an extremely interesting case, because many of us who were present at that meeting cited cases somewhat similar which we had seen ante mortem, and had not always had the opportunity of seeing post mortem. I had one myself, which corresponded very closely: the child is still alive, and I have persisted in the opinion that it is a case of somewhat doubtful syphilis. We never could get a positive Wassermann reaction, but, in spite of that, I contend there is some syphilitic taint. Now, however, in the light of what Dr. Parkinson tells us, I shall go more carefully into the question of it being tubercle, and shall abandon the syphilitic theory.

Dr. PORTER PARKINSON (in reply): With regard to the question of the tumour, to which allusion has been made, I meant a tumour which was decidedly on the left side of the abdomen: it came down in just the position of the left lobe of the liver. It was not as far to the left as the spleen, but it extended decidedly below the projection of the right lobe. It gave one the impression that the left lobe was very much more enlarged than was the right lobe. The post-mortem specimen cannot be held to support what one thought during the child's life. I think it was the left lobe of the liver which was felt, and that the liver must have been somewhat rotated, otherwise I do not see how the tumour could have been produced. I believe there was a decided projection to the left of what Dr. Pritchard refers to, and the post-mortem specimen does not show how it was produced; hence I assume there was rotation.

Enlarged Suprarenals and Sudden Death in an Infant.

By Eric Pritchard, M.D.

These are specimens of enlarged suprarenals from an infant aged 5 weeks, who died suddenly. Post mortem no cause of death could be discovered. The suprarenals are very large, about the size usually found in a fœtus at the sixth month; otherwise they are quite normal macroscopically and microscopically. The thymus and other glands are also normal.

DISCUSSION.

The Chairman (Dr. Parkinson): This is a very peculiar case of extra-sudden death, and I know of no parallel case in which the suprarenals were associated. Many years ago I examined the suprarenals and kidneys of a large number of fœtuses of various ages, and I found that, especially at about 6 months of age, the suprarenal of the fœtus was decidedly larger than the kidney, and appeared to be an actively functionating gland. After that stage of fœtal life, the suprarenal began to diminish in size, until, in the newly-born, it was found to be decidedly smaller than the kidney, though larger than one finds it at a still later stage. The present case looks like one in which the functional activity and the size of the suprarenals has continued for some time. Will Dr. Pritchard say what was the size of the thymus and some of the other glands with an internal secretion? For I have heard of instances of sudden death in cases in which the thymus has been enlarged, death having been due not altogether to pressure, but to some alteration in the internal secretion. Has there been some alteration in those other glands as well as in the suprarenals?

Dr. Eric Pritchard (in reply): On naked-eye examination, at any rate, there was nothing abnormal about this child's thymus. Neither was there anything abnormal about any other gland which was examined. Histologically the suprarenals simply show hypertrophy. I agree with Dr. Parkinson that one might reasonably have expected to find some abnormality in the other endocrinous glands. It may be there is some close association between the suprarenal and the thymus, and if the thymus was not enlarged in this case, the explanation may be that the suprarenal was taking on the work of the thymus, and might show the same tendency to cause sudden death as does enlargement of the thymus.

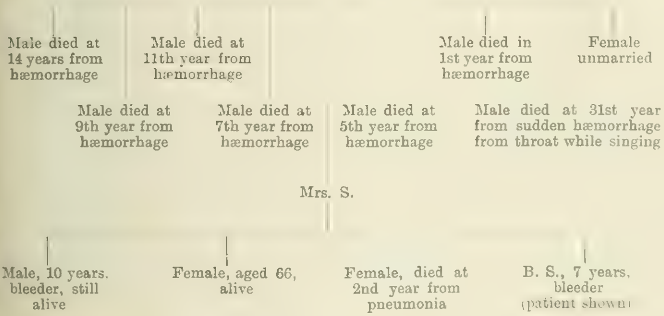
Section for the Study of Disease in Children.

President—Dr. H. Morley Fletcher.

Case of Hæmophilia.[1]

By Eric Pritchard, M.D.

B. S., a boy, aged 7 years, was admitted to the Queen's Hospital for Children with a large hæmatoma under the palmar fascia. By fixation of the hand on a splint in an elevated position the swelling gradually disappeared, and the boy was able to leave the hospital after three weeks' treatment. As far as can be ascertained, there is no foreign blood in the family. The mother states that many of her family had died from bleeding. The following is the family history as far as the mother can give it:—


Male died at 14 years from hæmorrhage
Male died at 11th year from hæmorrhage
Male died in 1st year from hæmorrhage
Female unmarried
Male died at 9th year from hæmorrhage
Male died at 7th year from hæmorrhage
Male died at 5th year from hæmorrhage
Male died at 31st year from sudden hæmorrhage from throat while singing
Mrs. S.
Male, 10 years, bleeder, still alive
Female, aged 66, alive
Female, died at 2nd year from pneumonia
B. S., 7 years, bleeder (patient shown)

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[1] At a meeting of the Section, held March 22, 1918.

Congenital Morbus Cordis; ? Patent Ductus Arteriosus.

By J. Porter Parkinson, M.D.

A boy, aged 9 years, was admitted into the hospital at the end of an attack of acute pneumonia. His mother had died of heart disease, and there was a history of similar trouble in her family. The boy is said to have suffered from heart disease since birth, but has been healthy otherwise; no history of acute rheumatism.

On admission he had signs of consolidation of the right lower lobe of the lung and a temperature of 103° F., which terminated by crisis the following day.

The heart's apex beat is in the fifth intercostal space, just inside the nipple line. The cardiac dullness is normal to percussion and on X-ray examination. A loud, rough, long systolic murmur is heard all over the precordium, loudest at the aortic base, and conducted well up into the neck on the right side. This murmur is also extremely loud over the back of the chest, especially in the interscapular area. A systolic thrill is occasionally felt in the second right intercostal space. The pulse is 96, regular, rather small. There is no cyanosis nor clubbing. The other organs are normal.

The three most likely conditions appear to be patent ductus arteriosus with deficiency in the interventricular septum, stenosis of the ostium, and coarctation of the aorta. The latter condition is unlikely, as the radial pulse is small instead of being larger than usual, and there is no undue pulsation of the arteries of the neck. Stenosis of the ostium is very rare, and the pulse is usually slow, which is not the case here. Hence I think that probably the ductus arteriosus is patent. This condition usually produces hypertrophy of the right ventricle unless the interventricular septum is patent, so it is probable this defect is also present. The systolic murmur is often prolonged into the diastolic period, but this is not a constant feature; in all other respects the signs correspond with what is usually found in this condition.

In addition the boy is much undersized and the lack of development is shown by only six carpal bones showing centres of ossification instead of seven, which is usual at this age.

Case of Congenital Heart Disease.

By ERIC PRITCHARD, M.D.

MALE child, aged 7 months. Breast fed; began to waste at the age of 6 weeks. Had attack of diarrhœa and vomiting at the age of 4 months. Was admitted to Queen's Hospital when aged 5 months for cough. On admission child was collapsed, head retracted, with rigidity of legs, but no bulging of fontanelle, and loud rhonchi and râles over whole of back and front. No abnormality of heart sounds recorded. Lumbar puncture was performed, result negative.

Sixteen days after admission the heart was found to be enlarged to the right, and a loud systolic murmur heard best to the right of the sternum. This murmur gradually changed in character and became less marked to the right of the sternum, and more distinct over the usual mitral area. There never has been any blueness. The loud murmur to the right of the sternum was in character that usually produced by a congenital defect in the interventricular septum; there are no symptoms of tricuspid regurgitation. No pulsation in veins and no enlargement of liver.

At first I regarded the case as a congenital one, the murmur being obscured by the loud adventitious sounds in the lungs; later I revised my opinion, but I cannot reconcile the auscultatory evidence with any pathological condition of the heart with which I am acquainted.

DISCUSSION.

Dr. EDMUND CAUTLEY: Dr. Parkinson's case is one of aortic stenosis, with secondary mitral regurgitation. There is a well marked systolic aortic murmur, in the common situation of a murmur of aortic stenosis and of the usual kind. I do not agree that the pulse is not slow, unless Dr. Parkinson uses that term in the sense of infrequent: I think it is very small and of slow rise. And there is a murmur in the mitral region which, I think, is independent of the aortic murmur. The X-rays show definite cardiac hypertrophy of the left ventricle. The apex seemed to me to be a little further out and lower down than Dr. Parkinson mentioned: I think it is probably under the sixth rib, in the nipple line. There is not sufficient evidence as to whether it is congenital or not. Congenital aortic stenosis is very rare, and acquired stenosis is rare at this age. But I have seen cases of both types, and the possibility of it being of rheumatic origin must be borne in mind, remembering the history of cardiac disease in the mother. In Dr. Pritchard's case, the diagnosis depends very greatly on the accuracy of Dr. Pritchard's clinical observations, and, knowing how careful he is, one should attach great weight to the fact that he found no murmur when the child first came in: I do not think it likely that he would have overlooked such a murmur if it had been present. In my opinion the murmurs are due to tricuspid regurgitation and mitral regurgitation. There is general dilatation and enlargement of the heart, and I think it probable that the condition is not congenital but due to a general dilatation of the heart. I have seen a similar condition in a child rather younger, in whom there was such a loud systolic murmur all over that I was convinced there was some gross malformation of the heart, yet post-mortem there was nothing found but dilatation.

Dr. PARKINSON (in reply): My view is that there is a double congenital lesion—viz., a patent ductus arteriosus, and patency in the mid-portion of the interventricular septum. Those two conditions would account for the murmur. Still, there is much to be said for Dr. Cautley's view that the condition is an acquired one. Both these conditions are fairly rare, and acquired aortic stenosis at this age, I should have said, is the rarer of the two. I did not think it likely that aortic stenosis would occur in such a child, who had had no illness. I made careful inquiry about these vague rheumatic evidences, such as sore throats, "growing pains." The mother says the child suffered from nothing until admitted for pneumonia. Dr. Cautley's diagnosis does not take into account the skiagram which shows Gerhardt's ribbon shadow, nor the fact that the heart is little if at all enlarged, and also it would not explain the infantilism. With regard to Dr. Pritchard's case, I agree with Dr. Cautley: he has stated almost exactly what I should have said, that it looked like a greatly dilated heart, and I took it to be due to the pneumonic poison acting on the heart muscle and causing that dilatation.

Case of Hæmophilia with Effusion into Knee-joints.

Radiograms of right knee by Dr. W. IRONSIDE BRUCE.

By PAUL BERNARD ROTH, F.R.C.S.

Note.—This case was described in the *Proceedings* for April (vol. xi, No. 6, pp. 29-32, *q.v.*), illustrated by drawings only of the radiograms. It has now been possible to obtain reproductions of the actual plates, as shown on p. 41 (figs. 1, 2).

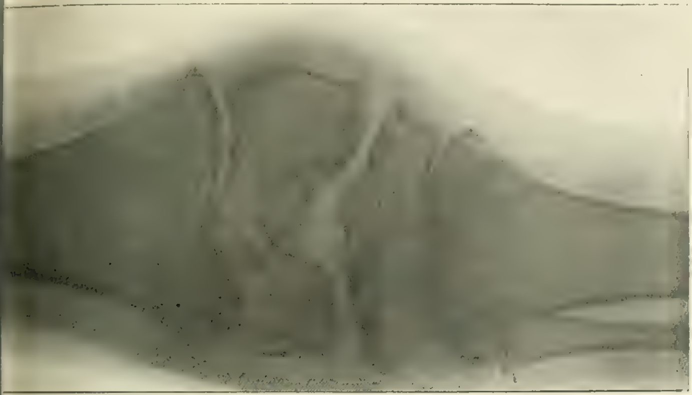

Fig. 2.

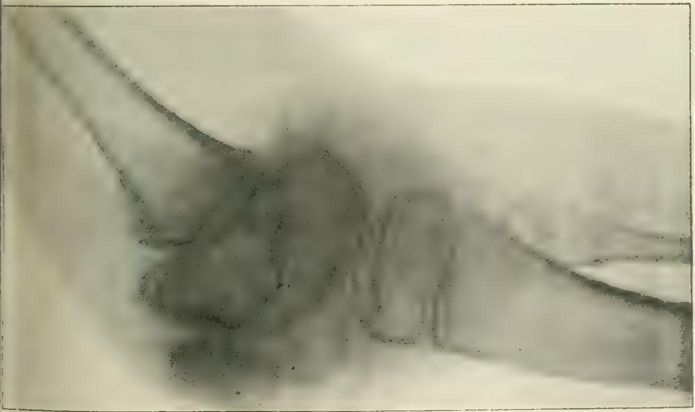

Fig. 1.

Case of Osteogenesis Imperfecta (Fragilitas Ossium, Fœtal Rickets).

By Paul Bernard Roth, F.R.C.S.

D. J., aged 14 years, was apparently a perfectly healthy baby. When aged 16 months one thigh bone fractured, and since then she has had ten fractures of her thigh bones, the right femur being affected seven times, and the left three. The last fracture took place when aged 9½ years: she was wearing a Hoefftcke splint at the time. When aged 7 years she came under the care of Mr. Carless, at King's College Hospital: he performed cuneiform osteotomy of both femora; the right bone united splendidly, but the left took a very long time.

She was first seen by me two years ago, and since then the limbs have been kept in Thomas's caliper knee splints, and attempts been made by means of bands and kneecaps to straighten out the various deformities present, which attempts have been partially successful. I have recently done a cuneiform osteotomy of the left tibia to rectify the severe bowing forwards of that bone. This has been a success as regards alignment, but bony union has been somewhat delayed. Thus operation took place on October 8, 1917, and union is not yet absolutely firm.

The problem which is troubling me now is the deformity of the upper end of the right tibia causing the knee to be extremely recurved. The radiogram by Dr. W. Ironside Bruce shows that a bend has taken place ½ in. below the epiphyseal line, with the concavity forwards. This deformity was present when I first saw the case, and I have not been able to modify it in any way.

Dr. H. C. Cameron: This is a well marked example of a mild degree of osteogenesis imperfecta. It is characteristic of it that the fracture occurred a long time after birth. The cases of maximum severity are either born dead or die shortly after birth. That is one of the reasons why we might well advocate the making of more post-mortem examinations upon cases of still-birth than is the case at present. It might throw light on a number of these rare conditions. That fracture occurred many months after birth is an indication that a case is mild, and that there will probably be recovery towards the later part of childhood. It is comparatively common to find only the femur affected. The temporal bulging in this case is slight but present. It is interesting to try to assess exactly what can be done for so apparently intractable a condition as this. One feels that if one of these cases had been continuously under observation, one ought to have been able to prevent the development of the degree of deformity which seems to be the rule when cases are allowed to go away from medical care.

Coagulation of the Blood in Purpura and Hæmophilia.

By W. PALMER LUCAS, M.D.

(ABSTRACT.)

THE results of clinical experiments carried out at the University of California Hospital according to the methods laid down by Professor W. H. Howell, Professor of Physiology at the Johns Hopkins Hospital, were described. There were three factors essential to clotting: (1) Prothrombin, (2) thrombin elements, and (3) antithrombin, a substance which held the blood in solution. In cases where the coagulability of the blood was diminished there was either an increase of antithrombin (as in purpura due to streptococcal infection and in some cases of jaundice), or a decrease of prothrombin. In Henoch's purpura prothrombin was decreased during an attack; the decrease was very pronounced, and the amount of prothrombin present always below the normal. In blood transfusion for purpura Dr. Lucas preferred whole blood to serum, as in some serums there was an excess of antithrombin. Good results had been obtained from blood transfusion in purpura, but in the case of hæmophilia he had not been able to raise the prothrombin factor enough to obtain success. He usually depended upon rest and local treatment. Hæmophiliacs usually bled up to a certain point and then stopped. If the blood were examined at once (and it was very difficult to get a sample at this time) the factors were practically in the normal condition. The whole factor in coagulation was the balance between pro- and antithrombin.

DISCUSSION.

Dr. H. C. CAMERON: Has this work helped in the prognosis, which is extraordinarily difficult in purpura? The prognosis depends, to some extent, on the site of the extravasation. The worst cases of purpura I have seen have been those with hæmorrhage into the pia mater, and that would be a factor from which one could get no information in the laboratory. Has Dr. Lucas noticed, in hæmophilia, an appreciation on the part of the patient of the moment when recovery is about to take place? Two adult patients whom I saw were able repeatedly to predict the moment of recovery from a bleeding attack.

Dr. ERIC PRITCHARD: Does Dr. Lucas consider it is unadvisable to treat these cases empirically in the absence of laboratory guidance? For instance, in cases of hæmophilia would it be unwise to try injections of brain solution, or of serum, or of coagulose?

The CHAIRMAN (Dr. Hutchison): Has Dr. Lucas made observations on the disease in the case of the newly born? Also, has he any idea why the prothrombin or the antithrombin is deficient in any given case? These researches, so far as they have gone, of course only carry us one stage further back.

Dr. W. PALMER LUCAS (in reply): It is not advisable to employ the method in private homes. A hospital laboratory is required. Kephalin is not by any means a panacea. I have used it on hæmophiliacs, but it has not had much effect. With regard to hæmorrhagic disease of the newly born there are two types: (1) In which the antithrombin factor is increased: the septic cases belong to this type and the mortality is very high on this account. (2) In which there is diminution in the amount of prothrombin. I have had a case which recovered after whole-blood transfusion. So far I have done no work on syphilitic cases. The reason for the want of balance between pro- and antithrombin is at present being worked at, but, so far, analyses have given no results. The investigation is a difficult one on account of the number of tissues which have to be analysed. In rheumatic purpura prothrombin is usually deficient. I have noticed that hæmophiliac patients are often able to predict the moment of recovery. This is especially so in the case of children, who often announce that they are "now going to get quite all right," even though there appears to be no change in the condition. It is often possible to tell the moment of recovery, even from the facial expression.

Agminated Molluscum Contagiosum.

By J. L. BUNCH, M.D.

THE patient, a boy aged 11 years, has thirteen molluscum contagiosum lesions in the left axilla. They are confined to this region. The first appeared three months ago, and is now pink, raised, slightly pedunculated, about the size of a very small marble. It has a central depression, and is situated just anterior to the posterior axillary fold. The second largest is situated just anterior to this and is slightly inflamed—probably due to friction. The other eleven lesions vary in size from a pea to a millet seed, are pinkish or flesh-coloured, and show more or less well marked central depressions. They are scattered through the axilla, but some of them tend to form little groups, of which the largest group comprises four lesions. During the two weeks during which the boy has been under my care no fresh lesions have appeared. No subjective symptoms are complained of. The boy has never had a Turkish bath, but does a good deal of swimming in a swimming-bath. Such agminated lesions in this particular region appear to be very rare.

Section for the Study of Disease in Children.

President—Dr. H. MORLEY FLETCHER.

Case for Diagnosis.[1]

By H. C. CAMERON, M.D., and W. H. TRETHOWAN, F.R.C.S.

A. H., AGED 5 years, was born at full time and was breast-fed. There are six children in the family. All the others are in good health. There have been no deaths and no miscarriages. The father and mother are healthy. Some difference between the two sides of the body was noticed at birth. No family history of similar deformities can be obtained. The left arm and leg are considerably shorter than the right, as shown in the photograph (fig. 1). The boy uses both hands freely and is able to walk and run, though with some limp because of the genu valgum on the left side. The phalanges of the left hand are shortened, and are broader and flatter than those of the right. The little finger shows a permanent flexion which cannot be reduced (fig. 2). The right arm and leg are apparently normal and well formed. The little finger of the right hand alone shows a slight deformity in its proximal phalanx (fig. 2).

There appears to be no abnormality of the nervous or vascular supply to the affected limbs. A Wassermann test was negative. Physical examination disclosed no other abnormality. The signs of rickets are very slight. An X-ray examination showed surprisingly advanced changes in the bones of the affected limbs. The changes were most marked in the femur (fig. 3) and in the hand (fig. 4), skiagrams of which are shown. The epiphyses are normal throughout. The diaphysis of the phalanges is distended into a mushroom-shaped expansion, with its convex surface towards the distal end of the bone.

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[1] At a meeting of the Section, held April 26, 1918.

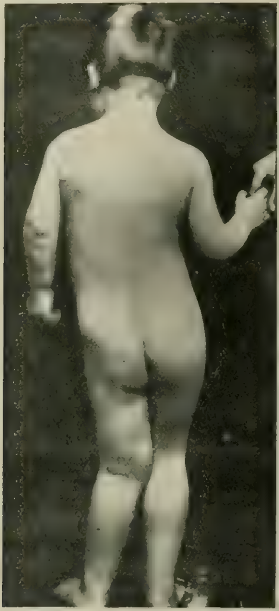

Fig. 1.

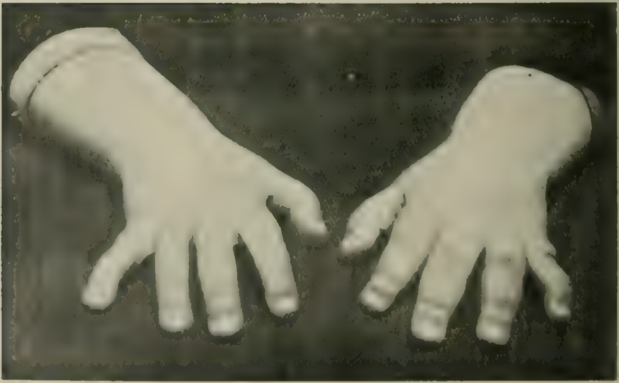

Fig. 2.

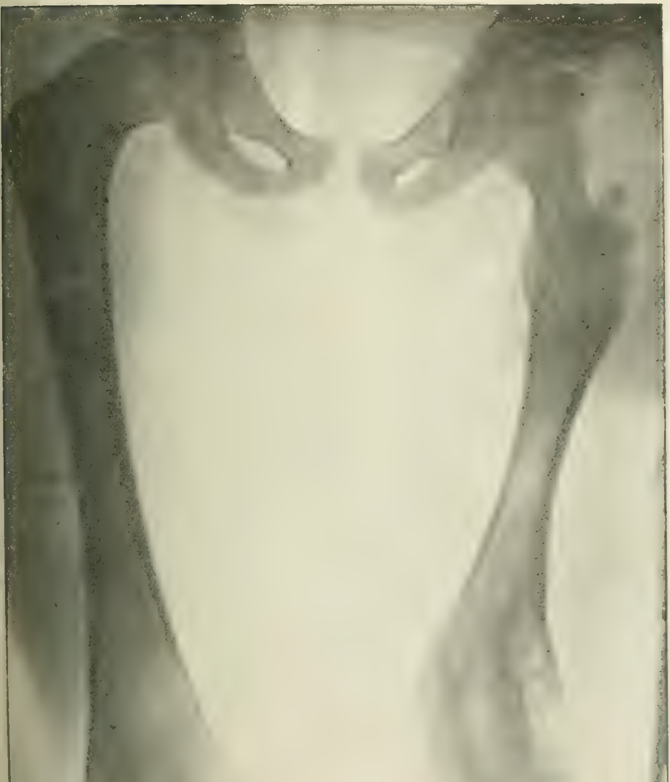

Fig. 3.

The metatarsal bones show a general expansion. The outline of the femur is in many places indistinct, and the shadow is mottled by irregular areas of lighter colour.

The only abnormality seen in the skiagram on the right side of the body is in the little finger, where changes, similar but of earlier development, are visible. Otherwise the distribution is one-sided.

Careful examination disclosed no abnormality of the skull, the ribs, the scapulæ, or of the bones of the pelvis.

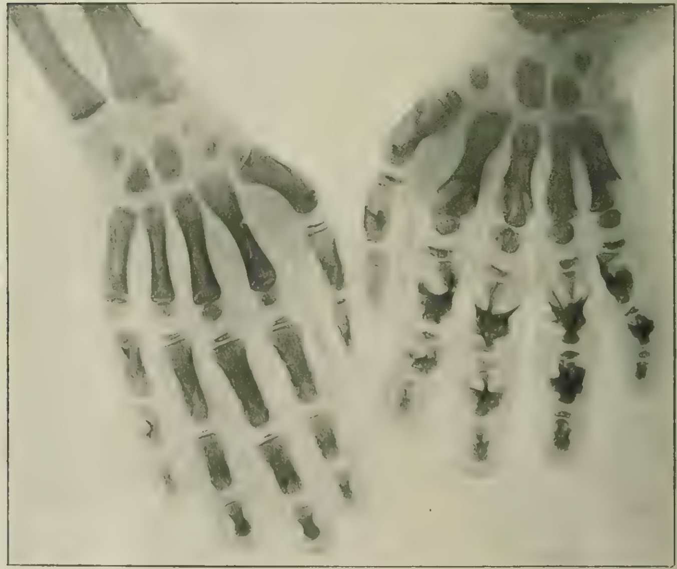

FIG. 4.

DISCUSSION.

Dr. F. PARKES WEBER: This case is clearly an atypical one of multiple enchondromata of bones. The only real peculiarities about it consist in the fact that it is almost, but not quite, confined to one (the left) side, and, secondly, that it is associated with relatively little external alteration in the outline of the bones.

Dr. **ERIC PRITCHARD**: On what grounds does Dr. Parkes Weber make so very definite a diagnosis? Not only are the bones and the cartilaginous portions of the hands affected, but the whole of the tissues remain in a flabby, undeveloped, embryonic condition. If the condition were one of enchondroma from birth, there would now have been more evidence of the condition than the child shows at present.

Dr. **F. PARKES WEBER**: The enchondromata, in this case, are within the bones. The classification of the case seems to me to be perfectly clear. One must remember that in cases of non-malignant tumours elsewhere in the body, there may be a peculiarity of position, so that, instead of the tumour or tumours projecting outside the organ or into its cavity, they may be confined almost entirely to the walls of the organ (intramural). Thus, in cases of intramural fibromyomata (fibroids) of the uterus, the uterus looks different from one in which the fibromyomata are projecting outside the organ or into its cavity. If, by any chance, the growths in the present case should grow outwards, there would then be chondromata or chondro-exostoses projecting from the bones, as in ordinary cases of multiple chondromata and multiple exostoses. The primary fault in a case like the present one is clearly a developmental malplasia (dysplasia) of bones, and it would not be at all surprising if portions of the growths were found to be of a lower (less specialized and more embryonic) type of tissue than even cartilage. Thyroid treatment has been suggested for trial in cases of multiple enchondromata.

Dr. **HARRY CAMPBELL**: I would like to draw attention to the large accumulation of subcutaneous fat in this case. One knows that children of that age are often plump, but in this case I take it the amount of fat is almost pathological. One notices the excess of fat in the hands. So I venture to raise the question, very tentatively, whether there is in the case any hypopituitarism. Evidently there is some defect in the development of the skeleton. Has the excess of fat any relationship to the condition of the pituitary body?

Dr. **CAMERON** (in reply): I have never before met with such a case. I accept Dr. Parkes Weber's suggestion that the appearances are due to multiple enchondromata. The soft parts are less involved in the dwarfing process than the bones and, as a result, seem to be wrapped somewhat loosely round the bones of the limbs. A similar disproportion is often seen in achondroplasiac limbs in young children.

Perithelioma of the Orbit.

By H. R. Jeremy, F.R.C.S.

Girl, aged 4 years. Admitted to the Queen's Hospital for Children on April 14, 1917, with diagnosis of "dermoid of orbit." History: Mother has noticed child's right eye growing out during past three months. Child has screamed with pains in eye, forehead and temple. No history of vomiting. No history of injury. Family history good. Mother has had two children; no miscarriages.

Physical signs on admission: The right eye is proptosed, and displaced downwards and outwards. Muscular movements of eye restricted upwards and inwards. Conjunctiva; congested vessels, chemosis below. Cornea clear; iris reacts; right pupil equal to left. Fundus: Vessels full and tortuous; disk, clean-cut edge, normal colour. Under the upper and inner margin of orbit can be felt a soft lobulated tumour, not adherent to skin, and movable from side to side, but attached deeply. The surface of the tumour is smooth and elastic. No enlarged glands. Nothing abnormal in chest or abdomen.

Operation: Two-inch incision through base of upper lid exposed a soft white tumour, which was encapsulated; it consisted of two lobes united by a stalk, which was attached to the optic nerve behind. The stalk was ligatured close to the nerve, and the two lobes removed. There was very little hæmorrhage. The tumour was friable, and ruptured during removal. There were slight adhesions between the capsule and surrounding structures. The wound was dried, and closed with silkworm sutures. During the following week the child contracted scarlet fever, and was removed to a Fever Hospital.

She was re-admitted two months later, with recurrence of the tumour, which had now forced the eye so far forwards that it could not be covered by the lids. As a result of exposure the cornea had ulcerated and ruptured and the iris was prolapsed through the corneal wound. The surface of the tumour was ulcerating. It was now useless to try and save the eye, so I removed the eye, and cleared out the entire contents of the orbit, including the periosteum.

The child has remained well since the operation, and has increased in weight. There was a slight cough in January, which disappeared on treatment, and there were no physical signs of any recurrence in the lungs.

Dr. Mildred Stogdon has kindly cut and examined a section of the tumour, and diagnoses it as a perithelioma.

DISCUSSION.

Dr. ERIC PRITCHARD: Does Mr. Jeremy consider the microscopical evidence to be sufficiently convincing to warrant the diagnosis of perithelioma? I did not see, in the specimen, more than one small piece which looked characteristic of perithelioma.

Mr. JEREMY (in reply): My chief evidence is the pathologist's report, which I accepted. I have some other sections which show clearly in the growth the curious whorls of spindle cells and myxomatous degeneration of the stroma, very much like that seen in brain growths.

Discussion on "Botulism."

The PRESIDENT: As we have not many cases to-day, I thought I might bring forward for discussion a subject which has been in my mind for two or three weeks, as it probably has also in the minds of other members of the Section—namely, the question of botulism. In the last fortnight or three weeks I have seen two cases, which have given me trouble as to diagnosis. I saw both the cases on the same day, though they did not come from the same part of London, nor were they in any way related to each other. One was a boy, aged 3 years, the other a girl, aged 2 years. The girl's history was, that during the ten days previous to my seeing her she had been getting very sleepy, and her mother was concerned about her because she fell asleep even over her meals, and it became increasingly difficult to rouse her. The onset was quite gradual, and the child did not seem to suffer in any way: there was no pain nor other disorder. I saw her when she was brought up to the Children's Department of St. Bartholomew's Hospital. She was well nourished and of good colour, but very stuporous. When aroused, she would look sleepily at one, and then rapidly relapse into the somnolent state. The pulse was slightly irregular in frequency. She had complete double ptosis, the right pupil was larger than the left, and there was left external strabismus. There was very little objective beyond that; the reflexes were normal, and there was no fever. I diagnosed early tuberculous meningitis, and admitted the child to the hospital. Examinations were made of the cerebro-spinal fluid, but the first two were worthless, as the fluid was blood-stained. The third examination of it, though still there was some blood, showed no increase in cells beyond what the slight admixture of blood would account for, but the majority of these were lymphocytes. Her temperature remained normal throughout. The somnolence gradually passed off in about a fortnight after admission. The ptosis has practically cleared up, and the squint has gone. I think the case falls into line with the adult cases of botulism we have seen recorded in the Press during the past two or three weeks. The second case, almost identical, was that of the boy. He also was brought with the history of falling asleep at meals and being in a very drowsy condition. He, too, was well nourished, and, but for the sleepiness, seemed well. He had ptosis and a slight squint, and his condition has cleared up in the same way as the girl's, in about a fortnight. I would have brought these patients up, but there is now nothing to see. I have made these remarks in order to find out whether other members of the Section have met with similar cases, and if so, whether they regard them as botulism, or whether they have an alternative diagnosis to suggest. In children the chief danger is in their being diagnosed as cases of tuberculous meningitis, an error I made in these two cases. Both my patients were put to bed, given a light diet, and the alimentary canal cleared out: no drugs were given.

Dr. J. D. ROLLESTON: I suggest, as an alternative diagnosis in the President's cases, polio-encephalitis. Judging from the literature, it is not so much the cerebrum as the pons and medulla which are affected in botulism, and these two cases may have been due to the unknown virus of polio-encephalitis.

Dr. PORTER PARKINSON: At present I have a case under my care in hospital, in an adult, which resembles very closely the cases the President has described. The woman was taken ill three weeks ago, a few hours after her husband had returned to the Front after leave of absence. The illness began gradually, with sleepiness, which increased, and three days later she was admitted to hospital. She was then in a drowsy condition, but could fairly easily be roused: she was intelligent, and could answer simple questions properly. Her temperature was normal, and has remained so. She had double ptosis, and paralysis of the upward and downward movements of the eyes, but there was no strabismus, and the eyes could be moved slightly laterally. She had no double vision. At first she had slight difficulty in swallowing, which has so increased that she can now only take liquids, and those with difficulty—some of the liquid trickling out of her mouth—and the taking of solids is impossible. Also, a few days after her admission, there appeared paralysis of the right side of the face, which has now become extremely marked: it seems to be of the lower neuron type, affecting the upper and lower regions of the face. In other respects she is normal: there is nothing wrong with lungs, heart or abdomen. There is no loss of sensation.

The reflexes vary: there is exaggerated knee-jerk, and well marked ankle clonus of the left foot, not of the right. Occasionally—not always—one can elicit left extensor plantar reflex. When the patient was admitted I thought she was suffering from an affection round about the corpora quadrigemina, owing to the loss of upward and downward movement of the eyes: and I thought, on account of the drowsiness, she had extensive thrombosis of some vessels in that region. But since reading the accounts of botulism, I have thought it probably is a case of that condition. I have not seen any account of affections of the knee-jerk or reflexes generally in botulism at all. On admission the urinary function was normal, but after a few days incontinence appeared, and now she has incontinence of both urine and fæces, not retention. The treatment has been more or less expectant until lately, when we have put her upon doses of strychnine. It is suggested that strychnine may be useful to ward off any bulbar paralysis, of which cases of botulism occasionally die. The cerebro-spinal fluid has been examined, and found to be quite normal. The fundi of the eyes are also normal. The Wassermann reaction of the blood was indefinitely positive.

The PRESIDENT: In reply to Dr. Rolleston's suggestion that my two cases might be cases of polio-encephalitis, the gradual onset and the absence of fever and fits constitute strong presumptive evidence against that view. Also, it was not coma that the children were suffering from, but somnolence, an exact simulation of ordinary sleep. The children would sleep for a long time, and would have to be roused, and then would go off into a similar condition again. I have not seen such an occurrence in polio-encephalitis. Inquiries were made as to whether there had been anything unusual in the diet of these children, and there was no evidence of such, nor of any particular article of diet which might have produced the condition. I think there are a good many of these cases in London at the present time, and that is the chief reason I have mentioned the matter here. I have another case in hospital, that of a woman who was attending the obstetrical department, and though five and a half months pregnant, was sent over to the medical side because she was supposed to be suffering from some form of insanity. During the last three or four weeks she had become very quiescent and drowsy: she could hardly speak. I noted an extraordinary passivity of the features, resembling an extreme degree of the Parkinsonian mask. She could barely close the eyes, and the facial muscles were uniformly weak. She is still in hospital, and is improving. When I first saw her I thought it was most probably a case of myasthenia gravis, but since hearing of the so-called cases of "botulism" occurring in London I think it likely it may prove to be a case of the same stuporous condition, though it appears to be highly improbable that this has anything to do with the *Bacillus botulinus*.

Dr. J. D. ROLLESTON: It would be useful to the Section to hear something about the treatment. One of the chief indications is to get rid of the toxic material as soon as possible by washing out the stomach. The manufacture of a specific serum is rather difficult, according to Dorendorf,[1] who says that in view of the experimental fact that diphtheria antitoxin neutralizes the toxin of botulism, it would be well to try antidiphtheritic toxin in these cases. He did that in one of his own cases, but without benefit. Still, until a serum for this disease has been made, it would be well to give antidiphtheritic serum.

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[1] *Deutsch. med. Wochenschr.*, 1917, xliii, pp. 1531-1534; 1554-1556.

Section for the Study of Disease in Children.

President—Dr. H. MORLEY FLETCHER.

Case of Anæmia with unusual Blood Count.

By J. PORTER PARKINSON, M.D.

THIS boy, aged 9 years, was admitted to hospital on March 24. Six weeks ago bruising all over the arms, shins, and back was noticed, and a week later a rash resembling flea-bites appeared on the body and neck, and has been coming out at intervals since. For a month he has been bleeding at the mouth, the lips were painful and swollen, and the gums sore. For a fortnight he has had severe headache, and was noticed to be rather pale and breathless. He had scarlet fever when eighteen months old, and "kidney trouble" and "dropsy" a year later. He had measles and whooping-cough when 6 years old, but no other illnesses. There is one other child, aged 3 years, healthy. No family history of hæmophilia.

On admission he was very pale and the face rather puffy. There was no rash except a few petechiæ on the feet. Gums not swollen, but bleeding slightly. The tonsils were enlarged and a few glands could be felt in the neck. The lungs and heart were normal, except for a pulmonary hæmic murmur. The abdomen was normal; the liver and spleen appeared of normal size. The urine contained neither albumin nor sugar. Temperature for the first fortnight rose to 99° F. in the evening, but the last week has mounted to 100° F. most evenings.

On April 25 the report on the blood was: Red cells, 1,260,000; leucocytes, 800; hæmoglobin, 20 per cent.; colour index, 0·9; polymorphonuclears, 27 per cent.; lymphocytes, 72·5 per cent.; eosinophils, 0·5 per cent. No nucleated red corpuscles seen; no marrow cells. The red corpuscles showed marked alteration in size and shape.

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[1] At a meeting of the Section, held May 21, 1918.

May 6: Red cells, 1,088,000: leucocytes, 1,200; hæmoglobin, 20 per cent.: colour index, 1·0: polymorphonuclears, 18 per cent.; lymphocytes, 76 per cent.; hyalines, 5 per cent.; eosinophils, 1 per cent. The red cells showed marked difference in size and slight difference in shape. No nucleated forms. Lymphocytes consisted almost entirely of small cells.

The cause of the profound anæmia seems obscure; there was no reason to suspect scurvy, hæmophilia, tuberculosis, or syphilis. (The Wassermann reaction of the blood was definitely negative.) Lead poisoning is unlikely. There is no evidence of intestinal parasites; microscopic examination of the stools is negative. There is a history of kidney trouble six years ago, but there are no signs of it at present.

The examination of the blood shows marked leukopenia, due almost entirely to diminution in the number of the polymorphonuclear cells, the lymphocytes being in relative excess but actually about normal in number.

Mr. Jeremy's Report on the Eyes.—Right eye: Fine vitreous opacities, edges of disk hazy. Disk swollen, + 1 D. Retina cloudy. Vessels embedded at edge of disk. Large crescentic hæmorrhage on inferior temporal artery below and to outer side of disk; small and scattered hæmorrhages along arteries at periphery of fundus. Left eye: Fine vitreous opacities, edges of disk hazy. Disk swollen and +0·5 D. Many hæmorrhages scattered along superior nasal and temporal arteries. Retina cloudy. Macula healthy in both eyes.

This suggested the possibility of some affection of the red bone marrow, and to exclude diffuse myeloma the urine was examined for Bence-Jones albumose, but this was not present. The blood does not suggest pernicious anæmia as there is no marked poikilocytosis, and no megaloblasts are seen. I am having X-ray applications over the long bones of the limbs, with the object of stimulating the action of the marrow.

DISCUSSION.

Dr. F. PARKES WEBER: This is a most interesting case, and illustrates a type of *aplastic anæmia*. It is typical, not of the acute form of that disease, but of the chronic form, which occurs occasionally (without obvious cause) in young people. It might be termed a case of "idiopathic" chronic aplastic anæmia. The red bone marrow is the only tissue that one can find at fault. For some reason or other, that has simply given up doing its work properly. Hence there is a deficiency in the blood-cells which are formed in the red bone marrow, namely, first of all the red blood corpuscles, and secondly the polymorphonuclear leucocytes. The condition of anæmia is not of the common secondary chlorotic type, nor is the colour index usually as high as in typical cases of pernicious anæmia. Naturally also there are generally no nucleated red blood corpuscles or myelocytes in the circulating blood (for the bone marrow is not reacting, as it does in ordinary cases of pernicious anæmia, &c.). Moreover, there is absence of decided poikilocytosis, anisocytosis and polychromatophilia, such as you get in the pernicious type of anæmia. With regard to the cause, in this type of case practically nothing is known. Some disturbance—a slight infection, for instance—may give rise to a destruction of red cells, which reveals a (previously latent) deficiency in the reactive power of the red bone marrow. On the other hand, this deficiency in the reactive power of the red bone marrow may be of recent origin, and may, in fact, constitute the whole disease. It seems to me that, with arsenic, iron and Röntgen rays, Dr. Parkinson has already tried almost everything in the way of therapeutic measures. The question is whether Röntgen rays may not actually further damage the red bone marrow. As a last resource it is well worth while to try subcutaneous injections of normal human blood-serum, or rather, blood, a few cubic centimetres of which could be easily obtained from a healthy person. I would not separate out the serum, but would either inject the blood immediately, so that no time is allowed for clotting, or else take care that the fibrin is removed by whipping the blood. Of course careful antiseptic precautions must be taken. The prognosis in these cases is not so bad as in acute cases, though, of course, that is not saying very much, seeing that the typical acute cases practically all die. I think there is a chance of this boy slowly improving. The advantage of simple *subcutaneous* injections of human blood is that if great antiseptic care is taken expert manipulations are not required.[1] I think it best, in the present stage of our knowledge, to add to the "aplastic anæmia" title the qualification, "so-called idiopathic." It excludes the typical secondary forms of aplastic anæmia, such as have occurred in munition workers as a result of tri-nitro-toluene poisoning.

Dr. PORTER PARKINSON (in reply): I agree with what Dr. Parkes Weber says, that the condition is aplastic anæmia, and that is the reason I spoke in the way I did as to whether X-rays would improve the function of the bone marrow. Aplastic anæmia is not, of itself, a disease, but is generally a sequel of some other condition: as far as I have read, the condition has usually been a sequel of syphilis, or kidney disease, or some other well-recognized disease. It is because the condition in this patient seemed to have come on of itself that I wondered whether it could be that rare condition diffuse myeloma, and why I tried the Bence-Jones reaction. I regarded it as anæmia due to want of action of bone marrow, of unknown causation.

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[1] *Postscript.*—Since the meeting I have had reason to suspect that injections of fresh normal human blood, even when made subcutaneously or intramuscularly, are not always harmless. Antithrombin is often present in excess in diseases of the blood-forming organs, and the injection of normal blood might harm rather than help patients with excessive antithrombin. Cf. H. G. Wells, *Chemical Pathology*, 3rd ed., Phila., 1918 p. 321.

Case of Myotonia.

By Reginald C. Jewesbury, M.D.

Girl, aged 9 years. The child was quite well until two years ago, when wasting and weakness began simultaneously in both legs. This gradually increased, and she developed such a marked degree of talipes equino-varus that tenotomy of both Achilles tendons was done at a provincial hospital. She was able to walk better for a time after this, but gradually the legs became very stiff, and the arms, especially the left one, and the muscles of the neck also became affected.

On April 2 the child was admitted to the Victoria Hospital. There was no history of any previous illness. Both parents are healthy, and there are three other children quite normal. No history of any muscular or nervous disease in the family. No evidence of syphilis in this child or in the parents.

On examination, the patient is a healthy looking child and very intelligent. Motor system: There is slight wasting of the calf and thigh muscles on both sides. No loss of power, but on attempting to make any movement the legs become very stiff. Double talipes equino-varus, especially marked on left side. She is unable to stand with the left heel on the ground. On walking the legs become very stiff; there is no inco-ordination. No ataxy nor Rombergism. Arms: Some wasting of muscles but not marked, no loss of power but rigidity becomes very marked when she tries to make any definite action. The movements are performed jerkily and stiffly. The left arm is much more affected than the right. When she tries to grasp any object with the left hand all the voluntary muscles of the body seem to be thrown into a state of spasm. Muscles of neck: There is a perpetual right-sided torticollis owing to spasm of the sternomastoid and trapezius, which becomes very marked at times. Muscles of trunk and back are in a more or less constant state of spasm. Facial muscles not affected. Cranial nerves all normal. Optic disks normal. Tendon reflexes normal when muscular spasm is relaxed; superficial reflexes normal; plantar reflex flexor; sphincters not affected. Sensibility quite normal to touch, pain, heat and cold. No tremors, no athetosis, no evidence of any visceral disease. Wassermann test of blood negative.

The case appears to be a very unusual one of some form of myotonus. There is no evidence of any organic nervous lesion. The fact that the condition developed when the child was aged 7 years, up to which time she had been quite well, and the absence of any family history, makes the diagnosis of Thomsen's disease unlikely.

The child has been treated with massage and passive movements. She appears to be able to exercise a certain amount of voluntary control of the spasm of the muscles.

DISCUSSION.

Dr. F. PARKES WEBER: I think the case is one of organic disease in the basal ganglia of the brain, and that it therefore belongs to a group which includes Wilson's family progressive lenticular degeneration. Apparently a number of various conditions have been suggested as belonging to that group.[1]

Dr. HELEN INGLEBY: The stiffness which this child presents has some resemblance to that of certain cases of paralysis agitans in which there is rigidity but no tremor. But in paralysis agitans rigidity is never present without some motor weakness in the affected limbs. If the case belonged to this group, one would expect the lesion to begin on one side, to be for a time hemiplegic in distribution, or finally to cross to the other side. Most of the progressive degenerations involving basal ganglia follow this course. In this child, apparently, the lesion began as a paraplegia and was never hemiplegic in distribution. At present there seems to be no motor weakness at all. If there is any tremor or athetosis, it is so slight as to be doubtful and is not such as would suggest disease of basal ganglia. The absence of weakness is strongly against the latter diagnosis and in favour of this case belonging to the group of myotonias.

Dr. PORTER PARKINSON: Without suggesting what this condition is, I think that at any rate it is not Thomsen's disease. There is, here, an absence of hereditary history, and an absence of the chief symptom of Thomsen's disease, that of rigidity, which passes off very soon after the movements are started. In this child, apparently, movements do not lessen the rigidity at all. The third point against it being Thomsen's disease is the fact that it is not universal: the right arm has, apparently, escaped entirely at present, and the only parts affected are the neck, legs, and, to some extent, the left arm.

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[1] On the whole, the present case seems to me an atypical one of the condition described by H. Oppenheim, in 1911, under the name, "Dystonia Musculorum Deformans" (*Neurolog. Centralbl.*, 1911, xxx, p. 1090). An earlier description, under another name, was that by Zieben, in 1910. Cf. J. R. Hunt, "The Progressive Torsion Spasm of Childhood," *Journ. Amer. Med. Assoc.*, 1916, lxvii, p. 1430. Hunt points out that the condition is closely related to, though not the same as, *Athetosis*. Cf. also H Climenko, *Med. Record*, 1914, lxxxvi, p. 1000; and Coriat, *Boston Med. and Surg. Journ.*, 1916, clxxv, p. 383.

Dr. HELEN INGLEBY: I was only putting forward the instance of paralysis agitans because the work, showing the function of those motor cells, has been done on cases of that disease. Most diseases involving the pallidal system are at first unilateral and later become bilateral. I did not say the lesions were necessarily symmetrical.

Dr. JEWESBURY (in reply): The condition is asymmetrical in distribution. It appeared to start in the legs, and now the leg and arm of the same side are involved. The left side is more affected than the right. On the other hand, the muscles on the right side of the neck are chiefly affected, so it is not a hemiplegic condition now, it is much more generalized In answer to what Dr. Parkinson said, I did not want to suggest it was Thomsen's disease. It seemed to me to be some form of myotonus, and I am afraid I rather shut my eyes to the fact that it might possibly have some nervous origin, so I am very glad to have that suggestion to think about.

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Section of Dermatology.

President—Dr. J. H. Stowers.

(*July* 19, 1917.)

Case of Circinate Erythema, possibly the "Erythème Annulaire Centrifuge" of Darier.

By E. G. Graham Little, M.D.

The patient is a business man, aged 40, of excellent physique and in good general health. He has been under the observation of Dr. Clarke, of Horley (to whose kindness I owe the case), since the inception of the disease, some five years ago. The eruption began in the summer while the patient was at the seaside, in the form of rings with a strong resemblance to ringworm. They disappeared with the application of iodine, and indeed spontaneously, but recurred apparently usually in the summer. As the patient was accustomed to spend this season at the sea, it was thought that this had some effect in the causation until the condition developed when he was at home at Horley. Repeated examinations for the fungus of ringworm were always negative, and this diagnosis had to be abandoned. The eruption was usually seen in the same sites as at present, that is, a copious distribution on the thighs, lower sacral region and buttocks (figs. 1, 2), the abdomen and the forearms. The rash at present consists of rings of erythema, but in the earliest stage these are said to begin with a pink papule, speedily spreading centrifugally as in the affection described by Darier, to form rings which may reach the size of half-a-crown or more. The skin enclosed in the involuted ring is unaltered, and as the margin enlarges the earlier vividly pink and raised edge fades and flattens. There is no scaliness to speak of, and scrapings yield very little material in contrast to what one finds with ringworm. Occasionally the initial lesion develops into a patch rather than a ring, that is to say the involution in the centre is incomplete. Itching is very moderate in degree, but fairly constant; vesication has never been noted, and the itching is not comparable with that of dermatitis herpetiformis.

The practically spontaneous retrogression, the periodicity, and the lesion seem to approximate the condition to the erythema multiforme group. But the persistence of the lesion is much greater, for individual lesions last for several weeks or even months. The Wassermann reaction is negative.

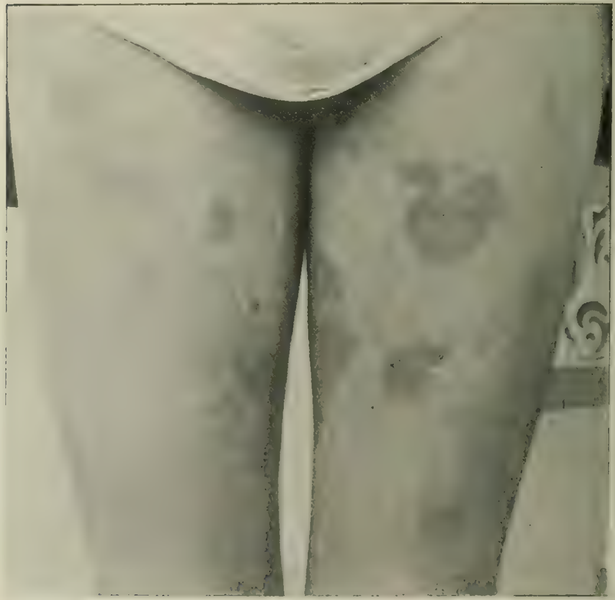


FIG. 1.

The cases of gyrate erythema described by Colcott Fox in the "International Atlas of Rare Skin Diseases," differ from these in the ultimate development of vesicles, which made Fox suggest the possibility of these being instances of dermatitis herpetiformis, and Darier takes it as established that this was their nature. The case which I showed at this Section a few months ago as possibly an example of "erythème annulaire" also developed in such a manner as to make it quite certain that it was a dermatitis herpetiformis. Itching in that case had been severe from the beginning, so that this development was perhaps not wholly unexpected.

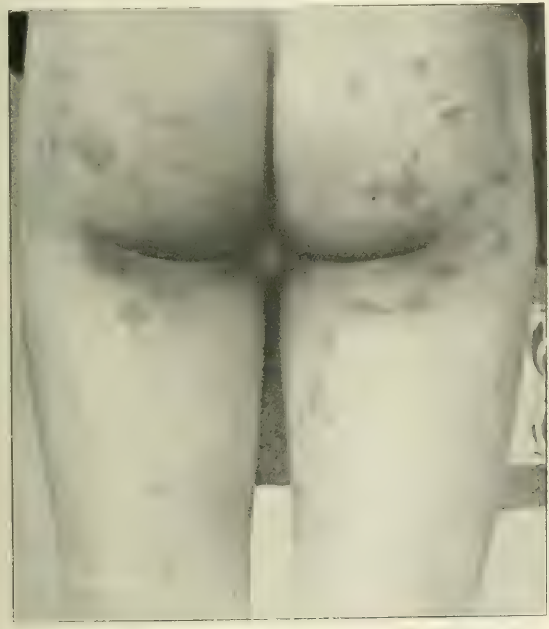


FIG. 2.

DISCUSSION.

Dr. S. A. CLARKE: The patient came to me each year immediately after his annual sea bathing. He had distinct itching, but he never visited me until the patches were distinctly circinate. I thought it was ringworm, and treated him for that, and each year it cleared up. This year he pointed out that he had not been to the sea, yet the lesions came out again. The patches have not been treated, and they have spread very much. The desquamation does not show so much now as it did. He has itching at times. Microscopical examinations have been made, but the ringworm was not found. The regions mostly affected are the buttocks and lower part of the abdomen and the upper part of the thighs. Sometimes there are one or two patches on the inside of the arms.

Dr. H. G. ADAMSON: Several years ago I showed a patient with an almost exactly similar condition, who had these rings persisting for months, and then disappearing, only to re-appear later. Previously I had a case in a child. In both my first diagnosis was ringworm; then I concluded they were the condition Dr. Fox had described as persistent circinate erythema. In both cases the eruption disappeared on taking salicylates, but came back afterwards.

(*July* 19, 1917.)

Case of Lupus Erythematosus following upon Ringworm of the Scalp in an Adult treated with X-rays.

By E. G. GRAHAM LITTLE, M.D.

THIS case presents features of social as well as of dermatological interest. The patient is a single lady, aged 37, who rather more than five years ago developed a condition of the scalp which was diagnosed as "eczema" by one general practitioner, and treated as such for a year. She was then seen by another general practitioner, who made the diagnosis of "ringworm," which he states was confirmed by a microscopical examination of hairs sent for this purpose to the Kent bacteriological laboratories. This diagnosis was supported by the X-ray operator, another general practitioner, who in his excellent notes of the procedure he adopted in X-raying, explicitly says the tinea was "diffuse and general." The patient, however, seems to be positive as to the "redness" of the parts prior to X-raying, and declares that the areas then involved were much less than at present, and that the invasion of the scalp has progressed very steadily and rapidly in the past two years, the whole vertex being now affected. She was submitted to X-rays on August 5, 1914, the entire head being exposed by the Kienböck-Adamson method, only one application being made to each of five areas marked out on the scalp. The hair fell out in due course all over the scalp, but according to the patient's recollection the permanently bare areas resulting are of much wider extent than previously to that treatment, and these areas are spreading, as has been stated. The present condition is quite typical of lupus erythematosus, and there is a fresh invasion behind the ears which is very characteristic. It is of course arguable that ringworm of the scalp in an adult being so rare, and no source of infection having being established, the diagnosis of that condition was erroneous, and that the original disease was in fact lupus erythematosus, which may well have been accentuated, but not initiated, by radiation. The patient, naturally enough, is inclined to attribute the baldness to an X-ray burn, and it is chiefly to dispel this attribution that I show this case.

REPORT OF THE APPLICATION.

E. S. Tinea Chart (Dean's).

Distribution: General, diffuse. Infected four to five years ago.

Tube number	Area	Milliampere	Interrupts	Pastille reading	Distance from anode to skin	Adapter
2,198	O.	0·6	9,000	B.	15 cm.	K.
2,198	V.	0·7	9,000 2,000 1,000	B.	15 cm.	K.
2,198	F.	0·7	10,000	B.	15 cm.	K.
2,198	R.T.	0·7	10,000	B.	15 cm.	K.
2,198	L.T.	0·6	10,000 1,000	B.	15 cm.	K.

DISCUSSION.

Dr. G. **PERNET**: I had a similar case, an older (female) patient. It was diagnosed as ringworm, but I am not sure that it had been microscopically examined. It was undoubtedly a case of lupus erythematosus, and it had been treated by X-rays, which certainly aggravated it. The case was a very obstinate one. I doubt if a microsporon was originally present in Dr. Little's case, notwithstanding the report.

The **PRESIDENT**: This is a case of lupus erythematosus.

Dr. **GRAHAM LITTLE** (in reply): The patient says the sitting under the rays lasted three and a half hours, and the detailed report shows that small areas were separately treated during that time, and that skill was used.

(*July* 19, 1917.)

Small-spored Ringworm in a Woman, aged 37.

By S. E. Dore, M.D.

This patient was referred to me for X-ray treatment by Dr. Abraham, who tells me he has seen five similar cases in adults. The question arises, particularly after our seeing the case which Dr. Little has just shown, as to what the treatment should be. She has asked for X-ray treatment, and I was prepared to adopt this method. The case, however, seems to be improving a great deal under the present treatment with ointment, and my experience, in the few cases I have seen in adults, is that the condition clears up in them more quickly than in children. Another important point is whether there is more risk in X-raying the scalp of a patient of this age than that of a child. I have applied the rays to a certain number of cases of alopecia areata in adults and the hair has returned satisfactorily, at any rate on the places not affected by the alopecia. I have not yet had an opportunity of making cultures, but under the microscope there is a stained specimen of a hair showing small-spored fungus, and Dr. Abraham says when he saw her, the top and sides of the scalp were a mass of stumps.

DISCUSSION.

The President: I fully agree with Dr. Dore's diagnosis and views of the case. It would be instructive to know whether he has been able to trace the source of infection. Although admittedly rare in practice, I have no doubt that this disease is occasionally overlooked owing to the erroneous impression among many that "ringworm" does not occur on the adult scalp.

Dr. Dore (in reply): The patient attributes the disease to handling dirty pay-books, which the soldiers often carried in their caps, in an Army Pay Office about three months ago, but that is scarcely a likely source of infection. There appears to be no definite history of infection from animals, although there was a suspected cat in the house, which was examined and found to be healthy. An interesting point concerns the duration of the disease, some authorities maintaining that these are really undiagnosed cases which have persisted from childhood. There is evidence that this was not so here, because the patient consulted Dr. Adamson five or six years ago for loss of hair, and his diagnosis was seborrhœa of the scalp.

(*July* 19, 1917.)

Photographs of a Severe Case of Congenital Ichthyosis.

By J. H. STOWERS, M.D. (President).

THESE are photographs of the most severe case of congenital ichthyosis I have ever seen. The patient (male), aged 51, presents

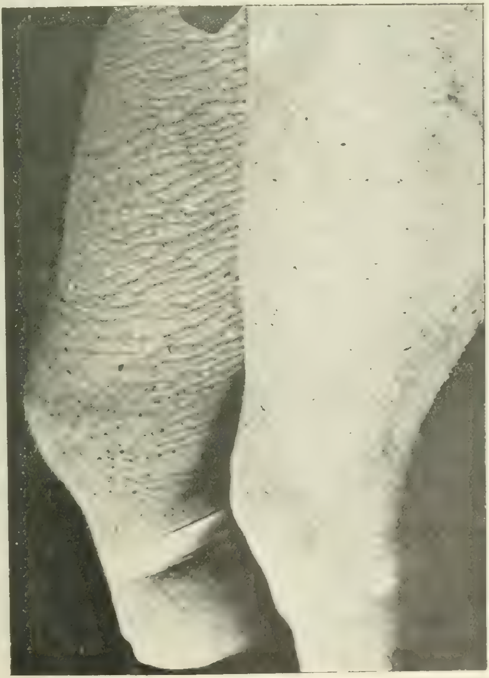


FIG. 1.

an unusual tesselated appearance upon the limbs, with exceptionally large elevated plates of thickened epidermis-like crocodile hide (ichthyosis sauroderma) (figs. 1, 2).

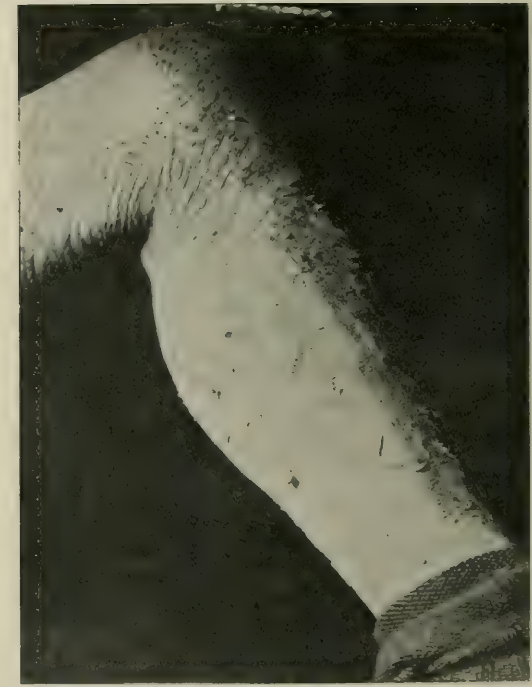


FIG. 2.

(*July* 19, 1917.)

Case of Multiple Morphœo-sclerodermia.

By George Pernet, M.D.

The patient is a girl, aged 11, and the changes in the skin were noticed six weeks ago. Now, over the lumbar region and the scapulæ, there are faintly violaceous areas with here and there oval patches of slightly ivory-white sclerosed glistening skin, bordered by pale lilac rings. These oval patches are inclined to be symmetrical about the back.

(*July* 19, 1917.)

Case of Extensive Nævus of the Trunk and Extremities, involving the Palms and Soles.

By George Pernet, M.D.

The patient is a boy, aged 6. According to the mother, she first noticed the condition of the skin a week or two after birth. The nævus occupies a corset area of the trunk and upper part of the thighs and buttocks, the fronts of the knees, with a boot and glove-like distribution about the feet and hands, the soles and palms being involved. The skin in this area is dry and rough to the touch, with striations, giving a curious variegated appearance, especially in front. The fronts of the knees exhibit a rough patch on each. The backs of the hands and dorsa of the feet are slightly affected, but the palms and soles are rather dirty-looking and rough, like a keratosis. Running up about the tendo Achillis are somewhat rough and red narrow linear bands, like a mild degree of nævus linearis.

Altogether the nævus is of a mildly verrucose type. Quite recently faintly pink scaly patches have appeared about the right elbow and under the left clavicle. On scratching up the scales bleeding points can be made out pointing to a mild degree of psoriasis, which some would perhaps consider seborrhœic, especially as the scalp is dry and scurfy. In any case these two patches are on the borderland of the aforenamed conditions. The boy is in good health and well-nourished. There are no indications of syphilis in either mother or child to account as a factor in the congenital malformation of the skin. I may add that the Wassermann reaction in both was negative.

(*July* 19, 1917.)

Case of Acne Varioliformis.

By George Pernet, M.D.

A woman, aged 34, a railway car cleaner, who has had an eruption about the border of the scalp and forehead for seven months. There the elements are characteristic of acne varioliformis, the necrotic lesions going back into the scalp in the usual way. More recently she has had a similar roughly triangular area, with the base upwards, of similar elements on the centre of the back. There is also a small group on the centre of the chest in front. It is for this unusual distribution that the patient is shown. She is improving on mist. ferri perchlor. alone, without any local treatment.

(*July* 19, 1917.)

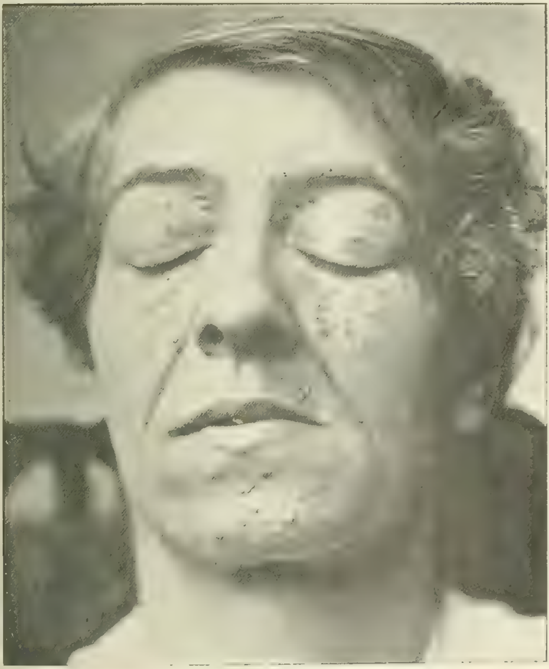
Case of Acne Agminata (of Radcliffe-Crocker).

By George Pernet, M.D.

The patient is a woman, aged 49, with a typical acne agminata eruption of the face, as is well brought out by the photograph. The eruption has lasted about five months. I do not remember ever seeing acne agminata in a woman before. In the cases I have seen exposure to heat, or strong sunlight, and accompanied by sweating, were the rule. There was no exposure of the kind in this instance. I examined one of Radcliffe-Crocker's cases[1] histologically, and found that the sweat apparatus was mainly involved. I do not consider these cases are tuberculous in origin—i.e., toxi-tuberculides. Quite recently Chalmers and Martyn, of Khartoum, agreed as to the pustular lesions with my histological results.[1] Nor do I consider these cases the same as acne varioliformis, though I notice Stelwagon includes acne agminata under the acne varioliformis heading.[2] They are, no doubt, toxic in origin in some way, and in the present case the state of the teeth and gums would be quite sufficient to account for the outbreak on the face. The patient has improved a little since she has been on lot. calaminæ and mist. ferri perchlor., as when she first came under observation on June 5 last, the eyes were very puffy, and the face somewhat swollen generally

---

[1] Pernet, *Brit. Journ. Derm.*, xiv, 1902, p. 131. *See* also Radcliffe-Crocker, "Diseases of the Skin," 3rd ed., ii, fig. 70, p. 1096.



Acne agminata.

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[1] A. J. Chalmers and A. C. F. Martyn, *Proc. Roy. Soc. Med.*, 1916-17, x (Sect. Derm.), pp. 23 *et seq.*

[2] Stelwagon, "Diseases of the Skin," 7th ed., p. 1055.

(*July* 19, 1917.)

Four Cases of Dermatitis due to High Explosives used in the recent Air Raids.

By J. H. Sequeira, M.D.

Two of the cases are the result of the raid on June 13. Both patients have suffered severely from a pompholyx type of eruption, beginning nine days after exposure to irritation. The hands were intensely stained an orange-yellow colour immediately, and vesicles developed on the palms and sides of the fingers on the ninth day. In both the bullæ became confluent, and ultimately the epidermis peeled off. Both have had slight pyrexia and eosinophilia. The other cases, caused by the raid of July 7, illustrated the staining of the skin and early vesication.

The powder which is the cause of this dermatitis in under investigation. I have had private information that it is not only tri-nitro-toluene but that there is hexamethyl-diphenyl-amine in addition. It is something fresh in explosives.[1] Hexamethyl-diphenyl-amine is also known as aurantia, which has long been recognized as a cause of dermatitis.

(*July* 19, 1917.)

"Accladiosis."

By Aldo Castellani, M.D.

(*Lieutenant-Colonel, Royal Italian Medical Service, Navy, Balkanic Zone.*)

Cases of accladiosis having been very recently observed in Europe (Balkanic Zone) by myself, Laurie, and others, it may perhaps be of interest to give a brief general account of the condition.

Historical and Geographical Distribution.—The condition has been observed by me since 1907 in Ceylon, but I did not fully describe it until last year. Cases have been observed in Ceylon, the Federated Malay States, and Macedonia.

---

[1] A fuller description of cases is given in the *British Medical Journal*, August 4, 1917, p. 148.

Clinical Symptoms.—In a well marked case ulcerative lesions are present all over the body, though they are in smaller number or absent altogether on the face, scalp, palms, and soles. Most of the ulcers are sharply defined, roundish or oval, with red granulating fundus. Their appearance is well shown in the illustration (fig. 1), a photograph of a Ceylon case. In many cases there is abundant purulent secretion which collects and dries up in thick yellow crusts covering the ulcers. Gummata-like nodules and furuncle-like lesions may also be observed. The superficial lymphatic glands may be enlarged. The lesions in most cases give very little pain, or none at all; itching is often completely absent, but occasionally the patient complains of slight pruritus. The general condition of the patient is not seriously affected for a long time, but he often complains of a certain degree of weakness and discomfort. Not infrequently there is serotine fever. The blood has been examined in two cases in the Tropics; in one case in the Balkanic Zone: Wassermann reaction negative. In the first two cases red blood corpuscles and hæmoglobin were slightly below the normal; in one there was eosinophilia (5 per cent.) which may have been due to a concomitant *Ascaris lumbricoides* infection. In the Macedonian case, the blood of which was examined, there was a distinct leucocytosis (16,000 leucocytes) of the polymorphonuclear type; in this patient there was abundant purulent secretion and serotine fever, which on some days reached 102° and 103° F.

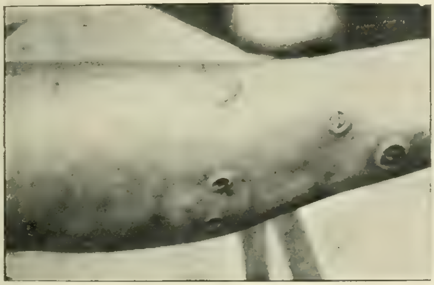


FIG. 1.

Case of accladiosis.

Histo-pathology.—The histo-pathological investigation of the condition is far from completed. From the preliminary investigation it would seem that the lesions are very similar to those one sees in sporotrichosis, and that three types of lesions may be distinguished: (1) An epithelioid or tuberculoid type, with presence of giant cells; (2) a lympho-connective tissue type (syphiloid); (3) polymorphonuclear type (ecthymatous).

Ætiology.—The condition is caused by a fungus, which, as stated, I isolated in Ceylon. Cultures were sent to Professor Pinoy, of the Pasteur Institute, who very kindly investigated it botanically and classified it, giving it the name of *Accladium Castellani* (Pinoy, 1916). I may quote a portion of Professor Pinoy's description:—

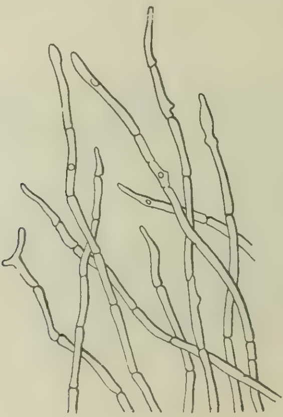


FIG. 2.

Microscopical appearance of the fungus in hanging-drop culture, twenty-four hours old.

> "The growth on artificial media (such as carrot, potato, glucose agar) consists of many small roundish masses, which later on may coalesce, covered by spiculated formations, giving them a prickly appearance, and consisting of erect, straight filaments, parallel to each other, or at times interlacing. These filaments are approximately 2 microns in diameter, and carry laterally pseudoconidia of variable shape, cylindriform, pyriform, or spherical, attenuated in size at their points of insertion. Most of these pseudoconidia are 4 microns

in length with a breadth of 3 microns. This type of fructification recalls the type Accladium described by Bodin in certain species of the genus Trichophyton (Malmsten, 1848).

"These pseudoconidia become detached and then develop by sprouting, and mycelial filaments are formed. Certain filaments produce spherical chlamydospores arranged in small strings, as found in certain fungi of the genus Fusarium. These small chains of chlamydospores are very frequently terminal, the dimensions being variable—8 to 10 microns" (figs. 2-4).

In cultures on carrot and potato the colonies are white, on glucose agar often amber colour. Very old cultures may show a certain amount of pigmentation (figs. 5-9).

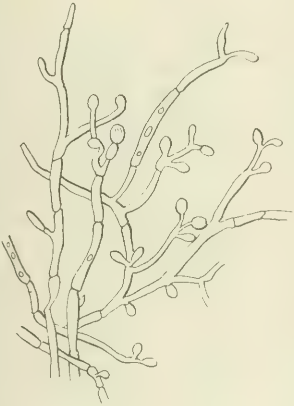


FIG. 3.

Microscopical appearance of the fungus in hanging-drop culture, three days old.

Diagnosis.—A positive diagnosis can be made with certainty only by cultural methods. The microscopical examination alone is of very little use, hyphomycetic elements being as a rule absent microscopically in the scrapings from the ulcers and contents of nodules. The material should be inoculated in glucose agar tubes. Four to eight days after inoculation small, yellowish, amber-coloured colonies appear; they enlarge fairly rapidly, become hemispheric and often coalesce in a knotty mass. At times the colonies may not fuse together; each colony then remains separate, reaches a large size, and occasionally presents peculiar radiating furrows as seen in certain species of trichophytons. In many cases where the material has been collected from ulcerated lesions, the fungus grows in symbiosis with a coccus and it may be difficult to separate the two organisms. The malady is often taken for a syphilitic condition. The history, the negative examination of the lesions for spirochætes, the failure of mercury and salvarsan treatment, will exclude it. When the lesions are covered by raised, thick, bright, yellow crusts the condition must be differentiated from yaws: in accladiosis, on removing the crusts, ulcers are found, while in yaws, the typical frambœsiform nodules will be seen; in scrapings from yaws lesions the *Spirochæta* will be found. Accladiosis can be differentiated from sporotrichosis and other affections of hyphomycetic origin by cultural methods.

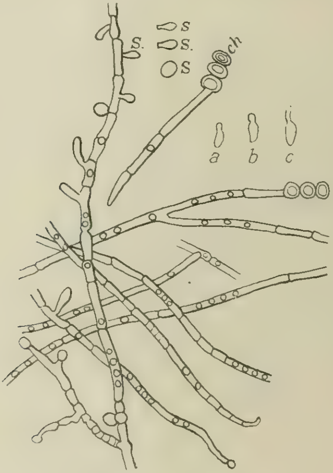


Fig. 4.

Microscopical appearance of the fungus in hanging-drop culture, five days old. *S*, pseudoconidia; *a*, *b*, *c*, development of mycelial filaments from pseudoconidia; *ch*, chlamydospores.

Prognosis.—The course of the disease may be very long, and there is very little or no tendency to spontaneous cure; but if a proper treatment is carried out a cure can be obtained fairly rapidly in the majority of cases. A few cases respond to treatment extremely slowly.

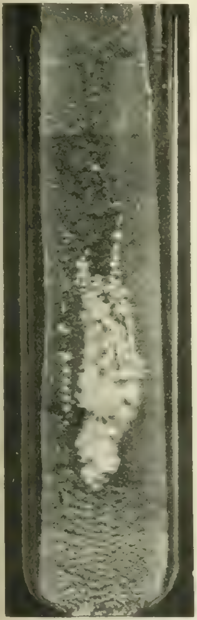


Fig. 5.

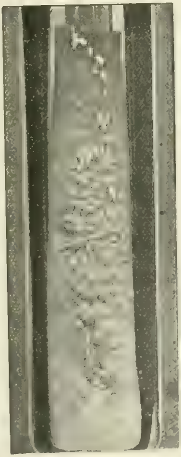


Fig. 6.

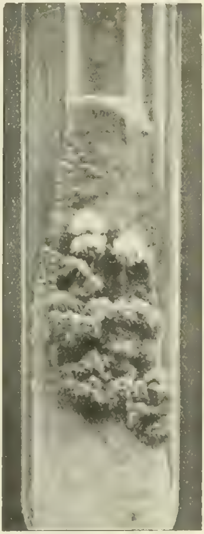


Fig 7.

Fig. 5.—*Acladium Castellanii* (Pinoy); young culture on carrot.
Fig. 6.—Young culture on potato.
Fig. 7.—Old culture on potato.

Treatment.—Potassium iodide given in full doses (20 gr. t.d.) acts satisfactorily. The drug appears to act at times more rapidly if given according to Professor Pinoy's method—viz., in conjunction with a salt-free diet. If potassium or sodium iodide is not well borne, sajodin and other similar preparations may be tried, but the result is not so satisfactory. Mercury and arsenic have no effect on the course of the malady. As regards local treatment, it is sufficient to keep the ulcers clean by using a weak mercury perchloride lotion.

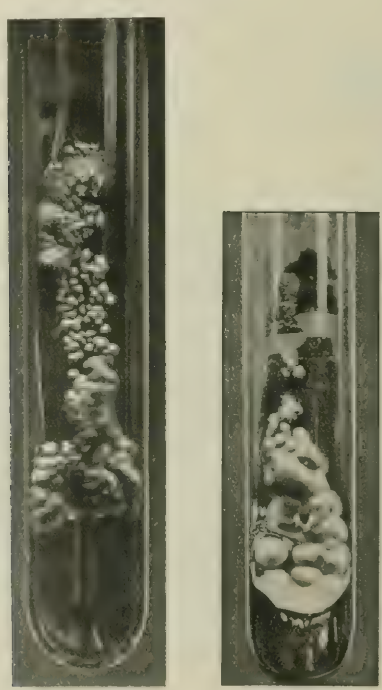


FIG. 8. FIG. 9.

Cultures on glucose agar.

REFERENCE.

CASTELLANI, ALDO. "Notes on a New Ulcerative Dermatomycosis," with Report on the Causative Fungus by E. PINOY, *Brit. Med. Journ.*, October 7, 1916, p. 486.

Section of Dermatology.

President—Dr. JAMES GALLOWAY, C.B.

(*October* 18, 1917.)

Chairman—Dr. J. H. STOWERS.

Case of Sclerodactyly.

By J. H. SEQUEIRA, M.D.

THIS girl is now aged 12. She came to London Hospital in 1914, when she was aged 9, suffering from small-spored ringworm of the scalp, and while she was under treatment there, Dr. Oliver, who was in charge of the ringworm cases, noticed the condition of her hands, and brought the patient to me. We recognized the case as one of sclerodactyly. You see a very marked degree of sclerodermia, beginning at the extremities—the hands and feet—and the face has the "marble statue" appearance which is characteristic. There is little of importance in her history, but the condition is supposed to have dated from a fright. She has had chicken-pox, scarlet fever, and measles. She has not had rheumatic fever. She has also suffered from herpes zoster, and, as far as we can make out, the scars of the zoster are the only alterations in the skin of the abdomen and the chest. So that the examination should be complete, a Wassermann test was done, and it was negative. A blood count showed an abnormality in the lymphocytes, 18·5 per cent., between twice and thrice the normal proportion. She has been given thyroid and various other treatments, but, I regret to say, without any marked improvement. Recently she has had a little sepsis over one of the knuckles, the skin over it being very tight. Can members suggest a treatment which is likely to ameliorate the condition?

DISCUSSION.

Dr. F. PARKES WEBER: It is a remarkably fine example of sclerodactylia associated with sclerodermia of the face, occurring at an unusually early age. Dr. E. A. Cockayne has recently collected the records of cases of the condition at a still earlier age: there are one or two undoubted cases in which the disease has been present at birth. In cases of this disease, when either acro-asphyxia or so-called "local syncope" of the fingers is observed before the diagnosis of sclerodactyly has been made, one should regard such symptoms as early symptoms of the disease, and not as representing a combination of Raynaud's disease and sclerodermia. One should also, I think, not speak of sclerodactylia as "supervening on a basis of Raynaud's disease."

The CHAIRMAN: My experience of these severe cases, which are not common, is that beyond palliative treatment little can be done for them. The last patient I saw who was similarly affected, after years of disablement and suffering, died from pulmonary complications.

(*October* 18, 1917.)

Case of so-called Multiple Idiopathic Pigment Sarcoma of Kaposi.

By J. H. SEQUEIRA, M.D.

THIS patient is a man of Russian origin, aged 40. When he was first seen by me he had a purplish eruption, of papular character, which suggested lichen verrucosus. The history given was that he had had the trouble two years. His hands show well the "purple congestion" described by Hutchinson, while the lower extremities show infiltration of a purplish colour with numerous small nodules. The fungating mass on the leg is a tumour which has, apparently, arisen after the excision of a piece of skin for microscopical examination. The Wassermann reaction is negative. My last case, which I showed at the International Congress, 1913, I have had steadily under my care, and great benefit followed treating it by means of X-rays: the swellings went down, and the skin became fairly normal in colour; the patient is also more comfortable. I propose to employ the same treatment in this case.

P.S.—Since the case was demonstrated the microscopical examination has shown the excised skin to be the seat of a granulomatous infiltration. I think the time has now come to exclude these cases from the group of sarcomata.—J. H. S.

(*October* 18, 1917.)

Case of Acne Scrofulosorum.

By E. G. Graham Little, M.D.

This is an extensive eruption of acne scrofulosorum in an obviously tuberculous boy, the condition having an unusually long history. The eruption dates from the age of 7, and he is now 14. A new eruption has occurred on the nose within the last two months. The question is whether that eruption is an extension of the tuberculide, or whether it is commencing acne vulgaris. Besides the skin condition, he has chronically swollen joints, and enlarged glands in the neck. The face condition is the same as on the other parts. The present distribution is chiefly on the legs, thighs, hands and forearms, where there is an extensive closely aggregated eruption of typical follicular papules and pustules. The extremities are blue and cold, and the boy is ill developed for his age. There is chronic alopecia areata as well, of many years' duration, the scalp presenting numerous bare areas the size of a florin.

(*October* 18, 1917.)

Case of Ulcus Molle Serpiginosum.

By E. G. Graham Little, M.D.

This is almost a repetition of the case I showed eight or nine months ago of the same disease, that having been the first instance of the condition which had been shown before this Section. The history in the present case is similar: a bubo developing in the left groin five years ago, was followed by chronic ulceration, spreading along his left side upwards and downwards. The extent of the ulceration is very remarkable—namely, upwards to the waist line; downwards to the knee. This man has been under the inoculation department of the hospital for some months, where several attempts have been made to grow the bacillus of Ducrey. The attempts to grow it failed, but it was thought bacilli had been seen in the pus. For several years he had been treated at the Lock Hospital for syphilis, though the Wassermann reaction had been negative all the time, and there was no local improvement. At present there is active ulceration at the lower border of the affected area, near the knee; on the rest of the site affected healing has taken place, with the characteristic scarring; between the two large plaques of scarred tissue on the thigh and the abdomen respectively, there is a small strand, about half an inch thick, of unaltered skin, like an isthmus connecting two continents on an atlas.

DISCUSSION.

Major GRAY: All the cases of this type which have been reported have arisen from a ruptured bubo, and is Dr. Little able to tell us whether this condition has ever started from the original soft sore? One constantly sees phagedæna starting from a soft sore, and I wonder whether this is really the same condition. Perhaps it is because phagedæna attracts more attention, and so gets more vigorous early treatment, that we do not see it spreading over the body, though I have seen one case in which the whole penis was destroyed and a large serpiginous ulcer covered the whole pubic region.

Dr. J. J. PRINGLE: I regard Major Gray's observation as of capital importance. None of the cases which have been reported have—as far as I know—started from the original ulcus molle, but from the bubo. I have seen four in a comparatively short time, and about a year ago I had a case under my own care at Middlesex Hospital in which this process began and extended for only about two inches around the bubo which had been incised. Fortunately the ulcerative condition was arrested, probably spontaneously. Such a happening suggests to me that slight degrees of this serpiginous process may not be very rare. I cannot help thinking that in this case there are two separate areas of disease starting from different foci, for I cannot see any continuity between the large area of disease on the abdomen and upper thigh and that on the lower part of the thigh: there seems to me to be a zone of perfectly healthy skin between the two.

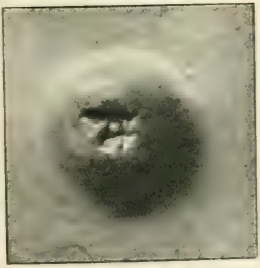
Dr. GRAHAM LITTLE (in reply): The history of the three cases I have had has been consistently the same: a bubo had burst and ulcerated, and the ulceration was very chronic, shading insensibly into the more superficial ulcers which you see. An initial and fairly extensive ulceration directly spreading from a bubo is almost essential. In answer to Dr. Pringle, I think the condition spread upwards to the abdomen and downwards to the thigh from the bubo in the groin.

(*October* 18, 1917.)

Case of Primary Syphilitic Chancre of the Umbilicus.

By GEORGE PERNET, M.D.

THE patient is a young man, aged 24, a discharged soldier, who was referred to me from the medical out-patients' department, West London Hospital, on August 28, 1917. He had previously presented himself to a V.D. clinic, and was told "he had come to the wrong place." I found a prominent dome-shaped lesion, with a depressed reniform ulceration occupying the centre. The latter had broken down in its central part and purulent fluid could be readily squeezed out. The lesion was over an inch in its basal diameter and raised quite half an inch above the



Primary syphilitic chancre of umbilicus.

level of the skin. The sore had commenced five weeks previously and was very indurated. My diagnosis was primary chancre. This was confirmed by further investigation: marked inguinal and especially femoral adenitis. The post-sterno-mastoid and submaxillary glands were also typical. There was no axillary adenitis. The tongue was ulcerated, as also was the posterior pharynx. There were small pigmented patches about the neck and trunk (involuted secondary syphilides). There were also secondary syphilides on the left palm, but none on the right. The right hand was *en griffe*, showing muscular atrophy, as did also the right forearm, a result of a wound of the arm involving the nerves. About the anus there were exuberant condylomata. There was nothing about the genitalia. The diagnosis was obviously syphilis. But to convince the sceptics, the umbilical sore was examined in the laboratory for treponemata (by Dr. Sydney Graves), and these were readily demonstrated. Moreover, the Wassermann reaction was found to be + +.

A primary chancre of the umbilicus is rare, especially in a man, and it is the first one I have had under my care, though I have seen primary chancres of the abdominal and pubic regions. In addition to its rarity, the case is instructive in other ways. The photograph, done on the same day that I saw the patient for the first time—viz., August 28, 1917, brings out the raised relief appearance very well. The patient has had three intravenous injections of galyl, and also mercury, with great benefit. The primary sore has involuted to a great extent, but some induration can still be felt.

Dr. J. H. SEQUEIRA: This is an exceedingly rare condition, but, curiously, I saw one at the London Hospital in a man aged 52, on October 2, 1917. The primary gland trouble in his case was in the axilla. There were also symptoms of secondary syphilis—viz., general adenitis, condylomata and mucous patches in the mouth and throat. The Wassermann reaction was positive. There is a good coloured photograph of umbilical chancre in the St. Louis Hospital atlas.

(*October* 18, 1917.)

Case of Adenoma Sebaceum: Pringle Type.

By E. G. Graham Little, M.D.

This girl shows a perfect example of adenoma sebaceum of the type named by our French colleagues after Dr. Pringle. It is of congenital origin, and associated with fibroid tumours on the back of the trunk, a common association. Over the sacrum there is a patch about the size of the palm of the hand, covered with the characteristic pale fibrous tumours. She also shows the mental backwardness typical of the disease. She is in a special school for backward children. She is aged 14, and is in Standard IV. There is a history of fits, probably epileptic, between the ages of 2 and 4; in a family of eight she is the only one who has such symptoms.

DISCUSSION.

Dr. J. J. Pringle: With regard to my name being associated with this "type" of adenoma sebaceum, it is an extremely graceful compliment, but, like many compliments, it is quite undeserved. The vascular or telangiectatic type cases—three in number—which I described in my paper in 1890[1] were all borrowed from French sources. The first two were observed by Vidal and fully reported in the descriptive catalogue of the St. Louis Hospital Museum. The third was narrated to me by my friend Dr. Hallopeau in a personal communication, and the model was in the same museum, although apparently undescribed in the catalogue.

The Chairman: I have seen a case recently in private practice but, as has been said, this disease is more often met with in hospital patients. The case I showed here last year was the daughter of a labourer. Treatment by electrolysis is a tedious method if the lesions are numerous, but I have had good results with CO_2, and also from the repeated application of liquefied carbolic acid, which I should recommend in this case.

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[1] *Brit. Journ. Derm.*, 1890, ii, p. 1 *et seq.* It is interesting to me on re-reading the article to note that the classification of adenoma sebaceum as a form of "nævus" was discussed and approved.

Dr. F. Parkes Weber: I understand that this patient has had epileptic attacks, besides being mentally backward. That suggests the bare possibility that she may have the very rare condition known as tuberous (or so-called tuberose) sclerosis of the brain, as well as adenoma sebaceum of the face. In every case of tuberous sclerosis of the brain there has been adenoma sebaceum present likewise, I believe. I suppose that tuberous sclerosis is to be regarded as a condition in the brain of congenital or "developmental" origin, analogous to the fibrous thickening in the skin, which is present over the lower part of the back in Dr. Graham Little's patient.

Dr. Graham Little (in reply): This patient has not had any fits since her fourth year. The opinion as to the disease being rare in well-to-do people is sound. I have not seen a case in private practice. I have told the patient's people that the only method of treatment is to attack each tumour separately, and as that is a tedious process, I do not propose to treat her at all.

(*October* 18, 1917.)

Two Cases of Morphœa Guttata.

By S. E. Dore, M.D.

These two young women present the same type of morphœa, known as the guttate type or "white spot disease." In the first patient, aged 21, the lesions first appeared when she was aged 3, immediately after an attack of measles, and have gradually increased in number since that time. There are now numerous small, well defined, more or less circular, chalky-white or silvery macules and atrophic patches, varying in shape and size, scattered about the clavicular, sternal and posterior scapular regions. The small white spots are comparatively few in number, and for the most part situated about the clavicles. At the lower end of the sternum there is a small group of pea-sized atrophic, smooth depressed areas without the peculiar snow-white appearance, and over both scapulæ posteriorly there are a few chalky spots, but most of the lesions consist of oval macules, larger than those on the chest, and having a somewhat linear arrangement from above downwards, looking as if they had been produced by excoriations with the finger-nails. One or two of the lesions on the back are covered by a crust, which appears to represent an intermediate stage between the first white morphœic and the final atrophic stage. The evolution of the lesions is evidently extremely slow and cannot be traced by the patient.

In the second case, a woman, aged 35, the lesions have only been present a year, and are much fewer in number. They take the form of a largish patch above the left clavicle, evidently due to the coalescence of small macules, several macules at the sternal end of the clavicle and a collection of small, rather more scattered, spots on the back of the neck. The macules in this case have the same sort of "mother-of-pearl" appearance, but not so well marked, and they seem to be more superficial, lacking the appearance of being let into the skin as in the other patient. None of them shows atrophy or crust formation.

The name "white spot disease" has been applied to more than one disease, and the first case shows how easily idiopathic macular atrophy may be confused with the atrophic stage of guttate morphœa. The term has also been used for some cases of lichen planus sclerosus, but Montgomery and Ormsby, from an analysis of the published cases, point out that the term is correctly applied only to the guttate variety of morphœa. The distribution in my cases about the clavicular and sternal regions and the back of the neck is very characteristic, but in 1904 Dr. MacLeod showed a case in which the lesions were situated on the abdomen. A point of interest and possibly of importance in the history of my two cases is the fact that both suffered from measles, and in one the attack directly followed that disease. In looking up the recorded cases I notice that nearly all the patients have a history of measles, and in some a severe attack is mentioned, although no special stress is laid upon it by the observers. It seems to me possible that the eruption of measles might be a predisposing factor.

DISCUSSION.

The CHAIRMAN: Some of the lesions appear to be aggravated by secondary microbic invasion. The general conditions might be treated with advantage by the continuous galvanic current.

Major GRAY: One of the patches in Dr. Dore's first case has got a definite hæmorrhage in it: I do not know whether that has been commonly noted in the guttate type of sclerodermia. Some years ago I had a patient, an old lady aged about 80, with very numerous patches of this type of sclerodermia on the chest and the abdomen. A large number of the lesions had hæmorrhages in them, and they were scaling very profusely.

Dr. DORE (in reply): I do not regard the formation of scales or crusts in the first case as due to an infection by organisms, but to a stage of the disease leading to atrophy. I have tried X-ray treatment in ordinary morphœa but without much success and there is a danger in continuing the treatment too long. I will try to obtain a section, but I should like to point out that Montgomery and Ormsby worked out the histology of their cases and found it to be that of ordinary morphœa.

Section of Dermatology.

President—Dr. JAMES GALLOWAY, C.B.

(*October* 18, 1917.)

Chairman—Dr. J. H. STOWERS.

Erythema Elevatum Diutinum.

By J. L. BUNCH, M.D.

THE patient, a woman, aged 31, first developed some raised erythematous lesions on the knuckles eight months ago. These patches spread to the dorsa of the hands, in a somewhat linear distribution, and to a small extent to the forearms. The lesions are now present on the knuckles and backs of both hands; they are red and raised, and in places tend to have an annular appearance. A few scattered papules are present on the forearms. There is very little irritation. An alternative diagnosis of granuloma annulare has been suggested, but the linear distribution of so many of the lesions does not quite seem to conform with such a diagnosis.

Pathologist's Report.—A section excised from the back of the hand shows a thickening of the stratum corneum with the stratum granulosum well marked and increased. The stratum mucosum is thickened and prolongations are present in places. In the dermis there are a number of small cellular masses which are composed of lymphocytes, plasma cells and a few mast cells. The blood-vessels in this region are distinctly dilated and surrounded with cells.

Postscript.—Treatment for six weeks has modified the appearance of the lesions to some extent, inasmuch as the colour is not so bright, but some of them are still quite raised and "lumpy." There is very little burning or irritation now.

(*November* 15, 1917.)

Case of Lymphangioma Tuberosum Multiplex.

By George Pernet, M.D.

The patient is a married woman, aged 34, with a number of small nodules, embedded in the skin, scattered about the front of the upper part of the chest in the characteristic area. They began to appear twelve years ago on the front of the neck, and thence spread to the chest. There are also a few similar lesions about the upper abdominal region. The back is practically free. The lesions, which are typical, are pinkish, firm to the touch, and become pale on pressure. I have used the old designation, "lymphangioma tuberosum multiplex,"[1] because the condition was first so described and appears to be best known by that name, although it is a misleading one. Another name, "nævi cystepitheliomatosi disseminati," agrees best with the histology,[2] but it is unwieldy. Syringoma and syringocystoma would be better perhaps, the nævus origin being understood, as first suggested by Besnier in the name he gave, viz., "cystic epithelial nævi."

As to treatment, a case of Radcliffe-Crocker's under my care I curetted under an anæsthetic,[3] and the result was good and permanent. At any rate four years later the patient's doctor informed me she had remained well. In the present case, I propose to try X-raying for the front of the neck before suggesting curetting.

The Chairman (Dr. Stowers): I consider the application of CO_2 would be a less formidable method of treatment than curetting and likely to produce satisfactory results.

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[1] *Vide* Radcliffe-Crocker's article and coloured drawing in *Clin. Soc. Trans.*, xxxii, 1899, p. 154, plate viii.

[2] *Vide* Pernet, *Brit. Journ. Derm.*, xix, 1907, pp. 67 *et seq.*

[3] Pernet, *vide supra.*

(*November* 15, 1917.)

Case of Granuloma Annulare.

By J. M. H. MacLeod, M.D.

The patient, who is a woman aged 42, in good general health, has suffered from the eruption for six months. The lesions vary from small nodules to typical rings of granuloma annulare and are present on the backs of the hands, knees, ears, and buttocks. Certain of the smaller nodules have involuted, leaving a depressed cicatrix. The larger lesions are all annular, with a whitish flat border, which in certain of them is clearly made up of the coalescence of small nodules. The lesions are definitely painful in some instances and in others are associated with sensations of burning or pricking. In addition to the granuloma annulare she has definite symptoms of rheumatism with stiffening and thickening of some of the joints of the fingers. My reason for exhibiting the case is on account of the intensity of the subjective symptoms, as in other cases which have been under my observation these have been comparatively slight.

DISCUSSION.

Dr. Graham Little: This closely recalls one of my first cases, that of a man who had an extraordinarily itchy eruption, so irritating that it kept him awake at night. He also had rheumatic symptoms, and sweated very freely. In the diagnosis of granuloma annulare I lay much more stress on the nodule than on the ring; I think the nodule here is characteristic of granuloma annulare. I have had cases in which there have been painful symptoms, in this acute stage, especially on the knuckles.

Major Gray: The condition in the present patient is almost identical with that of a patient Dr. Bunch showed at the International Congress of Medicine in 1913. It is of about the same degree of acuteness. I consider that this type of case is intermediate between the ordinary chronic cases of granuloma annulare and those very acute cases of which I showed an example before this Section,[1] also at the International Congress, and labelled it "chronic persistent erythema." The lesions in that case came out very much as they do in this case, but were much more numerous, came out more quickly, and there was a tendency to very marked vesicle and bullous formation. I think all these cases belong to the same group, but are different types. I also think that the cases which Crocker described as erythema elevatum diutinum belong to the same group. The latter group was said to be associated with rheumatism or was found in people with a rheumatic family history. One case I saw in a young girl had numerous rheumatic looking nodules about the elbow, and cleared up under salicylate of soda.

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[1] *Proc. Roy. Soc. Med.*, 1913, vi (Sect. Derm.), p. 133.

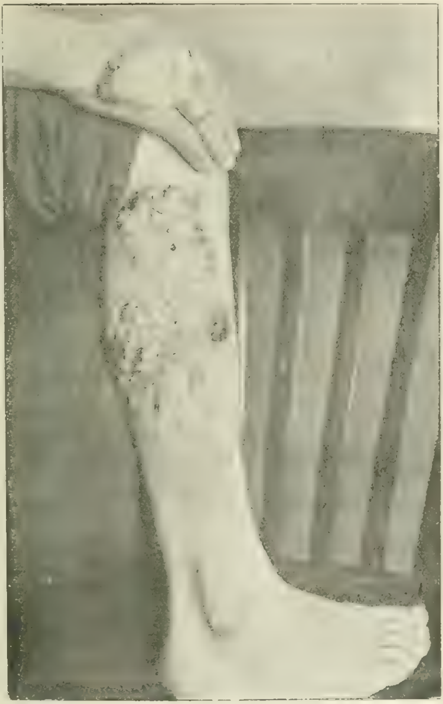
Dr. W. KNOWSLEY SIBLEY: The association of rheumatic conditions in cases of granuloma annulare is what one would expect. Is not the presence of subjective symptoms in this patient due to rheumatism? I take it that the enlargement of her finger joints is a rheumatoid condition, and it might suggest that her rheumatic symptoms are of the same nature as those seen associated with old chronic rheumatic joints, with raised erythematous patches over them, such as on the elbow. It is possible this skin condition may often be rheumatic in origin.

(*November* 15, 1917.)

Case of Bromide Eruption.

By W. KNOWSLEY SIBLEY, M.D.

THE patient is a single man, aged 19, a flour-miller by occupation. There is nothing of note in his family history. Last February, he says, a sore place commenced on the outer side of his right thigh, and afterwards on the dorsum of the right hand, and these rapidly extended. In March a similar condition appeared on the left calf, followed by a lesion on the dorsum of the left hand, and extensive ulceration of the lower leg. He was sent to the hospital six weeks ago, by a doctor who did not know what the condition was, and he then presented a large fungating granulating sore measuring some 8 in. by 4 in. on the right thigh and a smaller one on the dorsum of the right hand. The lesions on the left side had healed. He had also a good deal of rather acute recent pustular acne on his face. On going into his history I ascertained that he was an epileptic, and had been taking bromide of potassium for five years (15 gr. twice a day), unknown to his doctor, from a prescription he had obtained from a former doctor. It struck me at once that it was a bromide eruption. The medicine was stopped, and now, six weeks later, there is practically complete healing of all the lesions. I have given him 10 gr. of borax three times a day, and he has had no more fits since. The Wassermann and von Pirquet reactions are both definitely negative, and there is no fungus in the exudation. At first the condition looked like blastomycosis, but further examination from that point of view was negative. I show a photograph



Bromide eruption.

of the condition of the arm and leg when he first came under observation. He is about the middle member of a family of seven healthy persons, and the only member of his family known to be epileptic. Numerous red and circular pigmented scars are present on both thighs from old healed lesions. Although much more extensive, the active lesions when first seen were not unlike some of the photographs of this condition published in Stelwagon's "Diseases of the Skin" (7th ed., 1914, p. 460).

Dr. GRAHAM LITTLE: There is too much ulceration here (and the history describes an even greater degree in the past) for it to be explained as due to bromide ingestion, in which ulceration is absent. I should regard this as probably syphilitic.

Note by Dr. Sibley (January, 1918). — A further Wassermann reaction was distinctly negative, and the whole condition had completely healed without specific treatment.

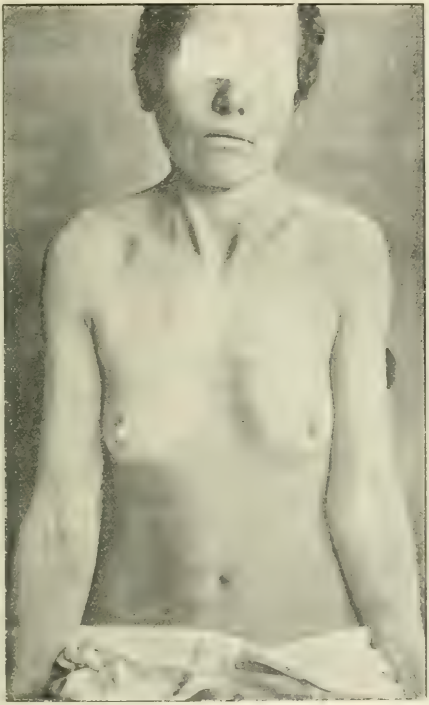
(*November* 15, 1917.)

Case of Keratosis Follicularis (Darier's Disease).

By W. KNOWSLEY SIBLEY, M.D.

THE patient, E. B., was sent to me by Dr. J. B. C. Brockwell. She is a domestic servant, aged 33, unmarried. Her case was published by Dr. MacLeod in August, 1909, and she has now had the disease for twenty-one years, it having first appeared on the extensor surface of the forearms, then about her forehead, afterwards on the abdomen and legs.

There is nothing to note in her family history; her parents are both living; her father is crippled with rheumatic gout, her mother is stated to have had measles when the patient was born; her three brothers and five sisters are living and well. The patient was in an asylum with complete loss of memory for nine months a few years ago, during which time the disease became obviously worse. Her general health has been indifferent for some time, and she has been unable to follow her occupation for the past six months. She states she never perspired much. The greater part of the skin area is now covered with dirty coloured, rather greasy, yellowish-brown, variously sized papules, which are most prominent on the abdomen, but few are present in the groins. The palms of the hands and soles of the feet have always remained more or less free from lesions, and the nails are but slightly affected, though most of the finger nails have been shed at times, and those of both great toes are much thickened. The scalp was covered with a greasy seborrhœic secretion, and the patient states that six months ago she became quite bald over the whole of the vertex of the scalp, but the hair has since grown again. On the anterior surfaces of the legs, and of the thighs the lesions have coalesced and produced some extensive superficial excoriation with enlargement of the glands in the groins. There have never been any lesions in the mouth, but at one time the vulval mucous membrane has been affected.



Keratosis follicularis.

The urine gives a specific gravity of 1014, acid, no sugar, but a marked trace of albumin is present, together with a few fine granular casts.

A good deal of controversy has taken place among various observers in connexion with this rare form of skin disease, which was first described by Morrow[1] and called by him "keratosis follicularis"—a most fortunate name, since it correctly expresses the pathological condition present in all cases reported to date.

In the case now under observation the conclusions have been based on the examination of a large number of sections taken from the different areas in which the disease has been prominent. A section was taken from a single, elevated lesion on the left side of the trunk, fixed in alcohol and cut in paraffin. On microscopical examination the hair follicle was found to be filled with a horny plug, and presented a marked hyperkeratosis of the stratum corneum; a large number of horny cells were present. Deeper down in the funnel-shaped opening of the follicle itself numbers of Darier's so-called psorosperms were found to be present, these now definitely accepted as being epithelial cells undergoing degeneration. In the neighbouring hair follicles the same hyperkeratotic condition was found to be present exhibiting many degenerated epithelial cells. In the diseased area the stratum granulosum appeared to be well defined, but not thickened in any way; the stratum mucosum showed a decided increase and an attempt at partial destruction in places; the cells of this layer were apparently undergoing hyaline degeneration, hardly any sebaceous glands were visible; in the traces which could be found the glands appeared to be much overgrown with epithelial cells; the sweat ducts appeared to be normal. The changes in the cutis were very slight, and consisted of small cell infiltration immediately below the lesion: the papillary blood-vessels were dilated and surrounded with small round cells; the deeper vessels were also dilated; there was also a small amount of pigment present in the papillary layer.

A section of a well-matured lesion occurring in the hair follicle was excised from the median line just above the mammary gland and confirmed Morrow's own views of the disease, that it is necessarily a true keratosis follicularis involving the whole of the apparatus of the sebaceous gland. The chief changes in the section were seen to reside in the hair follicle itself, and consisted of a distinct hyperkeratosis which contained epithelial cells in various stages of degeneration. The stratum granulosum was well defined though not increased in any way. The stratum mucosum, especially at the side of the hair follicle, showed a marked thickening, but at the base of the funnel-shaped opening the layer appeared to be much thinner, and was made up chiefly of degenerated epithelial cells; the hyaline degeneration at this stage of the disease did not appear to be so well marked. The duct leading from the sebaceous gland was distinctly dilated and overgrown with epithelial cells, which was probably a secondary affection, the outer layers of the epithelial cells of the sebaceous glands were flattened against the basement membrane; the more internal layers were also flattened, and showed no signs of undergoing fatty transformation. The principal secondary changes apparently occurred in the sebaceous gland itself, and were fully recognized by the contents of the follicle and its situation or limitation to the upper part of the corium; the hair follicle extended much deeper than the lesion.

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[1] Morrow, *Journ. Cut. and Ven. Dis.*, 1886, iv, p. 257.

If the lesion especially affected the hair follicle it would have extended to the subcutaneous tissue, and the contents would not have consisted of so many degenerated epithelial cells. The appearances in these sections, as in Morrow's case, resembled those found in comedones, except that there were not so many epithelial cells in the sebaceous plug as were present in that disease—that is, the lesion under consideration showed a marked hyperkeratosis combined with obstruction and dilatation of the whole of the sebaceous apparatus, the duct leading from the gland being considerably enlarged.

In conclusion, the pathology of this interesting disease is undoubtedly a keratotic follicular condition with a secondary affection, which occurred in the whole of the sebaceous apparatus, hence the term first applied by Morrow "keratosis follicularis," which was well defined both in his sections and in those accompanying this case. Further, great credit is due to Morrow for the very able illustrations and description he has given us in connexion with the histology of this disease.

The disease which Darier describes in his paper as "psorospermose folliculaire végétante" is unfortunately named, and his psorosperm theory has been entirely abandoned by histologists who regard these so-called parasites as only epithelial cells undergoing degeneration.

After all the pioneer in the elucidation of this very rare disease is Morrow, who first described it in 1886. The next in the field of research was Professor White,[1] of Harvard University, whose paper, accompanied by an account of an histological examination by Bowen, was published in June, 1889, and was followed immediately afterwards by the famous memoirs of Darier[1] and Thibault[2]: this was exactly three years after Morrow first described the disease. British dermatologists have also made valuable contributions to the research work in connexion with this disease, principal among which is the joint paper of Dr. Ormerod and Dr. MacLeod.[3]

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[1] J. C. White, *ibid.*, 1889, vii, p. 201; and 1890, viii, p. 13.

DISCUSSION.

Dr. J. M. H. MacLeod: This case was under my care at Charing Cross Hospital nine years ago. On the whole the patient's condition is not so good as it was then, and her disease is more extensive. I tried every form of local treatment I could think of, including X-rays, but with disappointing results.

Dr. Sibley: I have applied three X-ray treatments to the left forearm, and all the lesions have apparently disappeared from the area treated.

(*November* 15, 1917.)

Bullous Urticaria in a Baby.

By H. G. Adamson, M.D.

This patient (D. W.) is aged 17 months, and since the age of 3 months she has had very severe nettle-rash, involving the whole body. It is not of the type of urticaria we usually see in children—namely, lichen urticatus—or more rarely, urticaria pigmentosa, and I do not remember having before seen this condition of generalized wheal formation in a baby. For more than a year there have been continuous eruptions of huge wheals over the chest and shoulders and head and neck especially, but also on other parts of the body and limbs. The wheals are accompanied by intense itching, so that whenever uncovered the baby tears at the skin with its finger-nails. During the last six months the wheals have been frequently the site of large clear bullæ, which come up in a few minutes and afterwards dry into a crust.

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[1] Darier, *Ann. de Derm. et de Syph.*, 1889, 2 sér., x, pp. 597-612.

[2] Thibault, Thèse de Paris, 1889.

[3] Ormerod and MacLeod, *Brit. Journ. Derm.*, 1904, xvi, pp. 320-334.

There are at present many crusts on the scalp and some clear bullæ on the forehead and neck. In spite of the almost continuous eruption the general health and nutrition has remained good. It is difficult to know what is the cause of this urticaria. If it were due simply to digestive disturbance one would expect to see such a condition quite frequently in babies, but the only other similar case I know is that pictured in the second edition of Dr. Sequeira's "Diseases of the Skin" (p. 367). It was thought that it might be due possibly to milk idiosyncrasy, and for six weeks the child was put upon a diet which excluded all milk, but with no other result than that it lost flesh. The baby was weaned at the age of 8 months and the change of diet made no difference. No drug treatment has been of any avail.

[Since this case was exhibited the child has been in hospital for ten days, and although the diet has been a normal diet for a child of that age, the eruption of wheals and bullæ continues.]

(*November* 15, 1917.)

Case of Pityriasis Rosea with Unusual Features.

By H. G. ADAMSON, M.D.

IN this case there was the usual history of a "herald patch," and, ten days later, the eruption of macules on the body, covering the trunk in the "vest area." It was at this time that I first saw the patient, and the body was then covered with the typical oval erythematous patches, which were beginning to scale from the centre outwards. This distinguished it from ringworm, syphilis and psoriasis, and, together with the absence of papular elements, from seborrhœa corporis. The case was unusual only in that the eruption extended on to the neck and face, the same characteristic macules with central scaling being scattered over these regions. The patient was seen a few days later, and it was noticed that the eruption had become very profuse on the face and neck and also on the trunk. Many areas on the trunk had become crusted and similar crusts were forming here and there on the face. On the next day the face became swelled, and covered with numerous crusts as it now is, so that it recalls the appearance of an acute dermatitis due to primula or some other irritant.

The particular points to which I wish to call attention are the profuse eruption on the trunk, and the redness, swelling and crusting of the face. That this crusting is neither the result of any local irritant, nor of impetigo contagiosa superadded, I am convinced, for I have watched its rapid evolution from the macules. It is not very unusual for pityriasis rosea to extend to the face and neck, and I have mentioned this fact and published a photograph of such a case in Allbutt and Rolleston's "System of Medicine." It is known, too, that it may sometimes extend down the arms and legs, and Dr. Graham Little has shown a case of this sort in which there were vesicles on the extremities. I think the prognosis is good in this case, and I do not propose to treat the crusted condition of the face, because I think it is not due to impetiginous infection and that it will subside spontaneously. It is analogous to the bullous formation with crusting which one sometimes gets in erythema multiforme or in lichen planus, and points merely to severity of the inflammation.

Dr. GRAHAM LITTLE: I showed a case a little similar to this in the extremely congestive and eczematoid character of this rash, in a boy,[1] and I have seen a second case almost as inflammatory and extensive. I regard the condition as undoubtedly pityriasis rosea.

[The case was seen four days after it was exhibited. The swelling of the face had entirely subsided and the cysts were drying and crumbling off, leaving a dry skin surface without anywhere exudation or pus formation. After three days more all the crusts had dried and fallen and the face had resumed its normal appearance. Had the crusting been of a superadded impetiginous nature, this would have hardly occurred without local treatment.]

(*November* 15, 1917.)

Case of Impetigo of Bockhart of Unusual Extent and Severity.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a young Polish Jew, aged 28, a milliner by occupation. The disease began to appear four years ago, with a pustular folliculitis of the same type as that now present. It gradually spread so as to cover nearly the whole body, with the exception of the face, the hands and the parts on which coarse hair is found, such as the scalp, axillæ and pubic region, which have never been affected. There is fine scarring on those parts where the disease has receded. There is active folliculitis at present on the abdomen, chest, back of the trunk, thighs and legs. The beginning is always the same, a pustule centring round the hair, and large sheets of the surface are thus affected, the lanugo and not the bristle hair being selected by the disease. The man has been called up for military service, and is in other respects a well-developed robust person in good health. But I have advised the medical officer sending him to me for an opinion that he would probably become infective to others and would not be likely to remain long out of hospital if he were subjected to the conditions of service.

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[1] *Proceedings*, 1914-15, viii, p. 83.

(*November* 15, 1917.)

Case of Fibrosarcomata, grouped in a Peculiar Manner, on the Chest.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a man, aged 45, sent to me by Dr. Charpentier of Uxbridge. For the past fifteen years he has had a curious efflorescence of nodules on an area covering about 3 by 4 in. of the chest just below the right clavicle. On this area there are now numerous scars where earlier nodules have apparently been present, and here there are also active new nodules of a dark red colour and firm consistence, of the size of half an almond, with a certain resemblance to a lupus nodule, for which they have been mistaken. The whole patch of affected skin suggested an infection of the sites of a herpes zoster, but this is not confirmed by the history, which does not describe any initial injury. The Wassermann reaction is negative, and there is no history of syphilis, though the appearances are not unlike an old tertiary patch. A recent nodule was excised and submitted for examination to Dr. Kettle, pathologist to St. Mary's Hospital. His report is that the nodule is a fibroma or fibrosarcoma, the latter being the more probable diagnosis, as the cells are young and active, and the appearance that of new growth rather than inflammatory.

Dr. Adamson has made an interesting suggestion that the appearances of the affected patch recall Bowen's disease, pre-cancerous dermatosis. That observer cited a series of six cases, which had almost uniformly been mistaken for tertiary syphilis, but the histological sections had demonstrated their epitheliomatous character. Clinically this case is surprisingly like the pictures given in Bowen's article, but the histological aspect is entirely different. I hope to show the sections at a later meeting.

Dr. DORE: I showed a woman here some time ago with a single lesion in the right posterior scapular region, with much infiltration. No definite diagnosis was made, but various suggestions were offered, that it was tuberculous, syphilitic, or cheloidal, and Major Gray suggested that it was a new growth. Sections proved it to be fibrosarcoma. I am not certain as to the exact condition implied by the pre-cancerous dermatosis of Bowen, but Dr. Little's case does not seem to me to resemble the photographs of that disease nor the few cases I have seen in which that diagnosis was probable.

Section of Dermatology.

President—Dr. JAMES GALLOWAY, C.B.

(*December* 20, 1917.)

Multiple Rodent Ulcers with X-ray Burns.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a gentleman, aged 63, whose occupation is indoors and sedentary. Some time prior to 1910 he noted on the left side of the forehead a small pimple or elevation with a subsequent similar lesion on the back of the trunk near the iliac crest, on the right side. He was seen in 1910 by Dr. MacLeod, who prescribed a zinc cream. He was again seen by the same observer in 1912, when the lesions had obviously developed into ulcerations of a rodent type, and freezing was adopted for the ulcer on the back and X-rays given twice to the ulcer on the forehead. X-raying was continued by a general practitioner in Southampton, who, up to April, 1917, gave numerous exposures to all the sites: twenty-five times to back, sixteen times to forehead, seventeen times to nose. Fresh ulcerations developed on the back about 2 in. higher and to the left of the older lesion, and on the right ala nasi. During these treatments the practitioner was conscious of having produced a burn, and very frankly explained to the patient the nature of the accident. At the present time there is a large circinate area about 2 in. in diameter on the forehead upon which, amid scar tissue, there are two heaped up scabs covering a chronic ulceration ascribed to the burn, and there is a similar appearance on the site of the lower treated area on the back. The upper ulceration has practically healed over. The newest lesion, that on the ala nasi, dates only from two years ago, and is a typical small rodent ulcer with no X-ray burn at present.

The case is of interest for several reasons. In my own experience, and after a considerable search of the literature, the position on the back is one of the rarest situations for rodent ulcer. I have had one case of a large rodent ulcer of the back, and collected in a paper on the subject some three others. The question of treatment is also important. My own feeling is that no further radiation, whether by radium or X-rays, should be undertaken.

DISCUSSION.

The PRESIDENT: Will Dr. Little tell us the exact criteria by which he judges this case to be one of "rodent ulcer": what evidence would he consider in favour of that diagnosis rather than of a para-keratotic and pre-epitheliomatous condition, such as is seen in persons of advancing years, and occasionally in the case of younger people? I recollect several individuals who were suffering from a "pre-epitheliomatous" type of degeneration in various parts of the body. I saw one such case who had been treated in various ways, and concluded, like Dr. Little, that such cases do not bear X-ray treatment well. That patient came to me with considerable X-ray ulceration and scarring, and the serious degenerative nature of such scarring resulting from the X-ray exposures, bulked more largely in my mind than the original degenerative condition from which he had suffered in the areas treated by X-rays. I avoided the use of rays, and treated him by medical cleanliness and mild antiseptic dressings for some months, and when he left my care all the lesions had healed. He has written to me since that he remains well; that X-ray treatment had been applied again to one or two areas presenting the original type of senile keratosis, and, apparently, successfully so far. Previous experience had indicated that the X-ray applications must be done with the very greatest care.

Dr. J. J. PRINGLE: The occurrence of such multiple growths is comparatively common, but we have no evidence that the growths on the back of this patient are, microscopically, of the same nature as the lesions we usually call "rodent," which those on the face undoubtedly are. I believe radium to be much the best application for small rodent ulcers. My experience on the point is now fairly large; I have taken all my cases of rodent ulcer for a number of years to the Radium Institute. My cases, however, have all been early ones, and I have not had any untoward results.

Dr. LANCASHIRE (Manchester): As regards diagnosis, the only person to whom we can look is the man who saw it before treatment: we cannot say by inspection at this stage. The history seems to point to it having been rodent ulcer. My experience has coincided with the President's as to the danger of pushing X-rays to any degree in senile malignant degeneration of the skin. At present I am treating an old gentleman with nine or ten of these senile degenerative lesions about his face and neck, and they heal remarkably readily with a simple application of CO_2 snow and mild measures of that kind. There is no question about the definite action of radium in early cases of rodent, though I am not so sure about ultimate results. In severe rodent ulcer I think you can do practically all with X-rays which you can accomplish with radium. The cases which in my clinic have not done well with X-rays and have been referred for radium treatment have given disappointing results.

Dr. GRAHAM LITTLE (in reply): In diagnosing this condition as rodent ulcer I was influenced by two considerations. The first was that this diagnosis was made by Dr. MacLeod in 1910, before treatment had been applied; the second was the course of the disease. I class epitheliomatous ulcerations as rodent ulcers if they show the two criteria—namely: (1) slowness of growth and (2) absence of metastasis. That is a convenient clinical formula for separating this from other forms of malignant growth. It is justified in this case because the ulcerations are very chronic. Even in 1910 they must have reached a considerable stage of development to warrant treatment by X-rays. This condition is much more chronic than is precancerous senile dermatosis as a rule. With regard to treatment by radium, a remarkable instance of rodent ulcer has been under my observation five years. It has been ionized, frozen, X-rayed, excised and treated with radium—practically all the recognized processes have been tried. While treated with radium the ulceration developed very rapidly, exposing the whole chin bone. The patient was seen three years ago by Sir Alfred Pearce Gould, who suggested a very extensive operation, scraping the bone and cutting away a large part of the tissues of the face. But before that was done, caustic, in the form of arsenical paste, was applied, and to-day the man came to me cured! I am glad to have my view verified, that raying should not be continued in this case in any form.

(*December* 20, 1917.)

Case of Extensive Dermatitis, possibly Mycosis Fungoides.

By E. G. Graham Little, M.D.

The patient is an elderly man, in poor general health, who was sent to me by a practitioner, after he had seen Sir Malcolm Morris, who diagnosed mycosis fungoides, and recommended X-raying. I persuaded the practitioner to delay the application of X-rays until the patient had come before this Section with a view to obtaining further opinions, as I was convinced that the condition was not mycosis fungoides, but a severe form of seborrhœic eczema. The patient shows large areas of affected skin, and the eruption is intensely itchy, but there is no induration of any of the patches, there is no glandular enlargement, and the earlier lesions are typically follicular, an observation which Dr. Adamson confirms. The history is that the eruption began, fifteen months ago, on the legs, which at present are the most extensively affected areas, and there is a very inflammatory eczematous eruption over the whole leg. On the thighs, and especially in the groins, there are large patches of dry seborrhœic type, and there are numerous areate follicular patches scattered over the trunk back and front, the forearms, and neck. The scalp and face are comparatively little affected.

DISCUSSION.

Dr. Adamson: I do not think this is mycosis fungoides, but a dermatitis arising by infection from the large patch of chronic dermatitis on the leg below the knee. I think it is of the type which we call "seborrhœic dermatitis," and that it will clear up under treatment by baths and mild antiseptic ointments.

Dr. S. E. Dore: I saw this patient with Sir Malcolm Morris, and concurred in the diagnosis, chiefly because some of the lesions were so raised, and had a semi-elastic feel, such as is given by the lesions of mycosis fungoides. I suggest that a biopsy would help.

The PRESIDENT: I strongly urge Dr. Graham Little to delay treatment of this case by means of X-rays. If you allow this man to go about five and a half days a week, and rest one and a half days only, I do not think there is much prospect of the lesions healing for a long time. I should say it was an infective dermatitis produced by organisms of no high degree of virulence. I strongly urge complete rest and getting the patient medically clean, and withholding X-ray treatment for the present.

(*December* 20, 1917.)

Case of Trophic Ulceration of the Foot after Gunshot Wound.

By E. G. GRAHAM LITTLE, M.D.

THE patient, W. S., a young man aged 23, was wounded in September, 1916, in the right leg. He was admitted into the Orthopædic Hospital in January, 1917, and there operated on by Mr. Bankart, who has kindly furnished the following note:—

"I operated upon W. S. at the Orthopædic Hospital on January 30, 1917. The right sciatic nerve was found completely divided and its ends buried in a mass of scar tissue. The scar was excised. The ends of the nerve were resected and united by end-to-end suture. The union was surrounded by a flap of fat. He was last seen by me in July (just before he left the hospital) walking, as I thought, very well in a walking instrument. While in the Orthopædic, he had trophic ulceration of the toes, but nothing on the sole of the foot."

The man had no sensation on the right side or sole of the foot at the time of the earliest appearance of mischief on the sole, which dates from about three weeks ago, when he thinks he must have scalded the foot with a hot-water bottle. There is at present a deep ulceration with a base of the area of an inch in the middle of the tread of the foot. I regard the ulcer as of the same general type as the well known tabetic perforating ulcer of the foot, but the prognosis is obviously better, as restoration of function is already beginning. The trophic ulcerations present in the earlier stages, as described by Mr. Bankart, have left some traces in a persistent onychia of some of the toes.

The following note from Dr. Wilfred Harris describes the symptoms from the point of view of the neurologist:—

"Foot-drop due to injury of sciatic nerve by gunshot wound. Voluntary power now returning well. Considerably more anæsthesia remaining than is usual with the degree of voluntary power present. Reaction of degeneration still very pronounced to electrical testing."

It is somewhat surprising that these ulcerations should be met with so infrequently, in view of the common occurrence of the nerve injuries such as this man has sustained. The sequence of events will remind members of the classic experiment of Head, who procured the division of his own ulnar nerve and then froze his hand. The resulting lesion persisted unhealed for many months as a chronic ulceration which I inspected, and of which this case reminds me.

The PRESIDENT: There can be little doubt as to the exact nature of the ulceration. The man apparently has injured his foot by walking and, owing to the nerve injury, he was not aware of the damage till the ulceration occurred and gave him a warning by eyesight, and not by the sensations produced. That is often so in the case of so-called trophic ulcers. I recollect a remarkable case[1] of a man who suffered from syringomyelia. Unknown to himself, he had trophic degeneration of the bed of his great toe nail, and the lesion had become septic and suppurated. On his getting into bed one evening, his toe-nail caught in the bed clothes and the nail was ripped off, but he was not conscious of the fact until the next morning. As to the actual duration of time necessary for healing, if such patient is rested and the condition is well cared for, healing takes place very quickly: in other cases there is so much vasomotor disturbance that healing is much slower. I regard the prognosis here as good.

(*December* 20, 1917.)

Case for Diagnosis.

By S. E. DORE, M.D.

THIS woman is aged 56. She states that she has had these multiple tumours for only three months. I saw her last night for the first time. A biopsy is being made, which I shall report later.[2]

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[1] *Brit. Journ. Derm.*, 1895, vii, p. 305.

[2] Sections showed the growths to be sarcoma, medium to large spheroidal cells being packed within areas of fibrous tissue.

She had a large number of subcutaneous tumours, the skin over some of them being a little discoloured. They vary in size from that of a pea to a walnut, and are scattered about the scalp, chest, back, shoulders, arms and the abdominal wall. A few are prominent but the majority can only be detected on palpation. They are acutely painful and tender, and she says she cannot lie on her back at night in consequence. I have made a provisional diagnosis of neuro-fibromatosis. I do not know what other condition of multiple tumours is likely to give rise to so much pain and tenderness with the possible exception of myomata.

DISCUSSION.

The PRESIDENT: There is the hardness of the tumours and the alleged duration of the disease to be borne in mind—she says she has had the nodules only three months—while none of the curious defects, such as unusual pigmentation, the presence of soft fibrous tissue masses, are present. If this is neurofibromatosis, it is an unusual example.

Dr. F. PARKES WEBER: I think the relatively short duration of the condition is against Dr. Dore's suggestion, and also the fact that there are no typical molluscous fibromata present.

(*December* 20, 1917.)

Case of Psoriasis.

By S. E. DORE, M.D.

THIS patient, aged 27, presents an extensive eruption of psoriasis, a little unusual in type, scattered over the trunk and limbs. He has had it four or five years. His finger-nails and toe-nails are affected. On several of the toe-nails he has little black plugs embedded in the substance of the thickened nail-plate. One cannot draw conclusions from a single case, but as it was such a typical and extensive example of the disease, I thought it would be useful to try intramuscular injections of sulphur, first advocated for psoriasis, I think by Jacquet. Louis Bory added eucalyptol, and Gougerot has also published a formula for the treatment of psoriasis by sulphur, and remarks that it can hardly fail to influence the "terrain" of the disease. I gave this patient Bory's formula: Precipitated sulphur 20 cg., eucalyptol 20 c.c., oil of sesame 80 c.c. After 5 c.c. he had a good deal of pain, but after 3 and 4 c.c. doses there was little inconvenience, although he has complained of shivering after the injections. He has had seven injections altogether into the buttocks, and he has distinctly improved without other treatment, but on the whole the result has proved disappointing in comparison with the brilliant results claimed by Bory.

Section of Dermatology.

President—Sir JAMES GALLOWAY, K.B.E., C.B.

Case of Acne Keloid.[1]

By E. G. GRAHAM LITTLE, M.D.

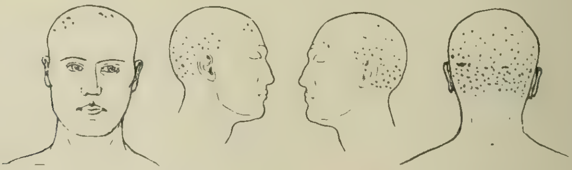
THE patient is a man, aged 32, a porter at St. Mary's Hospital. His history is very definite and emphatic, to the effect that the eruption began less than three months ago, and was not preceded by any symptoms. He has never had acne vulgaris. Two types of lesion are present, an early perifollicular plug of blackened and hardened tissue, the whole lesion looking very like an acne comedo, but differing essentially from this in the feature that pressure upon it does not result in the squeezing out of the plug, which can be dislodged with a fine scalpel, leaving an infundibuliform pit round the hair. Lesions of this type are found over the greater part of the scalp, with the exception of the area between the vertex and the site of the anterior fontanelle. The second lesion is seen to be especially profusely distributed over an area on the occipital region about 2 in. wide in the vertical axis, bounded by the lines joining the upper edges of the pinna and the lower lobes of the ears (*see* diagram). Here there is a broad band of skin closely occupied by firm, hard, shotty papules, with a base of about $\frac{1}{16}$ in. diameter, and raised about $\frac{1}{8}$ in. from the general level of the skin. These lesions are a deep red, and in the great majority there is no obvious suppuration; in quite a few there is a very small bead of pus visible. Lesions of this type are also found about the temple, but elsewhere the eruption consists of the non-inflammatory comedo, like a plug of hyperkeratosis. It

[1] At a meeting of the Section, January 17, 1918.

is especially to be noted that the nape of the neck is free, the lower limit of the eruption on the occipital area being the line joining the two lobes of the auricle, a line which is well above the level of any stiff collar. The general distribution is shown in the diagram. The scalp is otherwise quite healthy, there is no seborrhœa, and the hair is unaffected.

The patient is a robust, well set up fellow, clean in his person, and his occupation, which is chiefly attendance at the door of the hospital, does not involve getting particularly dirty.

It will be noted that there are no bands of induration such as form so conspicuous a feature of the old-established disease. But this is an unusually early case, in date and in type. The onset has been exceptionally rapid, and the area covered is unusually extensive; nevertheless I regard the case as certainly an example of acne keloid, probably one of the earliest in date that has been recorded. A complete bacteriological examination has been made by my colleague, Dr. John Matthews, who reports absence of the acne bacillus, and otherwise a varied but not specific bacteriological content.



A subsequent report will be made on this case including its histology.

DISCUSSION.

Dr. Whitfield: This case is not one of true acne keloid. I do not believe it will develop into keloid masses. It is practically acne indurata of the scalp. I have seen a fair number of those cases, more since the war started, which I think is partly due to the filthy cap worn by these men from the Front: possibly the steel helmet aggravates the condition as well. If that man were to daub his affected areas twice a day with a 20 per cent. solution of salicylic acid in alcohol, the lesions would probably disappear. I cannot conceive acne keloid clearing up after such an application.

Dr. H. G. ADAMSON: This case appears to be a typical example of the earlier stage of acne keloid. The intensely hard dome-shaped papules present in this case were described by Kaposi as the earliest lesions. Kaposi pointed out that the early lesions are papules of uniform consistence, and not pustules. The appearances here are exactly those of an early case of acne keloid of which I published a photograph in an article on "Dermatitis Papillaris Capillitii (Kaposi), or Acne Keloid."[1] The conclusions at which I arrived in that paper supported the early view of Kaposi that this disease is a plasma-cell infiltration, and probably due to a microbic infection, which is neither that of the acne bacillus nor of ordinary pus cocci—that the disease is neither related to acne vulgaris nor to sycosis, nor is it a true keloid.

Dr. GRAHAM LITTLE (*in reply*): Dr. Whitfield's suggestion of an infective folliculitis from a dirty cap is best met by pointing to the extremely scanty degree of suppuration: moreover the sites where infection from this source would be most pronounced are not the sites where such inflammation as is present is at its worst; thus, for example, there is no inflammation on the forehead where the constriction of a cap is greatest. Moreover, there is no reason to assume that the unavoidable dirt of the headgear of the trenches should be found in this case, and the man's general appearance further negatives this supposition. Nor can this be regarded as an "acne indurata," for surely an acne vulgaris beginning at this age, occupying the sites here affected, and with this development, would be unique. The hardness of the inflammatory nodules is highly characteristic of acne keloid; with regard to the comedones on the forehead these have been recorded in true acne keloid; they are scanty in the present instance.

Case of Erythema ab igne, or possibly "Melanopathia Syphilitica" of Wilson.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a man, aged 45. He is a mortuary porter, in poor general health, anæmic and ill-nourished. He admits that he has sat by the fire a great deal in the past few months, the eruption dating from six months ago. Clinically this is very like that of erythema ab igne, a reticular congestion and staining with large meshes, and wide trabeculæ, occupying the skin in front of the knees on both sides, but more marked on the right side, from the patella down to the level of the junction of the middle and upper third of the limb. There is less accentuated staining at the inner side and on the calf. There is no ulceration in these areas but there was a large vesicle situated immediately over a branch of the mesh work on the right leg, probably due to friction or injury. He says that he had a sore on the penis lasting about a fortnight, some fifteen years ago. The Wassermann reaction is negative at present.

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[1] *Brit. Journ. Derm.*, 1914, xxvi, p. 75, fig. 2, Case II.

Wilson described a case of reticular discoloration of the leg in a young woman with symptoms of syphilis, and named the plate "Melanopathia syphilitica" in his Atlas, "Portraits of Diseases of the Skin," plate 24. Ehrmann has redescribed the condition as "a new vascular symptom of syphilis." Wilson's plate certainly bears a strong resemblance to erythema ab igne, and his identification of the eruption with syphilis was apparently overlooked or not accepted. There would seem, however, to be a reticular syphilide, rare, and very difficult to distinguish from the eruption of erythema ab igne; this case may be an example of the syphilitic condition if the sore on the penis may be taken to have been a hard chancre.

Dr. H. G. ADAMSON: This is erythema ab igne: the faint net-work of pigmentation beyond the more obvious net-work of inflammatory infiltration favours this view.

Case of Lupus Vulgaris, with Cutaneous Horns.

By W. KNOWSLEY SIBLEY, M.D.

MALE, aged 26. Father and uncle said to have died of phthisis. Six brothers and one sister living and well. His next younger brother also suffers from lupus.

This man's trouble commenced with a patch on the left cheek fifteen years ago, and it has gradually extended. He has been in one of the London infirmaries for some time. For some ten years he has had these curious cutaneous horny growths appearing round the left side of his mouth, especially on the upper lip. Horny lesions are described as occurring in lupus, but I have never seen one in which horny growths have been as prominent as in this case. They are true horny substances, not warty nor crusty, and are very firmly attached to the skin. Apparently there is no epitheliomatous change at the base of the horns, but I presume it is a case in which such change may occur later. He has also extensive superficial lupus on the upper part of his body, and on the right foot.

Case of Keloid (?).

By S. E. Dore, M.D.

I have brought this case for diagnosis. This young woman says she had ulcers on both shins seven and a half years ago, and that they were preceded by a rash on the trunk. The ulcer on the right leg healed in fourteen months, leaving a healthy more or less circular scar, which is visible now. The sore on the left leg nearly healed in eighteen months, but a small "pimple" was left, and this gradually increased in size to the present time. There is now a raised button-like growth about the size of a shilling on the site of the scar on the left leg. The edges are well defined and without any "processes," the skin over the tumour is slightly thin and shiny but otherwise normal, and the growth is firm to the touch, movable over the underlying tissues, and does not show any evidence of inflammatory changes. As there is a scar in a corresponding position on the opposite leg, and a history of the tumour having originated in scar-tissue, I made the diagnosis of keloid, but I shall be glad of alternative suggestions.[1]

With regard to treatment, I suggest radium, but it has just been suggested that the growth should be excised and microscoped. If it is a keloid, however, I doubt the advisability of excision, although I have heard it stated that if a keloid is sufficiently widely excised it does not recur.

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[1] Microscopical examination showed the growth to be a variety of fibroma.

Case of Keloid in a Child, aged 7.

By S. E. Dore, M.D.

This little girl was brought to me in the same week as the other patient, and I thought she might usefully be compared with her. The story is, that five weeks ago she was bitten by a dog on the left cheek, in several places. A fortnight ago, when she was brought to me, there were linear keloidal growths at the inner and outer canthus of the left eye and similar raised growths along the naso-labial fold, at the angle of the mouth and in the centre of the cheek. The case is also interesting in comparison with the other patient from the short incubation period, if I may use the term. My own feeling was that radium should be applied at once, but Dr. Whitfield is of the opinion that cases of this type, which he regards as one of hypertrophic scar tissue, and not true keloid, improve spontaneously, and should not be treated for several months.

Section of Dermatology.

President—Sir JAMES GALLOWAY, K.B.E., C.B.

A Syphilide with Resemblances to Pityriasis Rosea.[1]

By E. G. GRAHAM LITTLE, M.D.

I HAVE brought this case to the Section as it presents to me some difficulties in history and origin. The patient is a young naval officer, who denies any sexual contact, and I accept his word. At the beginning of December last he noticed some "sores" on the penis and showed them to a practitioner in Ramsgate, who diagnosed the condition as herpes præputialis. There were at first three vesicles, which a few days later increased to six. They had healed over in about ten days. About December 17 he suffered from a severe "cold" with some catarrhal sore throat. On January 4 there was an acute eruption, affecting the vest area, of pale nummular patches very like pityriasis rosea. He was seen by me just a month later, when the eruption was difficult to differentiate from pityriasis rosea. It followed the same distribution: there were no papulo-squamous lesions, and the mucous membranes were intact. On the penis there were several hard nodules in the site of the earlier herpetic lesions, and the glands were greatly enlarged all over the body. The fauces were congested, but there were no mucous patches. The patient had been treated by the practitioner at Ramsgate with a course of liq. hyd., and this induced me to defer carrying out the Wassermann test.

Postscript.—The Wassermann test was not done until after the meeting. It proved positive, so that the diagnosis is in favour of the syphilitic causation. I have seen cases of pityriasis rosea in which the diagnosis was not in doubt—one quite recently, in which the glandular involvement was quite as extensive and pronounced as in this case, and I had hoped that this would prove to be of this character.

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[1] At a meeting of the Section, held February 21, 1918.

Case for Diagnosis.

By E. G. Graham Little, M.D.

The patient is a man aged 45. The eruption consists, when fully developed, of large ringed elevated patches of a deep bluish purple colour, and a warty surface, rather like hypertrophic lichen planus.

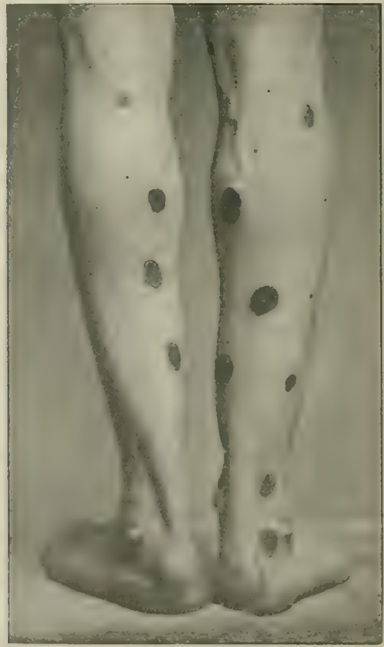


Fig. 1.

They commence with a small, very slightly scaly, pink patch, not at all like a planus papule, and the deepening of colour, the ringed halo, and the subjective sensation of itching only come with the enlargement of the patch. The condition started about six months ago with no explanatory circumstances. None of the lesions has disappeared, but fresh lesions have continually arisen, so that there are now numerous patches present, distributed chiefly on the legs below the knee, but present on the forearms and trunk. The mucous membranes are intact. I have not formed any opinion on the case as yet, which is quite new in my experience, and hope to make a further report later. The photographs (figs. 1 and 2) give an excellent idea of the fully developed patch.

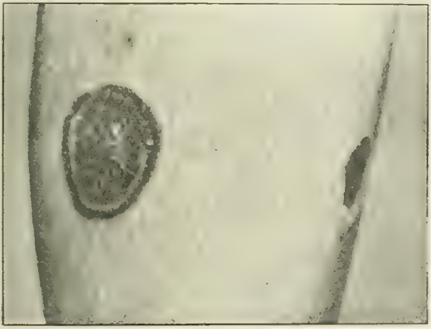


FIG. 2.

DISCUSSION.

Dr. ALFRED EDDOWES: Sir Jonathan Hutchinson would have called this case a "lichen psoriasis," because there are some points of great similarity between these patches and those of psoriasis.

Dr. VINRACE: The appearance and development are consistent with psoriasis guttata.

Dr. GRAY: I regard this as a case of parapsoriasis *en plaque*.

Dr. GRAHAM LITTLE (in reply): Lichen planus seemed to me the nearest possibility. The colour of the lesions is not unlike those of hypertrophic lichen planus, but I do not press the possibility. It certainly is not psoriasis: he has been under my observation for the past month, during which he has had chrysarobin applications. The eruption has not changed under this treatment: it was just as deeply coloured before its application, and it was never scaly enough to warrant that diagnosis.

Case of Persistent Erythema.

By Sir MALCOLM MORRIS, K.C.V.O.

(Exhibited by Dr. S. E. DORE.)

Dr. DORE: The patient is a clerk, aged 48, who has suffered from a persistent macular erythematous eruption, coming out in crops, for six years. There is a special localization to the hands and feet but the eruption is also scattered over the body. The macules are bright red in colour and there is no perceptible pigmentation. All of them disappear on pressure. Itching is slight or absent and there is slight factitious urticaria.

DISCUSSION.

Dr. GRAHAM LITTLE: This is undoubtedly urticaria pigmentosa. It is exactly like two cases shown on one afternoon at the Section, one by Dr. Sibley and one by myself. The eruption in both was especially prevalent on the forearms. There was nothing on the feet. It is usual in adult cases to see it on the wrists and forearms. The histology in the adult is similar to that in the young, and I can see no reason for denying the identity of the disease.

Dr. GRAY: An interesting feature, which I have not observed in other cases, but which Dr. Graham Little says is not uncommon, is that the colour disappears on pressure. There is no pigmentation in these lesions, nor are there hæmorrhages. It is obviously a congestive or nævoid condition. There have been two types of urticaria pigmentosa described from a histological point of view: those in which there is an increase in the mast cells, and those in which there is no such increase. It would be interesting to know whether this case is one of the latter type, in view of the supposed relationship of mast cells to pigmentation.

Dr. GRAHAM LITTLE: The diagnosis would be very difficult apart from mast cells, as that feature has been usually relied upon to determine diagnosis. I am quite unfamiliar with cases of true urticaria pigmentosa without mast cells.

Case of Lupus Mutilans.

By J. L. Bunch, M.D.

The patient, a labourer, aged 36, was treated at Addenbrooke's Hospital when a small child, and again when aged 18. He has not had any treatment since. The face began to be scarred when he was 2 years of age, and the process has gone on slowly ever since. The hands became involved when he was 12 years old, and the first fingers of both hands are now smaller than the others, and are both drawn down on to the palms, much resembling Dupuytren's disease. But there is no contraction of the palmar fascia. The drawing down process began when he was aged about 12, and has proceeded slowly since. There is scarring of the dorsa of both hands. The face is now distinctly scarred, the scar tissue practically involving the whole face. The lips are typically prominent and puffy. The ears are distinctly ulcerated, and so are both alæ nasi. This ulceration has resulted in the formation of much scar tissue. There are two or three small recent lupus nodules on the face.

The man seems to be in good general health, and is capable of hard outdoor work.

The patient has only just come under my care. I propose merely to treat the few active nodules he has; no surgical treatment will be of benefit, and forcible straightening of the fingers would do more harm than good. The first finger on both hands is smaller than the others. As he is doing hard outdoor work, he would not listen to such a suggestion as amputation of fingers.

Two Cases of Alopecia Areata.

By S. E. Dore, M.D.

Both patients are laboratory attendants. In the first patient, aged 40, the hair began to fall six weeks ago, when he noticed a single bald patch on the crown of his head, followed rapidly by other patches which coalesced, the scalp being nearly completely bald in six weeks. The other patient, aged 20, also has total alopecia of the scalp of six years' duration, and he has a brother, not a laboratory attendant, who has been bald for six years, but has now recovered. In seeking for a common factor to account for the alopecia in these two patients the supposition of contagion may be discarded, nervous shock does not seem to have entered into the ætiology, and, although I have seen cases recently which have been attributed to air-raids, these two patients seem to be particularly impervious to that form of nervous shock. Another possible factor common to these two patients is that of microscopical work and eye-strain, and although neither of them have errors of refraction, nor show evidence of having suffered in this manner, it occurred to me that, as Dr. Whitfield has recorded cases in which errors of vision have been the cause of alopecia areata, the strain of constant microscopical work might be a possible cause. The first patient has stained a section of a hair from a scale, and it shows a collar of microbacilli surrounding the hair, but I think Sabouraud's theory that alopecia areata is an acute localized seborrhœa cannot be substantiated, and is not generally held at the present time. It would be interesting to hear if the disease has been noted in other laboratory attendants doing microscopic work. Neither of these patients has had epidemic influenza, but one of them suffers from glycosuria.

DISCUSSION.

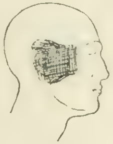
Dr. GRAHAM LITTLE: I think Dr. Whitfield rather overstates the expression of opinion at the International Congress. Several important authorities supported contagion. I consider pain to be very essential as a factor in the production of alopecia areata. I saw an extraordinary case illustrating this view, that of a doctor, who had a very unusual growth of hair on the abdomen. He suffered from renal stone, and had acute pain in "Head's area" on one side. At the seat of maximal pain there was an area of quite smooth skin forming a sort of island in a forest of hair. So that here there was loss of hair from a well-ascertained cause. Personally I do not hold the view that alopecia is contagious: I always allow my children patients with this condition to continue at school.

Dr. S. E. DORE (in reply): It is generally agreed that alopecia areata of the ordinary type is not contagious. It is possible that some of the epidemics which have been described have been due to impetigo or to ringworm in which the fungus has died out. Colcott Fox recorded an epidemic in which no ringworm fungus was present.

Case of Rodent Ulcer treated by Radium.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a man aged 50. He was referred to me by Dr. Lynham, of the Radium Institute. He was treated for over a year with radium, and at first there was considerable improvement, then suddenly the whole condition broke down and extended. The skin is eroded over an area extending from the right ear to the middle of the cheek, the pinna is adherent only by a small isthmus of tissue, the auditory meatus is deeply excavated by ulceration; the parotid gland and facial nerve have disappeared, facial paralysis being well marked. Ulceration extends to the margin of the orbit, but does not affect the eyelids. The limits of ulceration are indicated in the accompanying diagram, rodent tissue being deeply shaded. I have brought the case with the idea that arsenical paste might be useful; I have not tried this personally, but I have seen it have a most excellent effect in a case I saw some years ago, and I hope to be fortified by the opinion of the Section on this procedure.



DISCUSSION.

The CHAIRMAN (Dr. J. H. Stowers): The treatment by erasion and arsenical paste seems to me to be alone suitable in such an advanced case.

Dr. GRAY: This case appears to be very suitable for the use of arsenical paste. It is not much good to use the paste in cases where the nasal cavities are involved, but where the disease is limited to a smooth area of skin the results, in my experience, are very favourable. It is essential first to remove as much as possible of the growth with the curette, under an anæsthetic, and when the bleeding has been stopped by pressure, the paste is applied to the surface. The paste I now use, which Dr. Norman Walker recommended, is composed of anhydrous arsenious acid, one part: sulphide of mercury, five parts: animal charcoal, one part. The paste is made immediately before use by adding a little methylated spirit, and is applied with the handle of a scalpel, or something of the kind. If bone is exposed it will eventually come away as a sequestrum, and if the area is large may take a long time to separate. As soon as the original slough separates from the soft parts I generally apply Thiersch grafts with the object of reducing the granulating area as much as possible. The most troublesome spot is the outer angle of the orbit, as the growth may spread along the periosteum into the orbit, where it is very difficult to deal with. Where the growth is approaching this margin, it is a good plan to cut down on to the orbital margin and peel off the periosteum backwards and apply the paste to the bone direct. I give a dose of morphia before these patients come round from the anæsthetic, and have found that they suffer comparatively little pain: in fact this is often considerably less than the intense neuralgia from which many of them suffer before the operation.

Case of Linear Lichen Planus.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a boy aged 12. There is a typical eruption of lichen planus, extending in a sharply demarcated line from ¼ in. to ½ in. wide, stretching from the middle of the left buttock along the thigh and leg and passing behind the internal malleolus to terminate at the inner edge of the foot. Just below the buttock the line broadens out into a less well defined patch, which continues in the direction of the line, which is resumed again about the middle of the thigh. There are no lesions elsewhere. The case is exactly similar to that of a little girl shown by me and reported in the *British Journal of Dermatology*, 1917, p. 220. The duration of the disease is also much the same—that is, about three months.

The CHAIRMAN (Dr. J. H. Stowers): I have had a precisely similar case under my own care and the lesion disappeared spontaneously in a period of about two years.

Case for Diagnosis.

By S. E. DORE, M.D.

THE patient, a girl aged 13, has had a persistent symmetrical excoriation of the toes and dorsal and plantar surfaces of the feet for five months. No treatment has been of any avail. I have failed to find any ringworm fungus[1] and bacteriological examination only revealed the presence of staphylococci. "Artefact" and tuberculosis can, I think, be excluded, and it does not seem to possess the characters of "dermatitis repens," which has been suggested as the diagnosis.

DISCUSSION.

Dr. GRAY: The edge should be carefully examined for the epidermophyton. I have seen a groin ringworm in which the intensity of the inflammation was as marked as in the present one, and I am also convinced that lesions starting between the toes are quite as common as those in the groin.

Dr. GRAHAM LITTLE: This case is probably one of ringworm, and I suggest Dr. Dore should give it a rest from treatment for a week or two, then examine for the fungus. Sometimes its discovery is rendered impossible by treatment. The extent of the excoriation is not sufficient to negative that diagnosis, nor is the age: I saw a similar condition in a boy aged 12 with ringworm of the toes, from a school in which were thirty-three cases on the feet only. Nor does the sex discount this opinion; while comparatively uncommon in females it is quite frequently met with among them.

The CHAIRMAN (Dr. J. H. Stowers): The symmetry is very remarkable, and the possibility of artefacta on the effect of remedies, as modifying appearances, should never be overlooked. This patient may be the subject of so-called "eczematoid ringworm" of the feet, but I never saw a case of this nature presenting the peculiar raised vascular patches with abrupt edges such as are seen here.

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[1] Subsequent examination of scales showed ringworm fungus.

Case for Diagnosis.

By S. E. DORE, M.D.

THE patient, a girl aged 7, has symmetrical, scaly, slightly red patches on both cheeks which have been present for five years and have not yielded to any treatment. At first sight they resemble a seborrhœic dermatitis, but the chronicity, the marked accentuation of the natural furrows, producing the appearance of wrinkles, and the freckling and slight alteration in the texture of the skin, seem to negative this diagnosis. Dr. Pernet suggested that it might be an early stage of a condition described by him under the name of "atrophodermia reticulata faciei" and would probably end in atrophy. At present, however, there is no atrophy and although scales are present they are not like the adherent crust of lupus erythematosus, another diagnosis suggested. The condition has remained stationary and comparatively free from inflammation under treatment by soothing astringent lotions such as lead and calamine, but any attempt to use resolvents such as sulphur or salicylic acid or tar is immediately followed by erythema and a tendency to spread.

Dr. GRAHAM LITTLE: It is too persistent for an "impetigo pityrodes." It is on the flush area of the cheek, and partly for this reason I regard it as an early lupus erythematosus. There are many cases which last a long time without spreading.

Section of Dermatology.

President—Sir JAMES GALLOWAY, K.B.E., C.B.

Case of Prurigo diathésique de Besnier.[1]

By GEORGE PERNET, M.D.

THE patient is a married woman, aged 32, a Belgian refugee from Antwerp. The duration of the disease is ten years. It has been aggravated by her last pregnancy, six years previously, and also by her war experiences four years ago. The rash is generalized, but involves the arms and legs more especially, and chiefly the latter. The picture is that of Willan and Hebra's prurigo, but that begins in early childhood, whereas in this case the disease commenced at the age of 22 and might be called an acquired prurigo: I have preferred to give it Besnier's name. There is very little inguinal and femoral adenitis, although the condition is of long standing. She is well nourished, and, apart from the skin conditions, is healthy generally. Urine normal. There is intense pruritus, which interferes greatly with her rest. Treatment is made somewhat more difficult by reason of pregnancy, and has not done much good. From puberty the catamenia have occurred only every three or four months, and there are exacerbations of the pruritus at the time menstruation should occur in the amenorrhœic periods. I have seen several cases of this type beginning in adult life and they were very refractory to treatment, but I have found thyroid, employed quite empirically, do some good. The X-rays have also occasionally been useful.

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[1] At a meeting of the Section, held March 21, 1918.

DISCUSSION.

The PRESIDENT: A point for discussion is the influence of pregnancy on the whole of this type of eruption. We all know there is one very marked type of erythematous, congestive, destructive lesion of the skin which is closely associated with pregnancy—namely, what used to be called hydroa gestationis. We must all have seen degrees of that, even such mild degrees that one could hardly recognize it was a true pemphigoid condition: simply erythematous and papular, but nearly always pruritic. The question arises whether in such cases, with a history of amenorrhœa and exacerbations during pregnancy, pregnancy and its consequences may produce this type of lesion in addition to the one which we recognize as hydroa gestationis. At the age of 22 was there any question, in this patient, of pregnancy having commenced? Very little good can be done by treating such a patient as an ordinary out-patient; benefit can only result by having her under control.

Dr. PERNET (in reply): I do not think she was pregnant at the commencement of this condition, which she attributed to something she had eaten.

Case of Pruritus in Hodgkin's Disease — Lymphogranulomatosis Pruriginosa.[1]

By F. PARKES WEBER, M.D., and P. W. DOVE, M.B.

THE patient, G. M., aged 34, is a well-built, but rather thin, man, with considerable brownish pigmentation of the skin, especially in connexion with numerous small papules on the trunk and thighs—a pruriginous eruption, which has been associated with much itching (and consequent scratching), varying in degree from time to time. Some of the papules are reddish, and in an active (irritable) condition, whilst in others there is no sign of active hyperæmia at present. There is moderate, painless, enlargement of the cervical, axillary and inguinal lymphatic glands on both sides; one of the enlarged glands in the right axilla and one in the right groin have only recently appeared. There is likewise evidence of enlargement of lymphatic glands in the mediastinum, according to X-ray examination last year. Otherwise the viscera seem not to be diseased, and the spleen and liver appear not to be abnormal in size. There is no evidence pointing to there having been any venereal infection. The knee-jerks are present and the pupils react to light. A blood count (September, 1917), gave: Hæmoglobin, 100 per cent.; red cells, 5,200,000, and white cells, 8,900, in the cubic millimetre of blood. Microscopical examination of a blood film showed nothing special. Of the white cells 84 per cent. were polymorphonuclears. One of the nodules from the skin of the front of the abdomen, slightly larger and more deeply situated than the ordinary papules, was excised by Dr. H. Schmidt for "biopsy" purposes, in September, 1917, but microscopic examination showed only a chronic inflammatory fibrosis and a little small-cell infiltration.

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[1] The full account, together with illustrations and remarks, was published in the *Brit. Journ. Derm.*, Lond., 1918, xxx, pp. 15-22.

According to the history the illness commenced in September, 1916, with itching and small reddish spots on the skin. Two months later some glandular nodules on the left side of the neck were observed, and then the patient suffered from cough, fever and sweating. In February, 1917, the enlarged left supraclavicular glands were very noticeable. In September, 1917, there was a considerable packet of enlarged lymphatic glands on the left side of the neck and in the left supraclavicular region. On the body and limbs there were many papules; some of them were acne-like in appearance, and most of them were reddish; but one or two were slightly larger and deeper and without any red coloration of the skin over them. The man's general condition and symptoms have varied from time to time. Sometimes the affected lymphatic glands have been larger, sometimes smaller; sometimes there has been troublesome cough, and in January, 1918, there was considerable dyspnœa (possibly in connexion with enlargement of mediastinal lymphatic glands); sometimes the patient has complained of sweating and insomnia; occasionally there has been pyrexia; once there was troublesome diarrhœa; at times also there has been much loss in body-weight. In regard to the cutaneous symptoms sometimes there have been exacerbations of the pruritus with increased irritable redness of the prurigo-like papules. But frequently there have been exacerbations of pruritus in which the itching has been referred to the papules without the latter showing any fresh hyperæmic or inflammatory change. Since the commencement of his illness the patient has lost much hair from the scalp, face, axillæ, and pubic region; and the hair of his eyebrows, which used, he says, to be "bushy," has become short and scanty. During the earlier part of the illness the presence of scabies was suspected, and diminution of cutaneous irritation, when it occurred, was regarded as a result of successful treatment against scabies. Arsenical treatment was tried in 1917, but was given up on account of soreness of the eyes. According to the patient himself most of his periods of improvement have occurred when he has been under thyroid treatment; they were accompanied by a sense of general well-being, but he believes that he has generally lost weight at the same time. Warm baths and the application of vinegar at one time seemed to have a good effect on the cutaneous irritation. For sweating and sleeplessness a little morphia and belladonna was found to give temporary relief, but belladonna had to be discontinued on account of dryness of the mouth. During exacerbations of cutaneous irritation bromides, internally, and lead lotion and calamine lotion, locally, gave no relief.

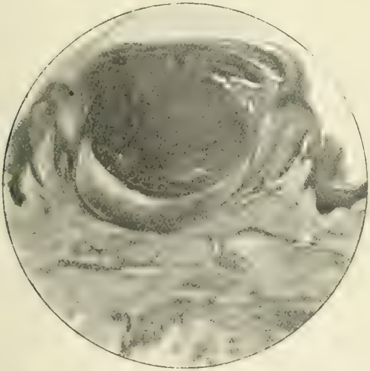
The PRESIDENT: This is the prurigo which precedes or accompanies Hodgkin's disease, or lymphadenoma. I remember a case, perhaps one of the most intense of all the cases of pruritus I have had to deal with, in association with Hodgkin's disease, in which we had the greatest difficulty in procuring any rest for the poor man.

Case for Diagnosis.

By W. KNOWSLEY SIBLEY, M.D.

A. V. H., A MAN aged 21, hairdresser by occupation, was sent to me by Dr. M. Goldfoot, who tells me that the patient's mentality has long been distinctly below the normal, and that he is very nervous. Both parents are living and well, and the patient states that a brother, two years older than he is, suffers from rheumatism and has somewhat similar a rash on his forearms.

Patient says he has never been strong, and has suffered from pains in his hands and feet for some years, but he has never had rheumatic fever. He had scarlet fever in September, 1914, and was in hospital for eleven weeks. The eruption on his body he states has been present for some six years; it does not irritate, and consists of slightly raised minute dots, like hæmorrhagic puncta, scattered over the abdomen, especially abundant over the scrotum and base of the penis, and over the dorsal and lumbar region, chiefly over the neighbourhood of the spinal processes. A few are scattered over the extremities, and are present on the tips of some of the fingers. Many have the appearances of minute nævi—"tache de Morgan." They are also seen on the buccal mucosa, especially that of the lower lip. The lesions on the scrotum are larger than in other parts, and are more like a nævoid condition. In this region the appearances vary from time to time, and become much less conspicuous after rest in bed. Those on the abdomen and back are all regular in outline, slightly raised minute dots. Those on the finger tips are very similar in appearance to the angiomatous stage of angiokeratoma in this position. The whole skin of the thorax and abdomen is decidedly pigmented. In parts, especially about the umbilicus, the condition has become dry and scaly, and here the red dots are easily removed by scraping, that is become exfoliated. Many of the spots do not disappear on pressure, others do. All the joints of the fingers are enlarged and painful, so also is the temporo-maxillary articulation, and patient is able to open his mouth only a little way. The lymphatic glands in the axillæ, groins, &c., are slightly enlarged, and hard and shotty to the touch. No enlargement of the liver or spleen can be felt. The second aortic cardiac sound is accentuated. Bruits are not present. Albumin is present in the urine, which has a very low specific gravity (1004), and the deposit contains squamous epithelial cells, fat cells, lymphocytes and phosphate crystals. No casts nor pus cells are present. The differential blood count shows an increase in



Section through centre of a papule from thigh. (⅙ obj.)

the mononuclear cells and basophils, otherwise it is about normal. An absence of blood platelets is noted. Polymorphonuclears, 69 per cent.; mononuclears, 10·25 per cent.; small lymphocytes, 16 per cent.; eosinophils, 0·75 per cent.; basophils, 3·25 per cent.; transitional cells, 0·75 per cent.

DISCUSSION.

Dr. F. Parkes Weber: I regard this case as a typical one of multiple hæmorrhagic telangiectases (angiomata) of the skin and mucous membranes. The paper which introduced this clinical group of cases most strikingly to the British profession was Sir William Osler's,[1] and not long afterwards I wrote on the subject myself. Dr. Sibley omitted an important point, which I ascertained—namely, that this patient's brother, who is two years older, has the same affection. The patient has had no bleeding from the mucous membranes, but I have little doubt that later on it will occur: he may have epistaxis, and bleeding from the tiny spots on the gums. Some of the most typical of the minute telangiectases, or angiomata, are on the finger tips. Some of these cases have renal trouble associated. What is the brachial systolic blood-pressure in this case? I think the Wassermann reaction ought to be taken.

Dr. Adamson: I do not remember having seen anything quite like this case. The three conditions to compare it with are: multiple telangiectases, certain unilateral nævi, and angiokeratoma. The late Dr. Fox and I have shown cases of unilateral nævi in which similar lesions occupied one area, one side of the chest, and in my case one arm. I do not agree with Dr. Parkes Weber that it is multiple telangiectases, because in that condition there are little tufts or dilated vessels. The affection is something like angiokeratoma, which occurs on the extremities. On looking at Dr. Sibley's microscopical section I do not agree that a hair follicle is involved; but the whole of the hæmorrhage is inside the epidermis; the horny layer is above it, and epidermis goes right down to the bottom. It is difficult to know how the hæmorrhage got there. The lesion, to me at all events, seems to be a unique one.[2]

Dr. Gray: I agree with Dr. Adamson. There is no trace of a hair follicle in the region of the hæmorrhage. It would be very interesting to find out how the blood got between the layers of the epidermis. I think a dilatation of a papillary vessel has occurred at the tip of the papilla and has burst fanwise into the epidermis on either side. In the specimen we have a lateral section of the hæmorrhage, and if serial sections were cut we might trace where it came from. The lesions do not disappear on pressure, and I therefore think they are purpuric.

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[1] *Quart. Journ. Med.*, Oxford, 1907-08, i, pp. 53-58 (2 coloured plates).

[2] A reference to the literature of angiokeratoma has since convinced me that Dr. Sibley's case is an example of the aberrant type of angiokeratoma, in which the lesions occur upon the trunk and scrotum, and I propose to refer more fully to this point in a letter to the *British Journal of Dermatology*.—H. G. A.

Dr. S. E. DORE: I do not regard this as a case of multiple telangiectases; in that condition there are dilated vessels which disappear on pressure, and in this case there appear to be definite hæmorrhagic points. The lesions are too widely distributed for angiokeratoma and there is no keratoma. The punctæ resemble the "cayenne-pepper grains" in "infective" nævus but they are said to disappear from time to time, whereas infective nævus generally spreads, and does not, as a rule, clear up. A purpuric eruption in a patient who has renal disease suggests a connexion between the two affections and I think this is probably a case of purpura in association with renal disease.

The PRESIDENT: Dr. Adamson has given us the differential diagnosis, and what he said almost completely covers the ground concerning the different possibilities. We have to be sure whether this is extravasation or hæmorrhage, or whether there is any evident telangiectasis, or not. Speaking from his appearance, I thought it was purpuric, without definite localized telangiectatic condition such as we see in the familial group of cases of which Dr. Parkes Weber reminded us. If this is familial, it is a very strong point in favour of the type of punctiform telangiectasis of which most of us have opportunities of seeing cases. There is a full-page picture in the Clinical Section's *Proceedings*[1] of a case I had at Charing Cross Hospital, and I have had opportunities of observing several. The spider-like telangiectatic condition, which has been going on for years, is one of the most striking features of the diagnosis. And that scarcely exists—certainly not to any marked extent—in this case. It remains with Dr. Sibley, partly by clinical examination and partly by more extensive histological examination, to tell us whether it is a definitely hæmorrhagic condition of the purpuric type. Failing that, there is the long-standing albuminuric condition, and the boy's obvious failure of health. I think the alternative diagnoses which lie before us are, diathetic hæmorrhagic purpura on the one hand and, on the other hand, a slight case—for one cannot call it an early case—of multiple telangiectases.

? Tumour of the Parotids involving the Skin.

By H. G. ADAMSON, M.D.

THIS middle-aged man has had a swelling on either side of the face since last Christmas. It came first on the left side, then on the right. It seems to occupy both parotids and the submaxillary gland on the right side, and to extend, so that it involves the skin and subcutaneous tissue to the nasolabial fold and the eyelids, and a part of the forehead. He had an injury below the right eye, and there was a "lump" there for two and a half years. Apparently the other swelling has only come since Christmas. I cut a piece out of the skin margin of the nasolabial fold, and it shows infiltration of the lymphatic spaces with epithelial cells. I have taken a piece from the parotid gland, but have not had an opportunity of examining that. When he first came to the hospital it struck me it might be some form of Mikulicz's disease, but further investigation showed that this was not so. His Wassermann was negative, and the blood count normal.

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[1] *Proceedings*, 1910-11, iv (Clin. Sect.), p. 43.

There seems to be no doubt that it is a malignant epithelioma of some sort. What I do not understand is how it started, and why it should affect both parotids. It is reasonable to suppose he had an epithelioma from the wound in the cheek, but I do not know how the parotids have become involved. If it is a primary parotid new growth, it is curious that it should start almost simultaneously in both parotids and in the submaxillary gland. There are cases described of carcinoma of one parotid, involving the skin secondarily.

With regard to treatment, I suppose there is nothing to be done.

(This case will be published in detail later in the *British Journal of Dermatology and Syphilis*.)

DISCUSSION.

Dr. Eddowes: Is it possible that this may be a case of so-called "hard œdema"? I have only seen one such case in the whole of my medical experience, and that occurred in a leg after fracture of the bones. The œdema lasted many months, and then slowly disappeared.

Dr. Gray: The section of this case shows exactly the appearance of a cuirass cancer—i.e., dilated lymphatic vessels blocked with epithelial cells—and the clinical characters also show the same. As to the condition being on both sides of the face, probably this is due to its arising from a common focus, which is not in the face. Most cuirass cancers are secondary to a deep-seated growth, and they generally appear on both sides of the middle line. There may be some such growth not yet determined in this case. This is certainly not a case of the ordinary parotid endothelioma of the mixed celled type, and I am not familiar with epithelioma of the parotid, though it probably occurs. With regard to the type of cell which one sees in these dilated lymph spaces, I do not think one can tell in what organ the primary growth is situated. I have seen secondary deposits in the skin from tumours in the abdomen and in the breast, some of them columnar carcinoma primarily, others of scirrhus type, and yet the cells in the lymphatic spaces seem to be of the same amorphous character. I do not think I have seen this type of secondary skin tumour arising from a primary skin tumour, therefore I do not know whether one gets typical prickle cell groups.

The PRESIDENT: This is an unusual condition. I agree with Dr. Gray that a growth secondary to a primary skin growth is almost unimaginable. If it is of the nature of a cuirass-spreading growth, it has almost certainly started from some non-epiblastic structure, so far as the upper epiblast is concerned, for one must consider the breast as epiblastic I mean epithelial in the true sense of the word. The diagnosis here must depend on a thorough microscopical examination and watching of the case. I had hoped, when I saw the patient, it might be a fibro-chondromatous mass, but the microscope puts that quite out of account, as well as symmetrical lymphangitis which Dr. Eddowes suggested.

Lupus Erythematosus of Unusual Distribution and Character.

By A. EDDOWES, M.D.

I SAW this woman yesterday evening for the first time, and at first I was only shown the lesions on her feet, which looked like a very superficial lupus vulgaris. But on inquiry I found there were lesions on both upper arms, and when I pinched the skin I found distinct atrophy, but the epidermis appeared to be fairly normal on the surface. Symmetrically on the right side is a patch which is at present erythematous, without atrophy. And I noticed that a portion of the pinna is destroyed; by a chilblain, she says. It was that which led me to diagnose the condition as lupus erythematosus. Of the different lesions the erythema seems to be the essential feature, the gradual necrosis and the epithelial changes being secondary. The atrophy can take place without obvious disturbance of the superincumbent epithelium. Over the centre of the face the skin is rather waxy looking, without any obvious affection being present. Pressure failed to reveal the presence of "apple jelly" nodules in any of the lesions.

DISCUSSION.

Dr. J. H. STOWERS: I agree with Dr. Eddowes as to the nature of the affection of the ears, but I am not so satisfied as to the more extensive lesions. They have characters suggestive of dermato-syphilis and a Wassermann test is necessary to exclude this possibility.

The PRESIDENT: There is the foveated nature of the scar. The lesion on the arm is probably a vaccination mark. The scar is evidently composed of some general atrophy, but there is a more distinct atrophy of the skin in more or less rounded patches. That is not common in the ordinary type of lupus erythematosus, whereas the foveated scar is much more likely to occur in superficial atrophy of skin due to syphilitic infection. The diagnosis, so far, is not clear.

Section of Dermatology.

President—Sir JAMES GALLOWAY, K.B.E., C.B.

Further Report on Case of Extensive Dermatitis, shown on December 20, 1917.[1]

By E. G. GRAHAM LITTLE, M.D.

THREE months ago this case presented a difficulty of differentiation between mycosis fungoides and a widespread coccogenic eruption. At the suggestion of several members he was taken into hospital, and has been under my observation two months, to ensure continuous dressings and other treatment. The lesions were of the seborrhœic type, that is to say, greasy, raised, slightly scaly patches; but these have now practically disappeared under treatment. The obstinate patches, which made me hesitate about the diagnosis, are those on the lower extremities as well as those on the lower part of the abdomen. They still cause me difficulty; they are still raised, and they do not heal readily. The question is whether I was not wrong, and whether those who suggested it was mycosis fungoides were not right. He is now in a stage in which it is, perhaps, more easy to differentiate. I have brought a section, which is again, I am afraid, dubious. It shows a considerable infiltration for eczema, but perhaps not enough for mycosis fungoides. I still think, and hope, it is seborrhœic eczema. At first it was seen by Sir Malcolm Morris and diagnosed by him as mycosis fungoides, and Dr. Dore and some other members supported that view.

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[1] At a meeting of the Section, held April 18, 1918.

DISCUSSION.

Dr. PRINGLE (Chairman): When this case was first shown I considered it would get better with prolonged baths and antiseptic applications. I still think that it is a pyodermia. The improvement which has taken place is very striking, and is not compatible with the diagnosis of mycosis fungoides.

Dr. S. E. DORE: I still maintain that this is mycosis fungoides. I suggest that X-rays would be useful for diagnosis. Patches of mycosis fungoides clear up with such rapidity under X-rays that it would be useful to try them here on a single patch. I do not think pyodermia or seborrhœic eczema would clear up so rapidly.

Dr. GRAHAM LITTLE (in reply): X-rays have been tried on the right leg only, but I cannot see much advantage, so far.

Syphilitic Rash first appearing after Twenty-three Injections of Novarsenobillon.

By J. L. BUNCH, M.D.

THE patient, a private in the Army, had a chancre eight years ago, which was diagnosed by a well-known surgeon as syphilitic, and treated by him with mercury injections and otherwise for three years. No skin eruption was seen then, or at any time until five weeks ago. At the end of the three years' course of treatment the patient was pronounced free from syphilis.

In November of last year his blood was examined at a London military hospital, and the Wassermann reaction was found to be positive. So he was ordered intravenous injections of novarsenobillon twice a week, 0·3 or 0·5 grm., and intramuscular injections of mercury once a week. After twenty-three injections of novarsenobillon he developed a rash, which commenced on the arms, near the sites of injection, and on the buttocks, also near the sites of injection. The injections were then discontinued. The rash spread down the forearms, but did not involve the hands, down the thighs above the knees, and later to the trunk. The face was not involved.

When seen by me, the lesions were irregular in distribution, erythematous, slightly scaly and definitely follicular. There was adenitis of the groin, and the epitrochlear glands were enlarged. The tongue and throat were clear. There was no sign of a chancre anywhere. The diagnosis lay between a syphilide, in which case the lesions corresponded in character to a secondary eruption, and pityriasis rosea. If a syphilide, it appeared that it must either be the result of a second infection (of which there was no evidence) or a lighting up of the eight-year-old infection, giving rise to a very anomalous rash. And this after twenty-three injections of novarsenobillon! Or, in view of a certain amount of itching, there was the possibility that it was a very abnormal case of pityriasis rosea. Before the course of injections the Wassermann reaction is said to have been positive, after twelve injections negative, and after another eleven (twenty-three in all) again positive. I had it done on April 9 (nine days ago) and it was positive.

At present date (April 18) the distribution of the rash is such that the face, the hands and the legs below the knees have escaped. The forearms, the thighs and the trunk are most affected, and the lesions are scattered and show but indefinite signs of grouping. They vary in size from small follicles to patches the size of a shilling, are erythematous or pinkish in colour, and some are definitely scaly. There are no lesions on the elbows or knees, and the patches in no way resemble psoriasis. Some of the patches are fading, but the better marked spots most closely resemble a follicular syphilide. There is no evidence of a chancre; the throat is not affected, but there is some adenitis.

I propose to excise one of the patches and examine sections for the *Spirochæta pallida*.

DISCUSSION.

Dr. PERNET: The mode of development and appearances of the rash are those of a secondary syphilide, especially the follicular grouping. Histories are so fallacious that it is wise to disregard them. I have found that the Wassermann reaction is always negative in pityriasis rosea, except when the latter occurs in an old syphilitic, when the reaction may be positive.

Dr. GRAY: I am convinced that this is not pityriasis rosea, because it is a very definite follicular hyperkeratosis, and does not correspond with the ordinary characters of that disease. The only two conditions that this case resembles are a follicular tuberculide or a follicular syphilide. The former is an extremely rare condition in an adult, and only occurs in people who show definite signs of tuberculosis. This patient shows no such signs, and I am therefore of opinion that it is a follicular syphilide. This appears to be an example of over-treatment by the arsenical preparations. Such cases are not uncommon when mercury is pushed so as to produce a marked lowering of the body resistance, and I believe that similar effects can be produced by arsenic, though they are perhaps not seen so frequently, as people are mostly rather cautious in the exhibition of this latter drug. I believe however that many cases of cerebro-spinal syphilis following the extensive use of the arsenical preparations are due to this cause. The only way of conclusively proving the syphilitic nature of these lesions is to find the spirochæte in the affected tissues.

Dr. GRAHAM LITTLE: I do not regard this as pityriasis rosea: I do not think it is, clinically, like it. I regard it as a syphilide, not necessarily an early secondary syphilide. It is not much against this being syphilis that the patient should have a rash after having these injections. I was nearly bringing to-day a very difficult case of a syphilide in a woman who had had syphilis twenty years, and had lesions of it all that time. She has had continuous and effective treatment, yet apparently it has made no impression on the rash.

Dr. A. EDDOWES: I believe that when a remedy fails at first after a fairly good dosage has been given, it is useless to go on with small doses afterwards. I think the colour of some of these patches is due to arsenic.

Dr. S. E. DORE: I agree with previous speakers in regarding this case as syphilitic, rather than as pityriasis rosea. I do not think there is any impossibility in this type of eruption occurring so long after the initial syphilis: the more one sees of syphilis the more difficult it is to date the eruption. It is possible that when these arsenical preparations are given the eruption may be postponed, and what is generally an early eruption may occur later when treatment is stopped. This patient has had a large total quantity of arsenobenzol, and over-treatment may lower the patient's resistance, but I have never seen a relapse so soon after such thorough treatment as this.

Dr. F. PARKES WEBER: Part of the cutaneous condition in this case is follicular, and it is possible that in the chronic follicular types of cutaneous eruption which occur in the secondary and the tertiary stages of syphilis the spirochætes become in some way protected by the hair follicles and sebaceous glands.

Dr. PRINGLE: In my opinion this case is morphologically a characteristic grouped small follicular syphilide, and I can formulate no alternative diagnosis. As to its occurrence at this period of the disease and after treatment of such intense character and long-continued duration, I can only say we all have seen the occasional persistence of syphilis under every form of treatment, ancient and modern. Only in the last month I have observed two cases of recurrent syphilides of a type which we would call "secondary," in men who have been treated for two years, with all the intensity of mercurial and arsenical administration at our command. They had been treated in France and in this country with salvarsan, neo-salvarsan, and galyl intravenously, and persistently with mercury intramuscularly, yet they still have active syphilis, and neither is a cachectic individual.

Case of Acute Generalized Lichen Planus treated by Lumbar Puncture.

By GEORGE PERNET, M.D.

THE patient, a thin, ill-nourished woman, aged 54, was first seen by me at the West London Hospital on March 22, 1918. The duration of the disease was two months. It began on the arms and spread quickly to neck, back, and body generally, and then to the legs. She described the irritation as "dreadful," allowing of no sleep night or day. She was admitted as an in-patient on March 26, and a lumbar puncture was carried out the same day (8 c.c. of cerebro-spinal fluid being removed, though 6 c.c. is sufficient) and reported on as follows: No lymphocytosis; Fehling reaction present.[1] The results, *qua* severe pruritus, were very good, as is usual, and the rash itself became pale and began to involute. Whilst in hospital the patient was on mist. sacchar. usti, but is now having a mist. hyd. biniod. to follow up the effects of the lumbar puncture.

This is the third case of acute lichen planus treated by lumbar puncture I have had at the West London Hospital (all women) with good results,[2] and following in the footsteps of Thibierge and Ravaut.

DISCUSSION.

Dr. PRINGLE: The case is a typical lichen planus, and I ask the experience of Fellows present as to the utility of lumbar puncture in this disease. I have had the procedure carried out several times and the immediate result has almost always been satisfactory, the relief to the itching being great and gratifying. But after a few days it has usually recurred, and I have never considered a second puncture advisable.

Dr. J. L. BUNCH: I have had only one such case, and although the puncture relieved the irritation very considerably, I do not think there was any benefit to the rash: any improvement in the rash I attributed to other measures. When I was working in Paris, Thibierge was doing it in all his lichen planus cases, and as his object was to relieve the itching, he repeated it, and with some success. I think he has always trusted to external treatment to cure the rash.

Dr. F. PARKES WEBER: In this case the correct explanation may be that the lumbar puncture was the turning point because it gave the patient, who had been worn out by intense cutaneous irritation and sleepless nights, a few nights' good rest. The nervous system has undoubtedly much to do with the course of, and recovery from, lichen planus, and in the present case a few nights' rest and cessation of the itching probably assisted the ordinary remedies in improving the cutaneous condition.

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[1] *Vide* Pernet, *Ann. de Derm.*, 1913 and 1916.

[2] One of the two other cases was shown at the Section of Dermatology, 1913 (*Brit. Journ. Derm.*, xxv, 1913, p. 261).

Three Cases of Fibroma Molluscum.

By G. W. Sequeira.

A man, aged 39, and his two sons, aged 11 and 8 respectively. The father states that he has had the "lumps" since he was aged 8, and they have continued to come out ever since. The two boys are said to have had them since birth. There are two other children, a girl aged 5, and another boy aged 20 months, but neither present any signs of the disorder.

The father presents about the body and limbs a great number of rounded and pear-shaped tumours varying in size from a pin's head to a small tangerine orange. Some of the growths are pedunculated and others are flat masses embedded in the corium. As usual in these cases of fibroma molluscum, the patient presents many brownish pigmentary stains on the trunk. The tumours are quite painless, thus differing from the special type of multiple fibromata described by von Recklinghausen, which are made up of fibrous and nervous tissue neurofibromata. There is no perceptible thickening of the nerves in the cases shown, which also differentiates them from neurofibromata.

The special interest in the present cases lies in the fact that the lesions in the two boys have yielded in a striking way to internal treatment. For the last two months they have been taking thyroid tablets in daily doses of ½ gr., which in the elder boy has been recently increased to ¾ gr. The growths have now practically disappeared and no fresh tumours have arisen. Fibrolysin has been advocated in these cases, but no special benefit from such treatment has been reported.

In a case shown by the late Dr. T. D. Savill at the Dermatological Society of Great Britain and Ireland in May, 1901, some of the tumours had been somewhat reduced in size by the local application of ethylate of sodium which produced a cicatricial contraction of the cuticle.

On the whole, surgical intervention hitherto seems to have been regarded as the only reliable method of dealing with these cases.

Up to the present time the father has not been subjected to any treatment.

DISCUSSION.

Dr. G. W. SEQUEIRA: Do members think that thyroid is likely to be successful in the father's case?

Dr. F. PARKES WEBER: I think that cautious treatment of the father with thyroid extract could do no possible harm, and it might have good effects on the general condition of the patient. It is just possible that it might also diminish the molluscous fibromata—at least temporarily. Neither thyroid treatment nor fibrolysin can bring about any permanent cure. Any results in familial cases of this kind are of special interest, because of the certainty of the diagnosis when the condition occurs in several members of the same family. Both the children shown to-day have typical patches of cutaneous pigmentation. Thyroid treatment has been tried in some of these cases, but I cannot remember that any striking results have been reported.

Case for Diagnosis.

By W. KNOWSLEY SIBLEY, M.D.

THIS man is a Russian, aged 32, whom I saw for the first time this afternoon: he was sent to me by a medical board. He is covered with folliculitis all over his body, with the exception of palms, soles and head and neck. He says he has had the condition for six years on and off, and that it always comes out in cold weather. On the front of the chest, and in the interscapular regions there is distinct pustulation. A fortnight ago I had almost an identical case in a Russian subject, also sent from a recruiting board. He told me that it was the first attack he had had, and that it came out immediately after a hot bath. He presented typical folliculitis, and the skin areas between the hair follicles were not inflamed. The condition of the skin, when I saw him, was intolerable, and he was obviously ill. He had no lesions on the hands and feet nor on the head and neck. There was no special line of demarcation anywhere. I took him into hospital, and in a week the condition had more or less disappeared, and his skin presented a general desquamative condition. But the day before he was to leave the hospital, the whole folliculitis broke out again. I cross-examined the nurse, but could get no information. As it had again quieted down, he left the hospital a few days afterwards. A week or two ago he was again sent to me, from another recruiting board, with a recurrence of the general folliculitis and pustulation all over the thighs, the same as this man now shows between the shoulders. In the other case I thought the man had taken a bath with some corrosive substance in it. But if that was the case I could not explain the escape of his feet and his hands.

DISCUSSION.

Dr. Bunch: One of my patients has an almost exactly similar condition to that in Dr. Sibley's case, and he too is a Russian, and a tailor, but no relation or neighbour of this patient.

Dr. Gray: I saw quite recently at a military hospital two cases exactly like this, though not so intense. Both the patients were Russian Jews, and the sergeant-major there told me they had had eight previous cases, all having started with the men going to certain baths in the neighbourhood. This case of Dr. Sibley's was on the point of being called up, and the ten cases at the hospital were due for service abroad. I have not the least doubt that the condition was artificially produced in order to avoid service.

Dr. S. E. Dore: I was asked to see a similar case in a military hospital last week. The patient was a Polish Jew and was covered with a pustular folliculitis which had occurred a few days after vaccination and a vapour bath. He denied having applied any irritant to his skin but, in view of these other cases, it would be interesting to know if some caustic application such as croton oil or turpentine had been used. This was the second case they had had at this hospital.

Dr. F. Parkes Weber: I feel convinced that the true explanation is a different one from that suggested. I think that the cause of the recurrence or exacerbation of the eruption in such cases is a traumatic one: that a hot water bath, and especially a vapour bath, combined with rubbing of the skin, re-infects the patient with micro-organisms which had become quiescent in the hair follicles. I think that I have also seen the condition occur locally in patients of my own after the prolonged application of wet fomentations to particular areas (for pains, &c.). Whether the fomentations are alkaline, or whether the fluid in the bath is alkaline, does not affect my theory—namely, that the microbes may remain quiescent in the hair follicles, and that in some way they may be stirred up and become active again, as the result of hot water or vapour ("Turkish") baths, and sweating, and the associated rubbing of the skin.

Dr. Eddowes: Examining these lesions under a lens, I find the hairs mostly excentric; suggesting that they start in the mouths of sweat-pores. I have seen a similar rash after three or four days' application of carbonate of soda. Do these men have a bath and afterwards rub soda or some other alkali on the skin? And does this neutralize the contents of sweat-pores while the sebaceous and hair follicles are protected by their own grease?

Dr. Pringle: The eruption is an obvious artefact and the case an extremely important one. The simultaneous observation of a considerable number of identical cases in Russian Jews, summoned to appear before tribunals for service, and always appearing after hot baths, is of curious and sinister significance.

Section of Dermatology.

President—Sir JAMES GALLOWAY, K.B.E., C.B.

Punctate Keratosis of the Palms of the Hands and Soles of the Feet; two Cases occurring in Cousins-german.[1]

By Sir JAMES GALLOWAY, K.B.E., C.B., M.D. (President).

WITH HISTOLOGICAL AND BIBLIOGRAPHICAL NOTES.

By H. G. ADAMSON, M.D.

THE following is a short note of this unusual condition, occurring in two patients, husband and wife, who have the blood relationship of first cousins-german.

Mrs. M., aged 35, was brought to me on March 3, 1914. She is not a robust woman, and suffered some years ago from tetany following influenza. To keep this condition in check she has taken 30 gr. of bromide of ammonia daily for a long time. The tetany has now practically disappeared. She has a sluggish circulation, evidenced by a tendency to unusually cold or unusually warm hands and feet. She states that she has suffered from her girlhood or infancy from the present condition, affecting the palms and soles, but she cannot say when it was first noted.

On the palms of the hands (fig. 1) large numbers of yellowish-brown, flattish nodules can be seen, in size from minute points to that of a millet grain. They are disseminated over the whole palm to below the crease of the wrist; they are now stated to be spread along the finger clefts. These nodules seem to be papules of very firm, horny epithelium; they are not vesicular. In process of time they tend to flatten, and at length become depressed below the surrounding surface. Many of them show definite pitting of their summits or centres.

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[1] At a meeting of the Section, held May 16, 1918.

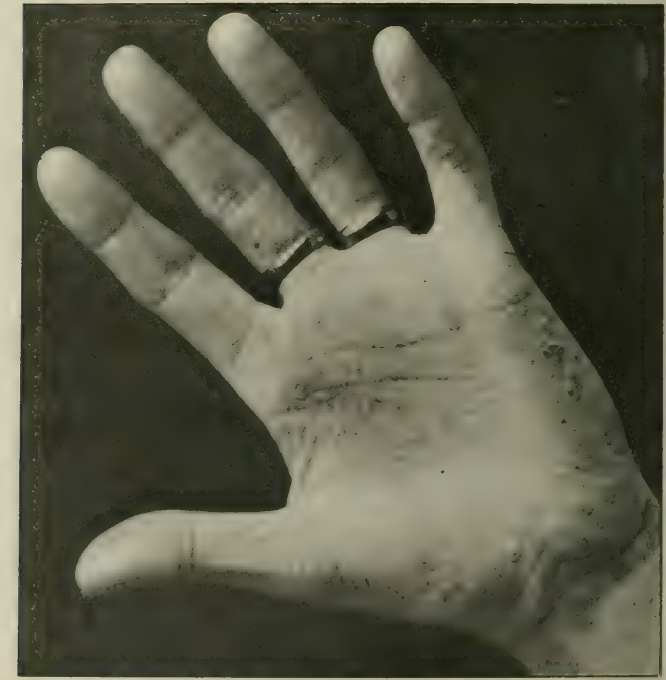

Fig. 1.

From a photograph of the palm of the left hand (Mrs. M.), showing the distribution and appearance of the nodules.

These nodules are stated to have recently increased in number, spreading along the fingers towards the finger clefts. A similar condition affects the soles of the feet.

The nodules themselves cause no pain. She is in the habit of removing, with the point of a penknife or similar instrument, those that become troublesome. The result is that the skin becomes roughened. The irregular epithelial surface incommodes her by being caught by her clothes and prevents her doing needlework. The inconvenience is at times almost intolerable.

Her husband, Dr. M., suffers from a similar though less severe condition of the skin of the palms and soles, the nodules being less numerous and causing slight inconvenience.

In the case of Mrs. M. the use of salicylic acid in various forms to produce desquamation and smoothness of the surface has done good. Carefully measured application of X-rays has also been applied without benefit.

Dr. Adamson adds a short account of similar cases on record. A case has been described by Dr. Albert J. Chalmers.[1]

It is clear that certain of the small number of cases described as "punctate keratosis" are not of the same nature as the two cases described in this communication, nor do they agree with each other in their ætiology and external characters.

In these two cases the condition appears to be a family complaint.

Although the previously published cases have many features in common, it is doubtful whether they all belong to the same class.

A punctate keratosis on the palms and soles may occur in lichen planus; in arsenical keratosis (Darier, Adamson); in syphilis (Brocq): in psorospermosis vegetans of Darier (Darier, Emery, Gaston and Nicolan); in linear nævus (Dubreuilh); and in association with verrucæ planæ. It is possible that some of the recorded cases of punctate keratosis of the palms and soles belong to one or other of these affections.

The following is a list of recorded cases of punctate keratosis. By some observers these have been called porokeratosis, but as it is uncertain that the keratosis really involves the sweat-duct orifices, it is better perhaps to use the term "punctate keratosis" rather than "porokeratosis."

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[1] *Journ. of Trop. Med. and Hyg.*, 1917, xx, p. 121, a précis of which will be found in the *Brit. Journ. Derm.*, 1918, xxx, p. 48.

LIST OF PUBLISHED CASES.

(1) Besnier. "Kératodermie erythémateuse symétrique des extrémités ; forme ponctuée. Keratose localisée à l'ostium sudorifère. Paume de la main." In the Museum of the St. Louis Hospital is a Baretta model (No. 560, 1892) which Besnier has labelled.

(2) Hallopeau and Clause. *Bull. de la Soc. Franç. de Derm. et de Syph.*, 1891, p. 117 ; *Ann. de Derm. et de Syph.*, 1891, p. 226.

(3) Hallopeau. "Sur une hyperkératose palmaire et plantaire localisée aux orifices sudoripares et sur le rôle des orifices glandulaires dans les néoformations épidermiques," *Ann. de Derm. et de Syph.*, 1895, p. 480.

(4) Emery, Gaston, and Nicolan. "Verrues familiales héréditaires avec dyskératoses systématisées disséminées et à répétition (type psorospérmose folliculaire végétante)," *Ann. de Derm. et de Syph.*, 1902, p. 1014.

(5) Mantoux. "Porokératose papillomateuse palmaire et plantaire" (plates and drawings of clinical and histological appearances), *Ann. de Derm. et de Syph.*, 1903, p. 15.

(6) de Beurmann and Gougerot. "Porokératose papillomateuse palmaire et plantaire," *Ann. de Derm. et de Syph.*, 1905, p. 629.

(7) Balzer and Germain. "Kératodermie avec porokératose en godets épidermiques localisés à l'ostium sudoripare et disseminés à la paume des mains" (case and literature), *Ann. de Derm. et de Syph.*, 1905, p. 633.

(8) Balzer and Boyé. "Kératodermie palmaire avec porokératose," *Bull. de la Soc. Franç. de Derm. et de Syph.*, 1909, p. 9.

(9) Buschke and Fischer. "Keratodermia maculosa symmetrica palmaris et plantaris" *Ikonographia Dermatologica* (Neisser and Jacobi), 1906.

(10) Chalmers, A. J. "Keratodermia punctata" (illustrations of clinical and histological features), *Journ. of Trop. Med. and Hyg.*, 1917, p. 121.

Of these cases of Besnier's the whole of the palm was keratotic and covered with punctæ. In the cases of Hallopeau and Clause and of de Beurmann and Gougerot there were keratotic streaks riddled with crateriform pits, and it seems possible that these were really examples of linear nævi. In Mantoux's case and in that of Balzer and Germain, the lesions did not cover the whole palm, and were of rapid evolution, and suggest the possibility of multiple plane warts. In Balzer and Boyé's case they were gradually evolved during several years in a man aged 66. There were forty to sixty pits on each palm. They appeared to begin as vesicles, which then became warty and formed a crater by separation of the little warty growth. In Chalmers' case there were very numerous tiny craters scattered over the palms and palmar surfaces of the fingers. They evolved during eight to nine months, and had the appearance of a circular heaped-up ring round a sweat orifice. Many exuded fluid at the apex and formed a central crust, which came away and left a cavity. Chalmers compares his case with those of arsenical keratosis, and suggests a toxic origin, but without evidence of arsenical poisoning.

The case recorded by Buschke and Fischer seems to have a closer resemblance to the two cases now described, and it was the similarity of appearance of their coloured plate which suggested the diagnosis in these cases. The plate shows about 200, 1 to 4-mm. large epidermal efflorescences scattered over normal skin. Each lesion is an opaque yellowish-white, hard, horny nodule, without any erythematous halo. The patient, male, aged 40, had noticed the affection for about sixteen years; no other cases in the family.

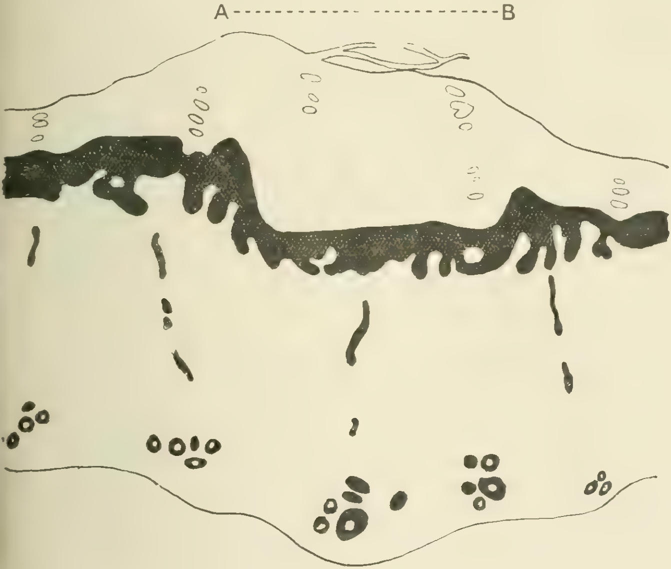

FIG. 2.

Diagram showing thickening of horny layer at A—B, but apparently no other change. There is no evidence of inflammation.

Histology.—Pieces of skin removed included one or more of the warty lesions, cut in serial sections to obtain every part of the lesion.

The findings were mainly negative. There was a considerable thickening of the horny layer at the site of a warty lesion (fig. 2). There were no changes in the sweat coils or ducts, which latter passed through the affected area as through the normal skin. There was no evidence that the warty thickening began around the sweat ducts, nor of inflammatory changes in the corium; nor of any alterations in the prickle-cell layer of the epidermis—no acanthosis and no porokeratosis. The absence of changes in the corium, sweat-apparatus and Malpighian layer of the epidermis, with merely a thickening of normal horny cells, corresponds exactly with the histological findings of Buschke and Fischer.

DISCUSSION.

Dr. H. G. ADAMSON: This is a very rare complaint. I have been able to discover the records of only ten cases. Most of them have been called porokeratosis, and Besnier so described the first case, and put a model of it in the St. Louis Museum. I think the cases are not identical. Clinically, they all had this punctate keratosis, with the tendency for the plug to come out and leave a hole. I am certain some of the cases described under this head have been small plantar warts, and some others are lichen planus. I think the two cases now exhibited are nævi.

Dr. F. PARKES WEBER: I hold the view, very strongly, that these lesions are of the nature of nævus. Certain similar cases have been published in which the keratosis is on a superficial telangiectatic basis. I have sent to the *British Journal of Dermatology* an account of a case which bears somewhat on these.[1] It is that of a healthy lad, who has a congenital vascular nævus, mostly superficial, involving a great part of the left lower extremity, including the gluteal region. On part of this area there are hyperkeratotic outgrowths, which can be knocked off, or torn off, and, naturally, tend to recur. They sometimes look like ichthyosis hystrix when they are very well developed. In my case there can be no question that the lad has a congenital vascular nævus of one limb, a variety of "nævus unius lateris," and in my description of the case, I have referred to cases of congenital or familial hyperkeratosis involving the hands and feet, associated with apparently congenital telangiectasis of the parts involved. I do not mean that the nævus is necessarily congenital, but that the condition was at all events potentially present at birth.

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[1] *Brit. Journ. Derm.*, 1918, xxx, p. 89.

Case of Hodgkin's Disease with Prurigo.

By J. M. H. MACLEOD, M.D.

THE patient, aged 34, ten months ago began to suffer from spasmodic itching, especially about the lower extremities; prurigo-like papules appeared later, chiefly on his thighs and legs. Two months later the glands in the axillæ, neck, and groins, began to enlarge progressively. When first seen I thought the eruption might be scabies. I was unable to detect a burrow or an acarus. Seeing the enlargement of his glands I thought the case was lymphadenoma associated with a pruriginous eruption. The blood-count was: Red blood corpuscles, 5,200,000 per cubic millimetre; hæmoglobin, 80 per cent.; colour index, 0·78 per cent.; leucocytes, 9,000 per cubic millimetre; polymorphonuclears, 52·5 per cent.; small lymphocytes, 30·5 per cent.; large lymphocytes, 4 per cent.; large hyalines, 7·25 per cent.; eosinophils, 5 per cent.; basophils, 0·75 per cent. A gland removed from the axillæ on microscopical section showed a simple glandular hyperplasia.

DISCUSSION.

The PRESIDENT: One is justified in calling this case Hodgkin's disease. An interesting feature is the associated pruriginous skin eruption.

Dr. E. G. GRAHAM LITTLE: I reported a case of lymphadenoma with the late Dr. Lee Dickinson,[1] occurring in a youth with very numerous red and yellow cuticular pruriginous nodules. He died with generalized lymphadenoma. The cuticular tumours were of lymphatic structure, neither lymphocytosis nor myelocytosis existed. In the present case the eruption is obviously the result of scratching. The eruption, as such, is not characteristic of lymphadenoma, but, as in my case, pruritus is a symptom of lymphadenoma, while the eruption results from scratching. The French school regard leukæmia and pseudoleukæmia as merely clinical types of the same disease, and it is unimportant whether the name applied is leukæmia or lymphadenoma, although the nature of the cell, whether lymphocytic or myelocytic, needs differentiation. (*Vide* Nicolan, *Annales de Dermatologie*, 1904, p. 754: "La leucémie et la pseudoleucémie, envisagées pendant longtemps comme deux espèces morbides distinctes, finiront par être reconnues par la majorité des auteurs, du moins en France, comme deux variétés d'une seule et même affection, la lymphadénie.")

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[1] *Brit. Journ. Derm.*, 1902, xiv, p. 219.

Dr. F. Parkes Weber: This case is almost a repetition of a case that I have just published jointly with Dr. Dove.[1] I differ from the views expressed by Dr. Graham Little, inasmuch as I regard the typical skin affection of Hodgkin's disease—i.e., lymphogranulomatosis—to be itching, which, in most cases, precedes the obvious enlargement of lymphatic glands. This pruritus is generally associated with a pruriginous eruption of papules. In fact one may speak of it as a prurigo, and the variety of Hodgkin's disease associated with such a pruriginous condition may be termed "lymphogranulomatosis pruriginosa." In the case referred to by Dr. Graham Little there was certainly *leukæmia cutis* present—i.e., a genuine nodular leukæmic infiltration of the skin, totally different from the prurigo-like eruption of lymphogranulomatosis. According to Dr. Little there was also pruritus, but was it sufficient to give rise to violent scratching of the skin and make the disease imitate scabies? In my case, and in several others, scabies was the diagnosis which had at first been made. The pruriginous eruption (or simple pruritus) which precedes the glandular enlargement in lymphogranulomatosis pruriginosa may be regarded as a *pre-granulomatous* condition, analogous to the pre-mycotic (pre-granulomatous) eruptions of mycosis fungoides.

The President: I agree with Dr. Parkes Weber's differentiation. Leukæmia cutis must be differentiated from the eruption preceding the other clinical manifestations of Hodgkin's disease.

Dr. MacLeod (in reply): I agree that this type of eruption is quite distinct from that which occurs in leukæmia. I recall the remarkable case of Dr. Rolleston and Dr. Wilfrid Fox, in which leukæmia was associated with nodules all over the skin, which were clinically and histologically different from the prurigo papules in this case.

Case of Granuloma Annulare.

By E. G. Graham Little, M.D.

The patient is a single woman, aged 40. The disease has existed for a year. The lesions are few and on the left hand. There are four well defined rings about the area of a sixpence, made up of glistening white papules, on the knuckles, and on the thumb there are some discrete pearly nodules not yet ranged in ring formation. These are especially characteristic. I have pointed out that the substance of the ridge which forms the ring can be seen to be constituted by individual similar nodules, so that the ridge is not a homogeneous structure, but is really a series of minute elevations. Itching is present. In a previous paper I suggested that tuberculosis was a frequent family history in these cases, as in this instance, both on the paternal and maternal sides. The disease remains a rare condition, of very obscure causation.

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[1] Weber and Dove, "Pruritus in Hodgkin's Disease—Lymphogranulomatosis pruriginosa," *Brit. Journ. Derm.*, Lond., 1918, xxx, p. 15.

Mycosis Fungoides.

By W. Knowsley Sibley, M.D.

The patient, aged 66, is a moulder in an oil mill. There is no family history of skin disease, and he has never been ill before. He has suffered for some years from dry skin with a varying degree of irritation on various parts of his body, attributed by him to working in a hot mill, since most of the men in the mill suffer similarly. Eighteen months ago the irritation became much worse, especially over the shoulders and dorsal region, and the skin became rough and thickened in places. More recently the distress, irritation and burning has become much worse, and there is profuse weeping of clear serum from some of the lesions. The greater part of the skin of the dorsal and lumbar regions now presents large areas of well defined infiltration. Some are simply vascular pigmented lesions, others raised and corrugated. Lesions of a similar nature occur in both flanks, spreading towards the abdomen, others are present on the calf of the right leg. The head and neck, upper limbs and left leg, are free. The glands, especially in the right groin, are slightly enlarged. The Wassermann reaction in all dilutions is negative.

The patient states that formerly he did not perspire even when very hot, but for the last eighteen months he has done so profusely, especially over the right side of his body. A week ago I gave him an X-ray treatment over three areas in the dorsal region, and the condition now is said to be more comfortable.

In a case I had a few years ago—a younger man with similar tumour formations on various parts of the body—a full dose of the rays always caused a tumour to disappear, and this was successfully applied to fresh tumours. That went on for a few weeks, and then the patient developed a severe sore throat, with enlargement of the tonsils. I diagnosed that he had a mycotic deposit in his tonsils. I never heard the end of the case. Had the effects of the rays dispersing the tumours anything to do with this?

Section taken from the right lumbar region shows the stratum corneum appearing practically normal and the stratum granulosum absent in places. The stratum mucosum is greatly thickened throughout the section, and the papillary processes are broadened and prolongating in different parts of the section. The dermis is composed of a mass of round cells and plasma cells and a very large number of dilated blood-vessels, some of which exhibit red blood cells.

The PRESIDENT: Dr. Sibley, no doubt, used the phrase "driving the tumours in" metaphorically, but there seemed to have been produced a toxæmia, which was very disastrous in its results. In mycosis fungoides, speaking of outward and visible signs, experience seems to favour the treatment of it by X-rays.

Case of Guttate Morphœa—Striæ et Maculæ Atrophicæ?

By G. W. SEQUEIRA.

THE patient, H.A., a member of the W.A.A.C., has recently returned from France, where she has been in hospital for many weeks suffering from "debility." Last August she noticed some redness on the left side of her neck which was accompanied by irritation. Surrounding the neck, collar-like, there are a considerable number of sharply circumscribed pearly macules varying in size from that of a pin's head to that of a threepenny piece, with a glistening scar-like appearance, soft and silky to the touch, and giving the sensation to the finger of small holes in the skin. The epidermis over the macules is wrinkled. Also, irregular linear striæ are seen interspersed among the macules, mostly in parallel lines in the direction of the lines of cleavage. The striæ in this case seem to be formed by the close union of the maculæ. The spots and streaks are somewhat depressed below the level of the surrounding skin. There are similar lesions on the legs about the knee, lower part of the thigh and the popliteal spaces, but in this region the malady appears to be in an earlier stage of evolution, as small red papules can be seen arranged in groups, with an erythematous tinge around them, blanching upon pressure.

Previous observers have stated in similar cases that no subjective symptoms are usually present, but in the case shown a good deal of irritation accompanied the first appearance of the spots. That there is inflammation and infiltration preceding the atrophy is here evident, as in cases reported by Liveing, Duhring and others.

The toxins of tuberculosis and syphilis can be excluded here.

DISCUSSION.

Dr. GRAHAM LITTLE: The excessive itching, the red spots which preceded the development of white patches and the distribution on the nape, seem to point very strongly to the diagnosis of atrophic lichen planus. There is also at the present time an indeterminate papular eruption on the inner surface of both knees which resembles that of lichen planus.

Dr. J. H. STOWERS: On the lower and outer aspect of the right thigh there are a few red, flat, shiny, angular papules, with a tendency to linear arrangement, and associated with itching. I regard the case as one of lichen planus atrophicus.

Dr. F. PARKES WEBER: This is a case of "white-spot disease," and is typical of it. I think there is no so-called "linear atrophy" of the skin, a term which is generally reserved for *striæ atrophicæ*, for which a still better name is *striæ cutis distensæ*. In "white-spot disease" there is a pearly white infiltration of the affected skin with some non-living material, and the favourite distribution of the change, about the neck, reminds one of the distribution of the diffuse superficial xanthoma, which sometimes occurs in advanced cases of obstructive jaundice. In both "white-spot disease" and in xanthoma there is infiltration of the affected skin with some non-living material. In the present case a few of the spots about the knee have the typical appearance of "white-spot disease," as those on the neck have.

Dr. H. G. ADAMSON: This is a case of "white-spot disease," otherwise morphœa guttata. It begins on the legs around the hair follicles, and suggests lichen planus, but I think it is not lichen planus atrophicus, because of the absence of atrophy. Here there is distinct infiltration.

Dr. S. E. DORE: This is a typical case of "white-spot disease." I showed two cases here some time ago with similar appearances. I have never seen the pearly-white lesions characterizing this disease in association with typical lichen planus papules. The fact that atrophy is the terminal stage of both has given rise to confusion with lichen planus atrophicus and linear and macular atrophy.

Dr. GRAHAM LITTLE: American writers who suggested the name "white-spot disease" themselves express doubt whether it represents an entity or merely a phase of other diseases. Thus McKee and Wise say: "We believe all the recorded cases of 'white-spot disease' can be divided into two groups—namely, the lichen planus group and the sclerodermic group." Stelwagon would add a third group, "striæ et maculæ atrophicæ," in which there has been no antecedent lichen planus or sclerodermia. I claim that this case is an example of a phase of lichen planus, and I see no advantage in retaining the name "white-spot disease," which is redundant and confusing.

The PRESIDENT: I think the case is one of guttate sclerodermia, or morphœa. The peculiar infiltration of the skin in this case differs from the atrophy which follows the atrophic form of lichen planus.

Section of Dermatology.

President—Sir JAMES GALLOWAY, K.B.E., C.B.

Lichen Obtusus Corneus.[1]

By W. KNOWSLEY SIBLEY, M.D.

THIS single lady, aged 31, is engaged on the land, milking cows; she asks if the papules on her fingers are due to her work. A papule first appeared on her left index finger six weeks ago; cow-pox was suggested. The lesion soon became solid; other lesions have since appeared on the fingers. She has now three on the left hand and two on the right. They are diffuse, very hard, more or less circular, and conical. Some are glistening, and appear to be solid corneous growths. At times they throb and burn, otherwise she has not had any irritation. The lesions are like the lichen obtusus corneus on a case I showed in July, 1916. On one of her hands there is a scar, and at the edge of it there is what might be taken for a lichen planus papule, but I am unable to detect any suspicious lichen lesions.

Case of pre-Mycotic Erythrodermia (early Mycosis Fungoides).

By GEORGE PERNET, M.D.

THE patient, a married woman aged 40, was first seen by me on May 28, 1918. She then gave a history of eczema of the legs and varicose veins four years previously. About a year ago the legs began to get red; the redness had spread to the thighs and trunk. In October, 1917, it involved the upper limbs and face. The main symptom has been, and is, intense irritation all over.

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[1] At a meeting of the Section, held June 20, 1918.

When I saw her, the upper and lower limbs were of a dull red, numb as they are now, glistening, and presenting marked lichenization and quadrillation of the skin, with some general superficial infiltration, but no desquamation. The legs nearly up to the knees exhibited a punctiform-purpura; and about the lower third of the left leg there was an elongated white depigmented area, atrophic looking, with scattered islets of pigmentation at the upper border, and an irregular one in the pale area. The body generally and the face and scalp were reddened, but to a lesser degree than the limbs. Some secondary adenitis is present. Her father and mother are living, aged 70 and 69. The picture was that of a *femme rouge*. I ordered X-rays and an effervescing quinine mixture; one exposure only (left thigh) has been possible, but will be repeated. The patient has improved. Recently she has had one or two abscesses about the right breast and axillary region. In my opinion the case is one of the pre-mycotic erythrodermia of mycosis fungoides.

DISCUSSION.

Dr. KNOWSLEY SIBLEY: I am sceptical about this being pre-mycosis, because one sees so many cases which are diagnosed as being of that nature, although mycosis fungoides is a rare condition. If they all get well, are we to attribute it to treatment, or to an error of diagnosis? At present I have half-a-dozen of these cases of cutis rouge under observation: I saw one this afternoon, an extraordinary condition, which the patient said had been present for two years. She had been in hospital for it for some months, and she is no better. Until there is a distinct localized infiltration of the skin, I think we should not speak of these cases as pre-mycosis.

The CHAIRMAN (Dr. LITTLE): I agree with Dr. Sibley that the diagnosis of pre-mycosis is much too frequently assumed, and I do not think the data here are convincing.

Dr. PERNET (in reply): I cannot agree that these cases are common. During the last six years at the West London Hospital, this is practically the first case of the kind I have seen, although I deal with a large number of patients. Further, mycosis fungoides takes many years to develop to the tumour stage.

Case of Dermatitis Herpetiformis.

By George Pernet, M.D.

The patient, a lad aged 17, is a baker working at night; the duration of illness four and a half years. I saw him first three weeks ago suffering from dermatitis herpetiformis. The rash has improved under salicin, although the intense pruritus persists. His occupation aggravates the condition, which calls for rest in bed. Originally the grouped vesicles and vesicularly bordered lesions, on an erythematous ground, forming rings and segments and arabesques by coalescence all over the body, but chiefly about the seats of election—viz., the small of the back and the axillary folds—presented a perfect picture of the disease.

Ichthyosis Hystrix Linearis of Palms.

By S. E. Dore, M.D.

This woman, aged 62, has had these curious symmetrical, warty, pigmented streaks extending along the palms and fingers and on to the dorsal surfaces of the terminal phalanges of fingers, for about one year. She states that she had a small lesion of a similar type on the left thumb twenty-four years ago and this disappeared, but the condition was not present before that time. I think there is not much to be done in the way of treatment. The cautery or very strong applications are to be avoided in a patient of this age. I have given unguent. acid. salicyl. B.P., applied after soaking the hands in hot water.

DISCUSSION.

The Chairman: The initiation of this disease at so advanced an age must be extremely rare; it is quite outside my experience.

Dr. J. H. Stowers: I consider the application of CO_2 and the subsequent use of salicylic acid plaster, to be an appropriate treatment in the present stage.

Case of (?) Miliary Syphilide.

By S. E. DORE, M.D.

THIS man came to me last October, with a good deal of redness and swelling of his nose, resembling simple rosacea. Subsequently ulceration took place and I diagnosed tuberculosis. X-rays were applied, and some dermatitis resulted, the disease being considerably aggravated. A few weeks later a few papules appeared on the upper lip, followed by similar lesions on the cheeks. The Wassermann test was negative. Subsequently he developed this eruption on his body, and this I regarded as a grouped miliary syphilide. He then had four injections of novarsenobenzol, but with no improvement in the lesions. He admits that he had a chancre thirty years ago, but it is a long time afterwards for an eruption of this type to develop.

DISCUSSION.

Dr. J. H. STOWERS: I support the diagnosis that this is syphilis; I recommend a course of mercury and potassium iodide in increasing doses. If this fails the case is probably one of tuberculosis cutis.

Dr. GRAY: I feel convinced that this is a typical case of lupus vulgaris, the rash on the body being a follicular tuberculide. This view is supported by the negative Wassermann and the failure to respond to the anti-syphilitic injections. I admit that, given a follicular lesion like this without other evidence, it is not easy to decide between syphilis and tuberculosis.

The CHAIRMAN: A patient I proposed to show this afternoon has not come. His eruption was like the follicular rash seen in this man, and I am inclined to diagnose a follicular tuberculide in both cases. In my patient there was a long and well-established history of syphilis.

Angiokeratoma.

By W. Knowsley Sibley, M.D.

This case was shown and described at the March meeting of the Section[1] and referred for further investigation. I now have to report his Wassermann was negative.

A series of sections which have been prepared from a lesion excised from the left thigh show that the chief change occurs in the stratum mucosum, consisting of a well formed hæmorrhagic cavernous space, which has its seat of origin in this layer. The covering of this space is composed of a thickened stratum corneum, the whole of the stratum granulosum, and a very thin layer of the stratum mucosum, the base of the space being formed by the remaining portion of the same.

On examination of its contents, it is found to be made up of red blood cells, serum, fibrin and leucocytes. In some of the sections there is a distinct destruction of the base of the nodule and a well-marked red-blood cell extravasation from the capillaries is seen to be taking place into the space itself. The only point of note is the presence of pigmentation, which is confined to the cells of the papillary layer. There is no change in the dermis or connective tissue, except a dilatation of the blood-vessels in this neighbourhood.

I have had the opportunity of examining a brother four years older, who has a similar condition of skin, but to a lesser degree. In this man, who is a soldier, the chief lesions are on the scrotum, the abdomen, and over the spinal processes, especially of the dorsal and lumbar regions. About the umbilicus a crop of larger lesions are to be seen, partially filling up the depression, and exhibiting small pedunculated vascular tumours. Scattered lesions are present on his left forearm and a few on the fingers, palm and wrist of this side. Many are present on the buccal mucosa of the lower lip and at the junction of the lip and upper jaw in front. One on the side of the left hand has rather a keratomatous character. This patient also suffers from chronic rheumatic pains, especially of the fingers, and from chilblains in cold weather. He has been rather deaf since having diphtheria at the age of 8, with occasional discharge from the left ear. The skin lesions were first noticed after this illness.

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[1] *Proceedings*, p. 70.

Case of Sporotrichosis.

By E. G. GRAHAM LITTLE, M.D.

THE patient was shown by me as a possible case of sporotrichosis in March, 1917.[1] At that time sporothrix was not found, the case being recorded as chronic ulceration of the legs. The patient, a girl, aged about 14, has suffered from these ulcers for nearly four years, and has been under my care for continually recurring ulcerations of the type now seen. These arise with nodules, which speedily break down and ulcerate, and the whole surface of both legs below the knee is scarred. The best treatment has been found to be considerable doses of pot. iod. At the meeting in March, 1917, the diagnosis of Bazin's disease was suggested. In the past few weeks, however, definite proof of the presence of sporothrix has been forthcoming, Dr. John Matthews having grown the fungus from an unopened nodule.

---

[1] *Brit. Journ. Derm.*, 1917, xxix, p. 125.

Section of Dermatology.

President—Sir JAMES GALLOWAY, K.B.E., C.B.

Case for Diagnosis.[1]

By J. H. STOWERS, M.D.

THE patient, a male, aged 53, married, is a clerk in a Railway Company's office. He lives under favourable conditions and his general health is satisfactory. The eruption commenced upon the outer part of the right thigh six years ago, and in a few months was followed by similar developments on both thighs and legs, upper arms, forearms and hands, including the fingers. Small papulo-tubercular elevations are also present upon the abdomen and back, including the buttocks. The original lesions were bluish-purple, subsequently assuming a yellow tint resembling "bruises," being followed by severe structural alterations. They have recurred in crops at intervals. Both the forearms and hands are much swollen. The surfaces now present innumerable clusters of indurated papules, some having slight central depressions and tubercles with deep pigmentary stainings and cicatrices due to the persistent necrotic action. Extensive uneven scarring and numerous irregular ulcers of limited size exist upon the discoloured and infiltrated skin. The clusters of agglomerated tubercles on the arms and legs are numerous, and, with the corresponding œdema, produce much swelling of the limbs, not unlike the appearance of Bazin's erythema induratum of severe type. No tuberculous glands are discoverable. There is no itching, but burning and pricking sensations interfering with sleep. Except for twelve months spent in New York the patient has lived in England. No family history of tuberculosis or similar affection exists. He is not syphilitic.

---

[1] At a meeting of the Section, held July 18, 1918.

DISCUSSION.

Dr. J. J. PRINGLE: I look upon the lesions as multiple examples of tuberculosis cutis. I have at present a counterpart of this case in hospital, in a woman, of about the same age, who has similar lesions, but in a very exaggerated degree. The disease is of several years' duration. In her case the diagnosis is definite; I confirmed it in a very unfortunate manner, by giving her a test-dose of tuberculin when she first came under my observation, two or three years ago. There was probably a slight error in the dosage of the tuberculin given. She had a very violent local and general reaction, her life being placed in great danger. It is a very peculiar and interesting fact that these very extensive cases of multiple cutaneous tuberculoses are frequently unaccompanied by clinical evidence of any internal development of tubercle, the general good health of the patients being maintained for a long time.

Dr. S. E. DORE: I suggest that this is a case of urticaria pigmentosa in an adult. The lesions seem to be of the nature of œdematous papules rather than solid nodules. Absence of itching does not necessarily exclude urticaria pigmentosa.

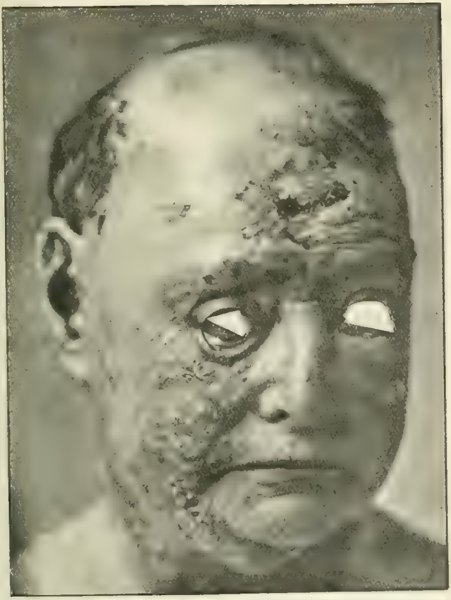
The PRESIDENT: I think, if there are alternatives to the diagnosis suggested by Dr. Pringle, Dr. Dore's is the most probable. There is the possibility of its being an unusual leukæmic manifestation, and that has to be excluded, and it is not always easy to do so, even by blood examinations, because in early and chronic leukæmia the blood examination does not, by any means, give an absolute diagnosis, as the condition of the blood varies. One or two points are rather against this being a tuberculide: the vascularity of some of those lesions: in the place where the lesions are most numerous there is slight evidence of necrosis of the areas suggested to be tuberculous. It is true there is scarring on the lower extremities, but this is common, and in this patient is larger than one would expect from the type of lesion. The third point is the pigmentation, largely unexplained by the diagnosis of its being tuberculide. The chronicity fits in with the diagnosis of a tuberculide. There is something in favour of Dr. Dore's suggestion, and this will quickly be evident on histological examination.

Dr. STOWERS (in reply): My first impression of this patient, and I have only had a limited time to examine him, was that it was of a more serious nature, more so than Dr. Pringle's remarks imply. I suspected the possibility of a developing sarcoma, although I admit it does not conform with our usual experience of such a disease. I thought it might belong to the class known as "tuberculides," but as the patient was aged 53 and the duration did not exceed six years I was doubtful. If Dr. Pringle's views are correct, then I cannot remember seeing so late a manifestation as this. I accept his view, as it fits in with the clinical aspect better than my own. Still the case requires further investigation. The urine is normal and the Wassermann test completely negative. Nodules have been removed for histological and bacteriological examination and further report.

Severe Case of Tertiary Syphilis of the Scalp and Face.

By George Pernet, M.D.

The patient, a housewife, is aged 63, and the duration of the disease seven years. It began on the top of the head and above the right ear, and has gradually spread forward ever since over greater part of face,



as shown in the photograph taken when first seen by me. There is much scarring of the anterior part of the scalp and face, and the borders of the serpiginous involvement are ulcerated and crusted, as also the ears (*see* figure). She has never had any treatment except fomentations and ointments. I diagnosed tertiary syphilis, and put her on pot. iodide, and she improved, but it had to be stopped after five or six days, as it led to a profuse bullous iodide eruption about the arms, and to a less extent on the back. Since then she has had Donovan's solution, and is decidedly improving. She has had ten children: seven are living, three died in childhood. The last child is living and aged 18. No miscarriages.

DISCUSSION.

Dr. H. G. ADAMSON: I think this is a tertiary syphilide: it is quite possible to have one as superficial as that. It is too rapid for lupus, and it is unusual for a person of her age to get such extensive lupus. An injection of N.A.B. or galyl would settle the diagnosis.

Dr. J. J. PRINGLE: I think that Dr. Pernet has not definitely established that the condition is due to syphilis. First, there has been no blood test, which in cases of dubious diagnosis is a matter of capital importance. Secondly, there is no history of syphilis. Thirdly, this woman has had ten healthy children, and there is nothing else suggestive of syphilis elsewhere about her: the lesion itself gives no conclusive evidence of syphilis. There are many reasons against it being syphilis, and in favour of the forms of senile tuberculosis of the skin—namely, that type of lupus vulgaris which was described many years ago by Leloir, of Lille, as *lupus vulgaris erythematoides*, a disease of advanced life, often attacking or beginning on the scalp. It is a rare but quite definite form. This woman presents typical destruction of the pinnæ of both ears. And I see none of the deep lesions which occur in tertiary syphilitic ulcerations, but a thin superficial "tissue-papery" scar, in which I think I can detect some nodules such as characteristically result in cases of the type of lupus I have referred to.

Dr. MACLEOD: At first I thought this was of the type referred to by Dr. Pringle—namely, lupus erythematoides of Leloir, that is, a superficial variety of lupus vulgaris: but on further examination, I regard it as a tertiary syphilide, as I have never seen lupus vulgaris commence on the scalp.

The PRESIDENT: The difference of views expressed shows that a good deal remains to be done to make the diagnosis as clear and distinct as possible. I agree rather with what Dr. MacLeod has said. At my first glance I rather felt, knowing very well some of the cases Dr. Pringle described, that this is a case of the erythematoid type of lupus vulgaris, described by Leloir, but the duration is a very important point in favour of it being a superficial syphilide.

Dr. PERNET (in reply): I have not found any other evidence of syphilis in the case. There have been no miscarriages. The blood of the patient has not been tested, but I feel I do not require a blood test. I should rely rather on clinical knowledge than on a blood test, which may be fallacious. I think it is not lupus vulgaris, since it is rare for that condition to affect the scalp at all, except by extension from the face, and for other reasons. The duration of the case was one of the leading features which made me exclude lupus vulgaris. As to the *lupus vulgaris erythematoides* of Leloir, which must be differentiated from lupus erythematosus, it is of very rare occurrence. Therefore I cannot agree with Dr. Pringle that it *frequently* begins on the scalp. As far as I recollect from Leloir's description it does not begin on the scalp, and does not ulcerate. I will have a blood test of my case carried out and report further as to the upshot of that and more energetic treatment.

Addendum.—July 19, 1918: Wassermann negative. July 27: Intravenous novarsenobillon, 0·15. August 3: Wassermann, very weak positive. August 17: Intravenous novarsenobillon, 0·15. August 24: Wassermann, weak positive.

Case for Diagnosis: possibly an early case of the *miscalled* Multiple Idiopathic Hæmorrhagic Sarcoma of Kaposi.

By J. J. Pringle, M.B.

This case is for diagnosis. The patient is an Englishman, aged 28, by trade a glass refiner, and resident in the East End of London. Before he came to the Middlesex Hospital on June 18 he had been under hospital treatment elsewhere for two years. The lesions which you see appeared as what he calls "spots" on the dorsum of his right foot four years ago. These were followed by "spots" below the internal malleolus on the same side, and, shortly afterwards, by similar lesions on the dorsum of the left foot, which have coalesced to form a deep purple hæmorrhagic patch. He had extreme pain. After a fortnight in bed he got much better, and resumed work. He had to return to the hospital in another three weeks. On June 28 I noticed a new development, which I believe to be of the utmost importance as regards the diagnosis of the case—namely, on the dorsum of the second, third and fourth toes of the left foot there were prominent soft, purple vascular enlargements, about the size of peas, which bled very freely, and showed some superficial erosion, probably due to sepsis. On some previous occasion he had had an ulcerative condition of the left sole, which had recovered. I place great importance upon these vascular lesions, as possibly giving the keynote to the diagnosis. They may still be seen, although, for our research purpose, I spoiled the case by having him X-rayed, for since then these vascular growths have become trifling in size. He has, however, a few raised vascular lesions on the dorsum of the second toe of the left foot,

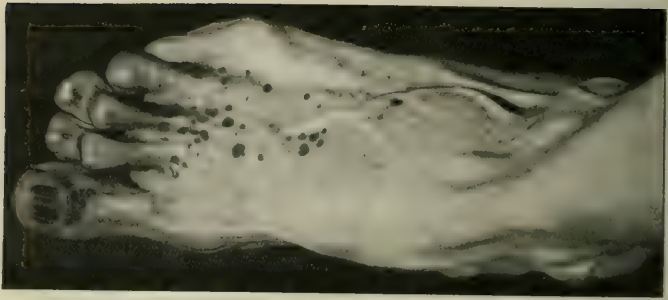


FIG. 1.

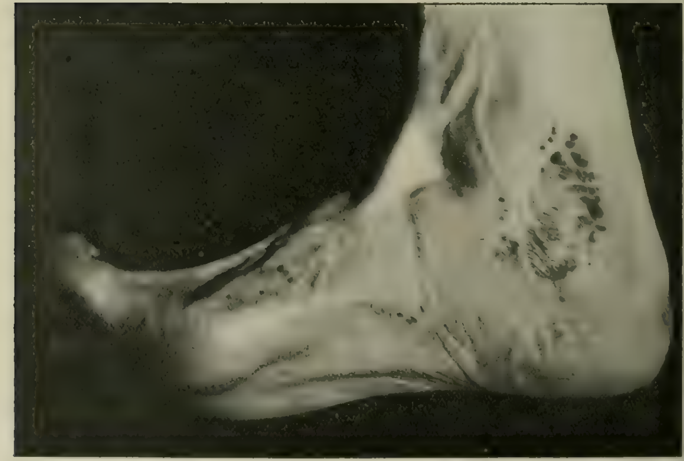


FIG. 2.

To illustrate Dr. J. J. Pringle's case for diagnosis.

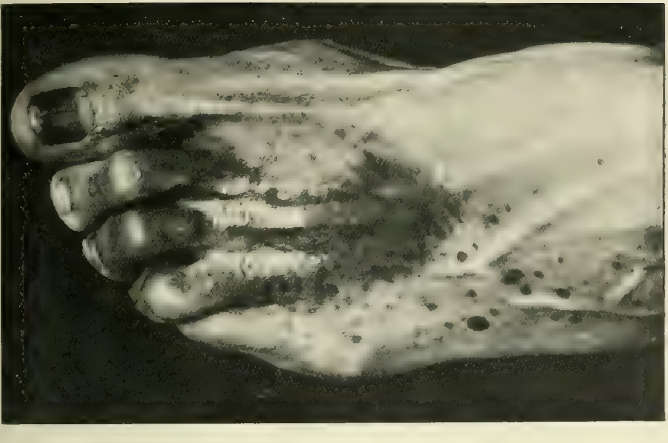


FIG. 3.

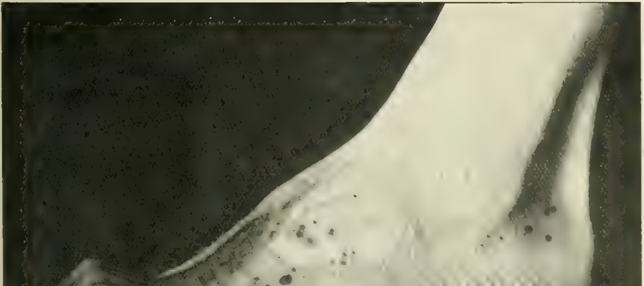


FIG. 4.

To illustrate Dr. J. J. Pringle's case for diagnosis.

which are, I think, coalescing to form lesions like those which have been destroyed by the X-ray. He is a very thin, delicate man, but without any physical evidences of disease elsewhere. I suggest the possibility of it being a very early case of so-called hæmorrhagic multiple sarcoma of Kaposi. In partial support of that vague view is the fact that the highly vascular lesions have subsided under the X-rays. This happening has been very strikingly brought out by Dr. Sequeira's experience, some early cases of the rare condition I have mentioned having been under his observation, and a few of these, I know, have shown improvement beyond expectation.

DISCUSSION.

The PRESIDENT: The latest manifestation, in the form of those little pulpy tumours, is exceedingly suggestive. I would draw attention to the manifestations of a vascular nature, almost as if they had been purpuric lesions, occurring before the appearance of the tumours. That, as an initial stage of the pigmented sarcoma, one knows little about. It is interesting to observe how the vascular lesions in this man's case appeared largely in the regions where one is so accustomed to finding purpuric manifestations in cases of vascular trouble, owing chiefly to static conditions in the lower extremities—namely, the parts which one is accustomed to look upon as areas of weak circulation—namely, behind the internal and external malleoli. Vascular lesions which often become inflammatory in consequence of damage to skin one sees frequently in such positions. The appearance of the malady on the extremity, involving the toes, is a little different. But if this turns out to be, as Dr. Pringle suggests, the Kaposi type of pigmented sarcoma, it would be an interesting point in the development of the case to remember that the early type of vascular purpuric lesion appeared in those places which one regards as *loci minoris resistentiæ*, so far as the circulation is concerned, in the lower extremities. Dr. Pringle has not given us histological evidence concerning the little growths, but we shall be glad to have that later on. I thought, at first, that this was of the nature of septic granuloma on the toe, but it does not look like that. It is not suppurating now. It looks almost as if it were a true granulomatous lesion of the Kaposi type.

Dr. S. E. DORE: The small vascular growths on the toes and feet suggest the possibility of the case being one of angiokeratoma, and I should like to ask Dr. Pringle, who saw the condition before the X-rays were applied, if he can exclude that disease.

Dr. PRINGLE (in reply): The possibility of angiokeratoma passed through my mind. The localization of a few of the lesions on the toes is identical with that of angiokeratoma, but the tumours were pulpy, soft, and elastic, with no keratoma, and not like the tumours of angiokeratoma. I shall be glad to furnish histological sections if opportunity occurs.

Case of Grouped Comedones in an Infant.

By J. M. H. MacLeod, M.D.

This is the most extensive example of grouped comedones I have seen. The neck and upper part of the back and chest are affected and present large groups of comedones, which here and there, owing to secondary infection, are situated on a raised red granulomatous base dotted over with small pustules. It is due to the rubbing on the chest of camphorated oil for the relief of symptoms following bronchopneumonia.

Dr. MacLeod (in reply to remarks): I know of no particular method of hastening a cure. It is important to refrain from the use of greasy applications in such cases, and I consider that the best treatment is soap and water and boric acid fomentations.

Erythema Multiforme Perstans.

By J. H. Stowers, M.D.

The ailment has existed for a period of fifteen months. The patient, a munition worker, aged 26, married, with one child, states that the eruption commenced over the upper part of the sternum, and, later, at short intervals involved the right upper eyelid and brow, the right and left cheeks and both forearms. The rest of the body was free. The general health of the patient is good. Some years ago she was the subject of rheumatic fever without complications. The lesions are pink, distinctly elevated, with abrupt edges in parts where the infiltration is being absorbed, and fading off in degree and colour towards normal skin, producing an urticarial appearance. The largest lesions are distinctly circular, or segments of circles, with hard raised borders, and slightly tender on pressure. They do not leave scars. The lower part of the body is wholly unaffected. There is no itching, but a throbbing sensation disturbs her at night. The patient has within the last year suffered from an acute herpes zoster on the left side which has cleared up, leaving the usual pigmentations and scars. The woman states that the present disorder developed while she was working with a powder known as "T.N.T.," and subsequently with "tetryl" composed of nitrate of ammonia and methyl aniline, and to this her ailment has been attributed. Also that two of her fellow-workers developed a similar eruption.

My first impression was that it was artefact, but the entire absence of vesication and serous exudation excludes this view. The lesions do not correspond with herpes iris; the persistence of the eruption is inconsistent with this view. I show the case as one of erythema multiforme perstans with an unusual distribution which is known to remain for months, and in rare instances even for years, and eventually to undergo spontaneous resolution.

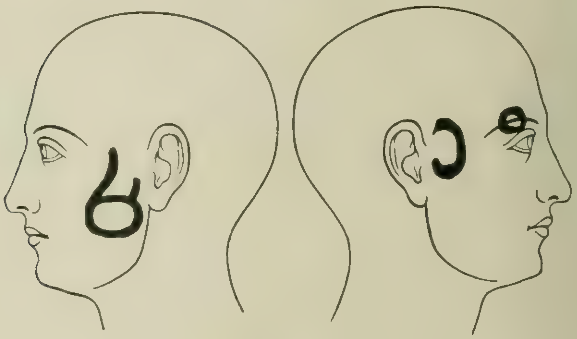


FIG. 1.

No drugs have been administered internally which would bring the case into the category of a dermatitis medicamentosa. I regard the cause to be probably a general toxæmia and possibly of intestinal origin.

Since exhibiting this patient, my attention has been kindly directed by Dr. Adamson to cases of a similar character reported in the *British Journal of Dermatology* by Dr. Graham Little, Dr. A. M. H. Gray, and Dr. H. MacCormac, in vols. xxiv, xxv, and xxvii respectively.

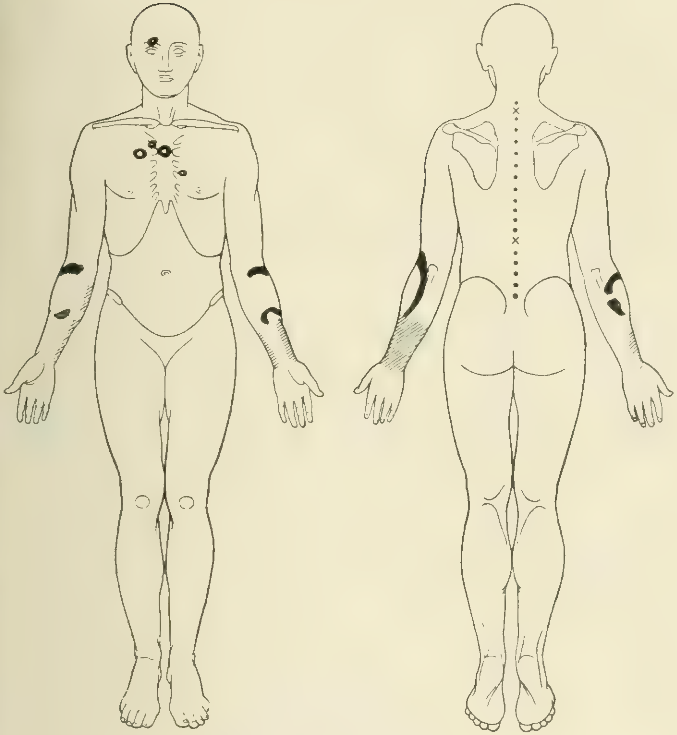


FIG. 2.

The lightly shaded regions indicate where the eruption has existed, leaving a pigmented condition of skin.

DISCUSSION.

Dr. H. G. ADAMSON: This case adds another to a series we have had before us, which in discussion we concluded were a type of "persistent erythema multiforme." In some cases they were on the wrists, in others on the face, and in some on the knee. We can exclude artefact, it is not sufficiently artificial. These cases improve under salicylates. There is another type of fixed erythema, which Dr. Colcott Fox described, "persistent circinate erythema," which is not quite the same, not necessarily symmetrical, and not in such raised hard rings. One or two lesions on the face of this girl look like lupus erythematosus: it was so, too, in one of the cases shown before. There may be a relationship between the two conditions.

Dr. GRAHAM LITTLE: Dr. Adamson has referred to the case shown by me. I observed this patient for four years, and one of the chief symptoms was the remarkable duration of individual lesions. We are not told whether the actual lesions in this case have a similar persistence, and this is of cardinal importance in diagnosis. The case has been compared with Dr. Colcott Fox's cases of circinate erythema recorded in the International Atlas of Rare Skin Diseases. These cases were probably instances of dermatitis herpetiformis, as tentatively suggested by Dr. Colcott Fox himself, and as Darier has independently suggested. I have had a case similar to these, which I reported as an instance of Darier's erythème circulaire centrifuge, but which eventually developed into a typical dermatitis herpetiformis. This may also be the diagnosis in this case, which clearly calls for further investigation and report.

The PRESIDENT: The appearance and distribution of the eruption, and the way in which it heals, suggest an artefact element: and unless Dr. Stowers had stated that he had excluded every possibility of it being artefact, that would have been my diagnosis.

Dr. STOWERS (in reply): I adhere to my conclusions. I did not exclude the possibility of artefacta until I had considered the point very closely. As to the duration of the individual lesions in this case, I cannot speak from personal observation.

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UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE ELEVENTH

SESSION 1917-18

SECTION OF ELECTRO-THERAPEUTICS

LONDON
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1918

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Section of Electro-Therapeutics.

President—G. Harrison Orton, M.D.

(*October* 19, 1917.)

PRESIDENT'S ADDRESS.

The Necessity for Education in Radiology and Electro-therapeutics.

By G. Harrison Orton, M.D.

The status, in this country, of radiology and electro-therapeutics and of those who specialize in these subjects is not satisfactory. I am not the first to touch upon this, for Captain Thurstan Holland, in his presidential address before the Röntgen Society last November, showed, with many illuminating examples, with which most of us are unfortunately too familiar, wha a detrimental effect this war is having on these branches of medical science and on those who practise them. No good will be done by reiterating example upon example, to show the dangers of allowing the present system, or rather lack of system, to exist, but I would strongly advise those who have not read Captain Holland's address to do so, and need only add that the present state of things is unfair to the patient, who is often the chief sufferer, and unfair to the radiologist and electro-therapeutist. There can be no doubt but that this state of injustice, this hopeless lack of system, is due to defective education, and therefore I think it necessary, that before we can expect any improvement to take place, some scheme of education must be devised.

That education is very necessary, even among prominent members of the medical profession, the unsatisfactory conditions existing in the X-ray departments of many military hospitals are alone sufficient to show. In the *British Medical Journal* of September 15, 1917, Captain Thurstan Holland published a letter which I will quote.

> I have before me the July number of the *American Journal of Roentgenology.* It contains a statement on the action of the U.S.A. military authorities in the organization of X-ray work for army purposes. As this is in such striking contrast to the methods adopted in this country, I venture to call your attention to it.
>
> On June 11, the Surgeon-General convened a meeting of prominent radiologists with some of his medical officers. This conference sat in session for two weeks. As a result—
>
> (1) A number of schools for the teaching of military radiographic work were established in charge of experts in different parts of the country, and were directly controlled by the Surgeon-General.
>
> (2) Medical officers intending to do X-ray work have to attend for three months' instruction, and then are accepted or declined according to the ability shown.
>
> (3) Arrangements for the X-ray examination of recruits—especially from the point of view of the condition of the chest—were made.
>
> (4) The numerous methods for the localization of foreign bodies were considered, and it was decided that three methods should be taught thoroughly, and only these three.
>
> The Government is defraying the expense of this organization and teaching, and officers taking the course of instruction draw, during this period, the pay of their respective ranks.
>
> This action of the American authorities in their recognition of the importance of expert medical men controlling the radiographic work shows great foresight.

Now in this country I think I am right in saying that practically the whole of the organization of military X-ray work has been carried out without any consultation with prominent radiologists, with possibly one exception. Why is this? Evidently the failure of the authorities to recognize either radiology as an important branch of medical science or the radiologist as a specialist, again evidence of ignorance due to lack of education in this subject.

What I have said with regard to radiology appertains with perhaps even more force to electro-therapeutics, and it seems to be the opinion of those in authority that any person capable of switching on an electric current is quite well enough qualified to carry out electrical treatment.

We are all familiar with the touching little photographs in the lay press —the small faradic coil, the possibly fascinating V.A.D. gently stroking the muscles of the wounded hero, while watching in the background is the generous donor of the apparatus, a pretty picture, but surely one not likely to elevate the status of electro-therapeutics or of the electro-therapeutist. Recognizing then that all this is due to ignorance and lack of education I propose now to outline as briefly as possible certain efforts which have been made, since last March, to put these branches of medical science on a satisfactory footing, and to raise the radiologist from the ignominious position in which he finds himself.

At the Council Meeting of this Section held on March 16 of this year a letter was brought forward by Sir James Mackenzie Davidson dealing with grievances which a radiologist had against certain of the military authorities. The Council was of opinion that the radiologist in question had not been fairly treated, but regretted that they, as the Council of the Section, were not able to take any action in the matter. This however led to an informal discussion, the upshot of which was this: A small committee was formed, with Sir James Mackenzie Davidson as chairman, to do what was in its power to counteract the bad influences at work, and try to improve the status of the branches of medical science in question.

The first meeting of the committee was held at Sir James Mackenzie Davidson's house on April 4 of this year, and the points discussed were:—

(*a*) The best manner in which to deal with questions concerning the status of medical men holding positions as radiologists and electro-therapeutists.

(*b*) The steps to be taken for the promotion of the teaching of radiology and electro-therapeutics.

Many suggestions and schemes were thoroughly discussed, and a small sub-committee was formed to deal with the matter, the upshot of this being that a letter, of which the following is the text, was sent to the Vice Chancellor of Cambridge University on May 26, 1917: –

> The call for some organization of the members of the medical profession who practise radiology and other branches of electro-therapeutics is urgent, and there is great need that some steps should be taken to establish these subjects on a proper footing both in regard to the public and the profession generally. At present no standard exists: there is no organized teaching of the subjects, and those medical men who have taken up this branch of medicine have had to pick up their knowledge almost entirely by experience—at the

expense of their patients. The result of this haphazard state of things is well depicted by Captain Thurstan Holland in his Presidential Address before the Röntgen Society. The lack of knowledge on the part of those who have been responsible for placing totally untrained men, both lay and medical, in charge of most of the military X-ray departments has led to endless tragedies, while in civil practice much suffering would be saved if competent radiologists were in charge of the departments. In civil general hospitals, where the X-ray department is properly organized, fully 10 per cent. of all the patients who enter the doors of the hospital find their way to this department, and this in spite of the fact that those in charge are so over-burdened with routine work that there is no time for research or the undertaking of new branches of diagnosis and treatment.

As the outcome of a discussion at the Council Meeting of the Electro-Therapeutic Section of the Royal Society of Medicine on March 16 of this year, a small committee was formed to consider:—

(i) The best means of raising the status of radiology and electro-therapeutics to their rightful place among the branches of medicine, and to attract the better class of medical men to take up these special subjects.

(ii) To suggest what steps should be taken to provide teaching in radiology and electro-therapeutics.

At this meeting, after a prolonged discussion, a sub-committee was elected to go into this matter.

After considering several schemes the sub-committee suggested that it was not advisable to start any programme of teaching in London until it was known if facilities could be offered for granting a Diploma at Cambridge, or some other University.

It was decided to approach Cambridge University in the matter, as it was stated by one of the members that funds would be found not only to provide for the necessary expenses, but also to equip, maintain and eventually to endow a Research Hospital and Chair if these facilities were granted by this University.

It is intended that the scheme shall be started on broad lines, in order that development may not be hampered; the objects being:—

(i) To provide a standard in these subjects.

(ii) To provide the teaching that will be necessary, together with the facilities for hospital experience.

So far as it has advanced at present the scheme of the sub-committee provides for:—

(A) A central organization in London, housed in an institute, where teaching facilities would be available in the scientific part of the curriculum, the practical experience being available in the large hospitals. Later, if necessary, centres could be started in other towns.

(B) A post-graduate diploma in one of the older universities, where a research hospital would be built. This hospital would be devoted to the scientific investigation of the many physiological, pathological and physical problems that are waiting to be worked out. This hospital would be available for those who wished to work on various problems and especially for those who were investigating with a view to the M.D. thesis.

Appended you will find outlines of :—

(A) The London Scheme.

(B) The Cambridge Scheme.

(A) THE LONDON SCHEME.

A representative board, elected annually, would be responsible for this scheme.

(1) The intention is to provide a Central Institute in London, equipped for general organization, and also for post-graduate teaching, where the necessary appliances would be available for demonstrations, and where all lectures on scientific subjects could be given.

(2) Arrangements would be made for teaching in physics, chemistry, pathology, and clinical medicine, and for lectures by specially qualified medical men in those branches of radiology in which they had specialized. Special lectures and demonstrations in allied subjects would also be arranged, and whenever distinguished colleagues from abroad visited this country, they would be invited to deliver lectures.

(3) In conjunction with the University which grants the diploma the Board would draw up the general lines of teaching, not only for the London school, but for any other schools that might be formed.

(4) The Board would arrange for students to hold appointments as clinical assistants in the various X-ray and electrical departments that would be available in the London hospitals.

(5) The Board would arrange centres throughout the country in close touch with the Central Institute, where it would be possible to arrange for post-graduate teaching.

(6) The Institute would be the centre for the spread of knowledge of these subjects and of the work done in this country; and if possible the existing X-ray Journal would be utilized as its official organ.

In the future it is probable that many overseas workers will be attracted to this country, and a strong effort should be made to establish an attractive post-graduate school, where the subjects could be taught in a manner worthy of their importance. It is especially important to get the scheme started as soon as possible, even in a small way, for when the war is over there will be many medical men who have done the comparatively mechanical war X-ray work, who will be attracted to the subject and who will welcome the idea.

Lay assistance is necessary in X-ray and electro-therapeutic work, and provision will have to be made later for the training of qualified assistants, to whom a certificate would be granted. The next step would be to approach the General Medical Council on the question of the recognition of radiologists, and the making of it illegal for any other than a qualified member to practise these subjects.

This would at once lead to the raising of the standard and the multiplication of posts of responsibility, which would attract the better type of medical man to the new subjects.

(B) THE SCHEME FOR CAMBRIDGE.

Clinical material for adequate teaching facilities is not available in Cambridge, but, on the other hand, there is quite sufficient for research, and the intention would be that, while London, and possibly other large towns, are the teaching centres, Cambridge should be the examining and research centre.

For this purpose it is necessary that there should be some man in charge in Cambridge who would, in conjunction with the duly appointed Board, make the necessary arrangements for the examinations and undertake the direction of the research hospital.

In the first instance the duty of the man in charge would be to find out what men were at work and what work was being done in the various hospitals throughout the country, and to enlist the active and sympathetic support of all workers, not only in the Central Institute in London, but also to obtain their co-operation in the research work. As far as possible he should also be in touch with the work that is being done abroad, especially in Canada and the States.

There is room for endless research in radiology, not only on the physical side, but also on the human—e.g., in the investigation of the alimentary tract and especially in the action of drugs, &c., &c., and it is proposed that a research hospital should be started with twelve or more beds and certain laboratories attached; this should be placed as close to the Addenbroke Hospital and also to the existing laboratories as can be arranged.

In the first place it would be advisable that this hospital should be started in temporary buildings, as it is impossible to foresee on what lines the hospital would develop.

Another meeting was held on June 15, 1917, when the following encouraging replies to the letter addressed to the Vice-Chancellor of Cambridge University were read.

May 28, 1917. The Vice-Chancellor wrote: "I brought your letter before the Council of the Senate this morning. The proposals contained in the letter were received favourably, and the whole question was referred to a special board for medicine, with the request for an answer to the Council before the end of the term." (Rev. T. C. Fitzpatrick.)

June 7, 1917. From Dr. Laurence Humphry, Secretary of the Special Board for Medicine: "The Special Board for Medicine considered your letter *re* medical radiology and welcomed the proposal. It was unanimously agreed to approve the general principle, and the Board were prepared to recommend that steps be taken to institute a diploma. It was thought advisable that the scientific teaching should, as far as possible, be carried on in Cambridge, and that the appointment should be rather a director or reader and not a professor. The Board appointed a small committee to consider the details, and, if desired, to meet members of your committee to discuss the matter."

The committee further considered that in view of the importance of the questions involved, and of the necessity for unanimity of opinion, it would be a proper step to invite the leading radiologists and electro-therapeutists to attend a meeting, and the secretary was authorized to call a general meeting, when the questions could be discussed in all their bearings.

It was also arranged that the secretary should write to the Deans of the various London schools inviting their co-operation in the scheme.

This has been done, and subject to the consideration of details, most of the Deans have replied approving of the scheme, and there can be no doubt that in the near future it should be possible to arrange for post-graduate teaching at several of these schools.

The general meeting above referred to was held at the Royal Society of Medicine on July 26, when after much lengthy argument and discussion the following was proposed and carried unanimously: "That this meeting of medical men practising as radiologists desires to express its approval of the initial steps taken by the gentlemen who have submitted an outline of a scheme, and to thank them heartily for the work they have done, and further, to propose that the gentlemen who have already taken action in the matter should form an executive of a larger committee, representative of all the London and provincial schools."

If such a body had been in existence at the beginning of the war, it would have been in a position to advise the military authorities as to the general organization of radiology and electro-therapeutics, and further to undertake the teaching of men appointed to the various departments, on standard methods. Unfortunately this was not the case, but is it yet too late?

From what I have seen and heard even in the last few weeks, I should say decidedly "No"; others will probably say "No," also, though I fear nothing so simple as saying "No" will help us out of the anomalous position in which we find ourselves. But as Captain Thurstan Holland has truly said: "The time has come to enter a strong protest against the continuance of methods which allow of such anomalies to exist." And I think we are now in a position to enter such a protest. From what has been done there appears no doubt that the Cambridge part of the scheme could soon be in active being, if the necessary financial support can be arranged.

It remains for us to see that the London scheme is put upon a sound basis so that the two may be able to co-operate in practical teaching and research work. To do this, we must endeavour by every means in our power to establish a central institute in London with facilities for post-graduate teaching on a fairly large scale, in order that men who desire to study these subjects may be able to do so under conditions comparable with those already established in other countries.

The task is not an easy one, and I foresee that there are many obstacles still to be overcome, not the least being the lack of funds. The way, however, has been paved, and if we push on with sufficient energy and determination there is no reason why, in the near future, our efforts should not be crowned with success.

Section of Electro-Therapeutics.

President—Dr. G. HARRISON ORTON.

(*April* 20, 1917.)

Heliotherapy and X-rays in the Treatment of Surgical Tuberculosis.

By H. J. GAUVAIN.

(ABSTRACT.)

THE modern conception of surgical tuberculosis which permits these methods of treatment being logically employed was outlined. Stress was laid on the importance of the "soil" of the patient with reference to the infection, and particular emphasis laid upon the desirability of regarding heliotherapy and X-ray therapy merely as adjuvant methods in the conservative treatment of surgical tuberculosis, and not as substitutes for other methods of known and proved value. The theories of the action of sunlight on patients the subject of tuberculous disease were discussed, Rollier's views being given special prominence. Evidence was adduced to show that the action of sunlight might be conveniently regarded from two standpoints: (1) Its local or direct action, the benefit of which was confined to superficial lesions by its local bactericidal effect, combined with the favourable inflammatory response which carefully graduated exposure elicited; and (2) its remote or indirect action which induces favourable constitutional effects and benefits deeply-situated lesions. Ultra-violet radiation has little penetrative power. It has the property of inducing in certain patients a deposit of pigment in the skin, which pigment may have the power of transforming the energy of ultra-violet radiation into some form of chemical energy. There was a considerable mass of evidence that such

transference of physical energy occurred in plant life, where the transforming medium was represented by chlorophyl pigment. On the other hand, pigmentation had an important protective rôle. Pigmented patients suffered much less from exposure to heat or cold than those in whom pigment formation did not take place. It was well known that the long-waved rays had considerable penetrative power, but in the absence of pigmentation exposure to these rays was not well tolerated. It was conceivable that under suitable conditions they might be of very great therapeutic value. The physiological properties of these rays demanded careful investigation. Their employment, after pigment formation had been induced by ultra-violet radiation, was advocated. Ultra-violet rays of shorter wave length than found in the solar spectrum had not been proved of value, might be harmful, and should be screened off during exposure emanating from artificial sources.

The remote beneficial effects of sunlight appeared to be dependent on the ability of the skin of the exposed patient to pigment satisfactorily. While ultra-violet radiation induced pigment formation, another factor or factors were concerned, which were inherent in the patient and varied widely in degree. Equal exposure in different patients induced very varying degrees of pigmentation, and in some there appeared to be absence of pigmenting power. In these latter the direct effects of light therapy were produced as easily as in pigmenting patients, but the remote effects were unobtainable. Sandy- and red-haired subjects attacked by tuberculous disease often had little pigmenting power. Freckling would often occur, but deposition of pigment was only obtainable with great difficulty or not at all. Freckling was undesirable, as its formation was not associated with any improvement. It was not necessarily confined to the "red-haired" type of cases, but was commoner in these. It was noted that failure to induce pigmentation was often associated with latent tuberculosis, even though there were no clinical signs of active tuberculous disease. Subsequent development and successful treatment of a hitherto unsuspected lesion was frequently associated with acquirement of pigmenting power, which then was coincident with rapid improvement, both general and local. This observation was therefore deemed to be of great prognostic significance, and corresponded to a somewhat remarkable extent with observations the author had made on the prognostic significance of the von Pirquet test. Pigmenting power appeared to be to some extent a measure of resisting power. Pigmentation was not necessarily synchronous with improvement, but was very frequently associated with amelioration of the disease, and to such an extent that its significance could not be overlooked, and was not coincident nor accidental. Insolation was regarded as an adjuvant method of treatment which accelerated and consolidated the cure. Viewed as a whole, patients did much better in the summer when exposed to sun treatment than in the winter. In the after-care of patients occasional courses of insolation might advantageously be prescribed, as it appeared to reduce danger of relapse and increase the patient's power of resistance.

The constitutional effects of insolation had been described with some degree of fullness by Rollier. It had a marked tonic and psychological effect. It speeded up metabolic activity. Oxygenation was increased, but the respiration rate diminished. There was increased elimination, which was of value in the discharge of toxic products. There was a profound effect on the blood-stream; increased formation of red cells, increase in hæmoglobin content and leucocytosis had all been noted. On even the deepest lesions marked improvement had been recorded. Sinuses frequently dried up where other methods of healing failed. Sequestra were often painlessly extruded. Fibrous tissue generally when not needed was absorbed, and this property was of special value in restoring mobility to healed joint lesions long fixed by fibrous ankylosis.

Heliotherapy in surgical tuberculosis was contra-indicated: (*a*) For its remote effects where there was absence of pigmenting power; (*b*) in amyloid disease; (*c*) in certain acutely septic cases until improvement by other means had been first effected.

The precautions to be observed in the application of heliotherapy were then outlined and a printed table of rules to be observed by patients taking the sun cure was exhibited. The possible dangers of over-exposure were detailed and the measures to be applied for their relief described.

X-RAYS IN THE TREATMENT OF SURGICAL TUBERCULOSIS.

The indications for the employment of X-rays in the adjuvant treatment of surgical tuberculosis were discussed, and the importance of careful technique and the selection of suitable cases emphasized. The desirability of working on definite lines and with limited dosage as suggested by Iselin was noted, and Iselin's table of tolerance had been observed in the treatment of the cases referred to. The observations made were summarized as follows:—

(1) X-ray treatment was of great help in certain local conditions, and, synchronous with their amelioration, general improvement was often observed and maintained.

(2) Its value and applicability varied in different regions of the body.

(3) It was useful in certain cases of sinus formation and in the treatment of many tuberculous ulcers, leaving supple, non-keloidal scars.

(4) It was especially valuable in certain cases of tuberculous adenitis, but the greatest benefits might usually only be anticipated if associated with other methods of treatment. It was particularly useful where there was much periglandular infiltration and here formed a useful treatment preliminary to extirpation or aspiration. Aspiration, with or without the employment of modifying fluids was to be preferred to extirpation, when possible.

(5) It was most safely applied in tuberculosis occurring in the neck, elbow-joint, and knee-joint. In tuberculosis of the hands and feet great care was desirable in its employment, but with limited dosage, and adequately screened exposures, its use in such cases was often associated with remarkable improvement.

(6) It was apparently helpful in certain cases of caseous abscess formation.

(7) It was both unnecessary and undesirable in the treatment of abscesses which could be easily aspirated, and should not be used with abscesses which leaked from an aspiration puncture which had resulted from faulty technique.

(8) Its employment was attended with danger in abdominal tuberculosis, and was apparently of little value in costal tuberculosis.

(9) Obviously its employment was contra-indicated in tuberculous disease of the genital organs.

(10) Possible untoward effects as the result of careless technique, or too frequent exposure, should always be borne in mind and guarded against.

(11) While frequently of great assistance in the treatment of specific lesions, other aids to treatment were often required and should be associated.

The paper was illustrated with numerous lantern slides and autochromes, and details given of cases.

Section of Electro-Therapeutics.

President—Dr. G. HARRISON ORTON.

(*October* 19, 1917.)

A Method of running the Coolidge Tube on Continuous Current Mains without the aid of Accumulators or other Transforming Device.

By FRANCIS HERNAMAN-JOHNSON, M.D.

[THIS paper has been published *in extenso*, with illustration, in the *Archives of Radiology and Electro-therapy*, November, 1917, pp. 174-176.]

(*November* 16, 1917.)

Nævus treated by X-rays.

By E. P. CUMBERBATCH, M.B.

(By permission of Mr. HARMER.)

THE patient is a girl, aged 19. At the age of 5 a nævus formed on her nose. Slight bleeding was occasionally noticed from the nose, but it was not profuse till she was aged 14, when severe hæmorrhage took place. She was then seen by Mr. Harmer, who found that the nævus occupied the anterior (cartilaginous) part of the nose and vestibule.

spreading to the cheek and upper lip, on the right side. The nævus pulsated strongly.

The facial artery was tied, but severe hæmorrhage again recurred, at a later date. The external carotid was then tied, but with no more success. I commenced X-ray treatment in November, 1910. Hæmorrhage persisted at intervals, sometimes slight, sometimes profuse, until March, 1912, when the bleeding became less frequent and less severe. Sometimes there would be no bleeding for two months. The X-ray treatment was left off in the summer of 1912. Towards the end of the same year the bleeding began to be more frequent and severe and a second course of X-ray treatment was commenced. It had the same beneficial effect. It was discontinued in the summer of 1913. Bleeding was slight and occasional until 1914, when a very severe attack took place and the patient appeared to be quite blanched. It was found that it took place from an artery in the septum. The vessel was obliterated by means of diathermy. Since then there has been no hæmorrhage.

During the first course of treatment, pastille doses, unfiltered, were given every three weeks. During the second course, half-pastille doses were given measured *under* a half-millimetre filter of aluminium. They were given every three weeks first, and later every four or five weeks.

DISCUSSION.

Dr. HERNAMAN-JOHNSON: I have treated several cases of a similar nature by X-rays. The results are varied, but sometimes remarkably good. The X-rays are of service in extensive involvements where surgery is unadvisable and needling has proved ineffective. My technique is rather different from that described. I employ small doses two to three times a week. This hastens results, so that, in cases which are going to do well improvement is manifest within three weeks.

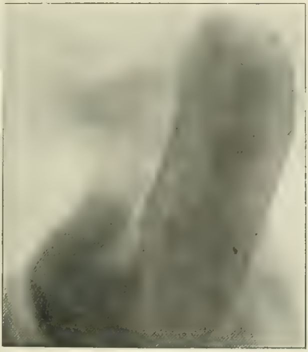
Dr. J. E. A. LYNHAM: I have not dealt with a cavernous nævus which bled from a mucous surface. I have had several which bled from a skin surface, and after repeated raying with radium, I succeeded in causing them to shrivel up. I believe that in reducing a cavernous nævus by any form of ray treatment, we do not depend alone on the production of an endarteritis obliterans. I think we also act on the connective tissue, which seems to be very susceptible and might easily respond to a comparatively weak treatment. If this be so, I think it quite possible that the exposures given by Dr. Cumberbatch may have been quite sufficient to contribute towards arresting the hæmorrhage.

(*November* 16, 1917.)

Hair-ball in the Stomach.

By Reginald Morton, M.D.

This patient, Miss B., aged 21, was brought to me by Dr. Robert Hutchison with a four years' history of gastric disorder. She was rather thin, but did not appear to be in bad health. Discomfort, pain and vomiting were the main symptoms. A large tumour could be felt in the abdomen, to the left of the middle line. All the various suggestions put forward as to the nature of this mass could be excluded.



Hair-ball in the stomach.

The patient could not take more than 7 oz. of the opaque meal, and on the screen this showed itself all in the fundus of the stomach, filling it completely. There was no air in the fundus. As we continued to observe, the form of a greatly enlarged stomach gradually came into view, the pyloric end showing up more clearly than the body. The lower pole extended almost to the symphysis and rose very little when the patient was placed recumbent. I had not seen anything just like it in my own practice, but I suddenly remembered two cases described by Mr. Thurstan Holland in the *Archives of the Roentgen Ray*, which enabled me to make a diagnosis of hair-ball there and then. Operation had to be delayed for the usual female reason, but in due course I received a letter from the family physician in which he stated that the hair-ball weighed 3 lb., length 12 in., greatest circumference 9¾ in. She had been in the habit as a schoolgirl of pulling out single strands of the hair of her head, chewing and swallowing them. Probably larger hair-balls than this have been discovered and removed, but they must be very few. The specimen is preserved in the museum of the Royal College of Surgeons, with a copy of the radiograph.

(*November* 16, 1917.)

Abnormal Condition of the Œsophagus, Stomach and Colon.

By Reginald Morton, M.D.

R. B., female, aged 50, complains of vomiting off and on for the past thirty years, sometimes two hours after meals, or it may be in the middle of a meal; often at one or two in the morning. She is relieved by the vomiting. There was no hæmatemesis until the middle of December, 1916, when she had it on four occasions, twice in the morning and twice in the evening. She improved until August, 1917, when hæmatemesis came on again twice in a fortnight. After this she suffered from rheumatism in the hands, feet and knees. This was accompanied by some diarrhœa and followed by several hæmorrhages. She is still frequently sick but may go a few days without vomiting.

This patient came to the West London Hospital as an out-patient under Dr. Grainger Stewart in February last. As the case presented unusual features he sent her to the X-ray department with a request that I should examine the stomach. There did not appear to be any difficulty in swallowing, and being rather busy at the time I ordered the opaque meal to be given while other cases were being seen to. She took the whole of the meal without any special difficulty and when I looked at her with the screen this (fig. 1) is what I saw. I made further investigation and other plates, but as she was an out-patient it was difficult and incomplete. On November 2 she was admitted as an in-patient under Dr. Arthur Saunders, and this gave me the opportunity I desired. The next plate (fig. 2) shows the thorax before the opaque meal was given. Note the large rectangular shadow taking up most of the right chest; it is more or less uniformly streaked or mottled, and some of the "tree-branch" markings can be seen through it. There were no pulmonary symptoms or past history of pulmonary disease.

The food on being swallowed left the line of the œsophagus at about the level of the sterno-clavicular joints, travelled almost horizontally, and then behaved very like what happens in the case of a normal stomach, except that the formation of the triangle with the apex downwards, and the canalization of the cavity took place more quickly—so much so that I could not secure a plate at this stage. The next plate (fig. 3) was taken immediately the last mouthful of the meal was swallowed. So little passed down the œsophagus that it was not noticed. Plates (figs. 4, 5, and 6) were taken at intervals of a few minutes and show the gradual development of the form of the sac of the diverticulum as the food sinks from the upper to the lower part. Fig. 7 shows a lateral view of the thorax in which it is seen that the sac is nearer to the back than the front.

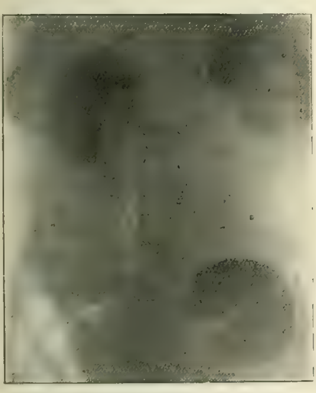


FIG. 1.

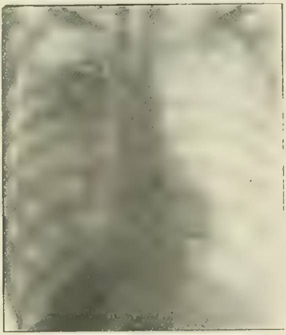


FIG. 2.

I now directed the patient to lie with the head low and on her left side for two hours. This was to see if it would facilitate the transference of the food to the stomach, as it was quite evident that her food did ultimately find its way there. The plate (fig. 8) shows how mistaken I was, but it also shows how large the sac is when fully dilated and that it passes right across to the left of the vertebral column. It looks very like the atonic and dilated stomach in some cases of long-standing pyloric obstruction.

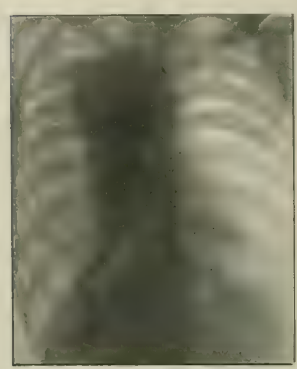


Fig. 3.

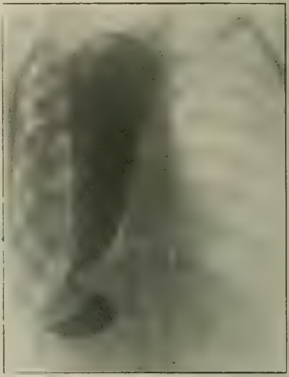


Fig. 4.

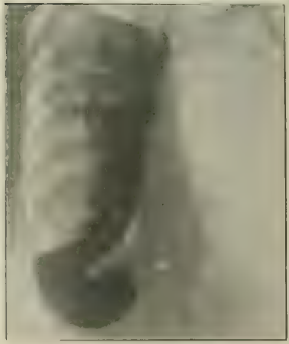


Fig. 5.

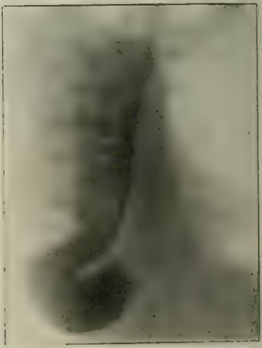


Fig. 6.

On two occasions I was able to observe this case after about one day's interval. The plate (fig. 9) was taken just twenty-four hours after an opaque meal, when the sac was quite empty. There is a fair amount of food in the stomach which is dropped and dilated; the rest is in the ileum and colon. I would draw attention to the circular shadow just to the right of the middle line and at the level of the upper part of the sacrum; it is divided into a lower dark and an upper light hemisphere. I could not satisfy myself on palpation whether this was gastric or colonic. There appears to be a narrow channel between this and the stomach, or is it a part of the colon behind? It will be noticed

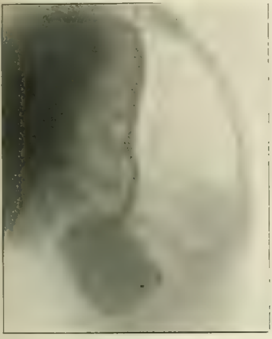


Fig. 7.

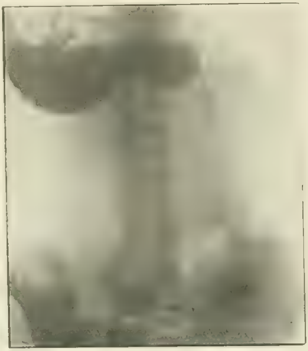


Fig. 8.

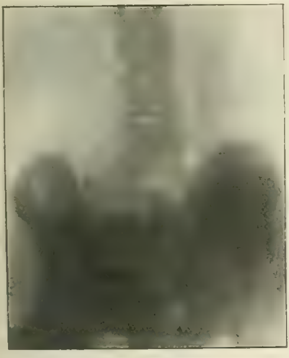


Fig. 9.

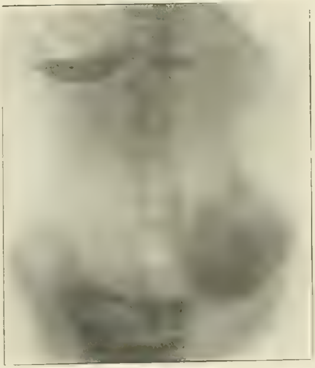


Fig. 10.

that approximately the same level is shown on each side of the middle one; this inclines one to think they are all parts of the colon. In any case there is interesting material for further investigation here. I took another opportunity some days later, twenty-seven hours after taking the meal, but it will be seen that this did not help very much except to demonstrate the fact that opaque meals do not always behave in exactly the same way. This last plate (fig. 10) shows a quite appreciable amount still in the sac of the diverticulum; the stomach is fairly well shown, also the ileum, cæcum and colon as far as the middle of the transverse portion, where we again see the horizontal level, with the lower part segmented, proving it to be undoubtedly colon.

This is as far as I have gone with this case; she is still in the hospital, and probably further examination will be made.

I shall greatly appreciate any helpful criticism regarding the lines along which subsequent examinations should be made.

Dr. A. C. JORDAN: From the form of the shadow, I suggest, in several of the slides that the case may be one of "cardiospasm," with enormous dilatation of the œsophagus. I realize, however, that I am offering a very small part of the complete explanation of this unusual case.

(*November* 16, 1917.)

A Convenient Method of Heating the Spiral of the Coolidge Tube.

By ALFRED C. JORDAN, M.D.

IN my own experience the only trouble with the Coolidge tube has been the care of the accumulators used to supply the current for heating the cathode spiral. In hospital work in particular the battery is frequently out of repair.

The most obvious method—to use the main supply, suitably cut down—has been negatived till recently by the need to insulate the cathode from earth. This restriction is not absolute, as demonstrated by Dr. J. A. Shorten (*Archives of Radiology and Electro-therapy*, August, 1917, p. 65), who has shown that it is possible—and in the humid climate of India advantageous—to "earth" the cathode. There are objections to this course, the chief being (as Shorten points out) the undue strain on the coil. No doubt coils could be constructed with insulation so perfect that they would stand the extra strain, but the fact remains that the average coil now in use would deteriorate rapidly, and might actually break down (as happened in the case of one of Shorten's coils).[1]

Clearly, then, it is essential to devise a method (of heating the Coolidge spiral) which permits of the complete insulation of the cathode. This has already been accomplished in two ways at least :—

(1) By means of an alternating current step-down transformer.

(2) By a continuous-alternating generator, and a Tesla transformer in oil.

Where an alternating supply is available No. 1 method is the simpler. With a continuous current supply No. 2 is no doubt satisfactory, but is expensive.

A simpler arrangement suggested itself to me—viz., to use an interrupter in conjunction with a small step-down transformer. The secondary winding supplies the current to heat the Coolidge spiral. An adjustable rheostat in the primary circuit provides the fine regulation. This rheostat is attached to the control table, and thus the temperature of the spiral is regulated with great ease and exactitude while the tube is "running." A motor-magnetic interrupter is used, and the only constructional problem is to get sufficient insulation between the primary and the secondary circuit. This has proved a simple matter. Messrs. Watson are making and supplying the little apparatus (at a cost not much greater than that of the insulating stand with accumulators and adjustable rheostats supplied heretofore). The accompanying illustration shows the details so clearly that no additional description is needed.

A feature of my transformer that must be borne in mind is the fact that the current needed to heat the spiral—as indicated by the ampèremeter—is a good deal less than with a battery. At Fishmongers' Hall, for instance, the spiral of the tube needs 4·8 amp. with a battery, but only 3·5 amp. with my transformer. If this difference be remembered there will be no accidents due to overheating the spiral.

In practice my method of controlling the temperature of the cathode spiral of the Coolidge tube has proved perfect, giving a uniformly illuminated screen, free from flickering and from changes of intensity. The heating device can be run for hours at a time. The interrupter needs no attention for long periods; it receives no more than 3 or 4 amp. on a 240 volt circuit.

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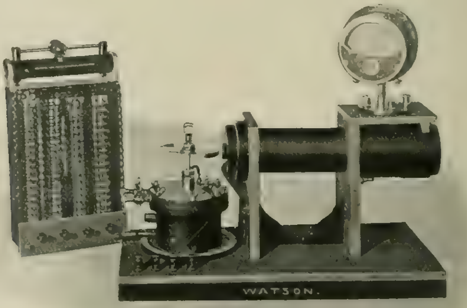
[1] At the preceding meeting of this Section (October 19), Captain Hernaman-Johnson described the means whereby he had actually carried out Shorten's suggestion to use the direct current from the main for heating the spiral of the Coolidge tube.

Once the advantages of a transformer are generally known, I am convinced that a battery will cease to be used with the Coolidge tube.



DISCUSSION.

Dr. G. B. Batten: Dr. Jordan's device is very practical, but it would be safer to get insulation from the high voltage current from the X-ray tube, to wind both the primary and the secondary of the small transformer on insulating tubes of different sizes and put the whole in an oil bath; also for treating the spiral, either by Dr. Jordan's device from the direct current mains or from the alternating current, it would be better and more economical to have the primary wound in a closed magnetic circuit.

Sir James Mackenzie Davidson: I agree that accumulators are not very satisfactory for use with the Coolidge tube, and I suggest the use of a small direct current dynamo giving the 3 to 5 amp. necessary for heating the coil of the Coolidge tube—which can be worked by a small motor with an insulating band or an insulated shaft. The speed can be regulated with a resistance, and probably this simple plan will work very well. I have not yet had the opportunity of trying it.

Section of Electro-Therapeutics.

President—Dr. G. HARRISON ORTON.

(*December* 21, 1917.)

The Interpretation of certain Electro-Physiological Phenomena.

By ARTHUR E. BAINES.

(ABSTRACT.)

THE object of this paper is, first, to reply to the criticism of my work by Professor Bayliss;[1] and, second, to call attention to some recent experiments in their relation to electro-physiological research.

Professor Bayliss says :—

(1) "The currents led off from various parts of the body by metallic electrodes were due to the inequality of the electrodes and to differences in the activity of the skin glands.

(2) "Neither static charges nor induction played a part in the phenomena.

(3) "'Nerve-leaks' were merely places at which the skin was moist."

We will take these statements seriatim.

I will first ask you to note that I claim that as expressed in electrical terms the average electro-motive force of a healthy person is rather under than over 5 millivolts, and the current about the tenth of a micro-ampere. I have here a pair of electrodes used to determine, in my way, the electro-motive force and sign of a patient prior to going over the body with other electrodes. They are made of German silver, to obtain a low co-efficient of increase of resistance with rise of temperature, and are heavily plated with chemically pure silver. They are of exactly the same surface area and weight, and remaining in the bath the same time, and therefore receiving an equal deposit, are presumably as near perfection as metallic electrodes can be. Moreover, they are not kept in contact with the skin for a sufficient time for electrolysis to take place in the presence of a current of 0·1 microampere. Assume, however, that it does take place, or may do so, and I will give you three tests which will conclusively disprove it:—

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[1] *Proc. Roy. Soc. Med.*, 1917, x (Sect. Electr.), p. 95.

(1) Let one electrode be connected by means of a flexible insulated wire to the right and the other to the left terminal of a sufficiently sensitive galvanometer, and let the patient, after being earthed, take one electrode in each hand without exercising any pressure. The resultant deflection is, we will say, 300 mm. *positive*. It follows, therefore, according to Professor Bayliss, that the right-hand electrode is electrically positive to the left-hand one, and that so long as the connexions upon the galvanometer are not altered it must so remain. If, however, the hands are crossed and the electrodes changed the resultant sign will always be *negative*; there will in fact be a reversal of sign.

(2) In each hand the four fingers and the thumb are of opposite polarity; some carrying a greater quantity of current. Place one electrode between the first and second, and the other between the third and fourth fingers of the same hand, leaving the thumb free. If the hand is not wet, and for that reason the leakage from the thumb is negligible, there will not be any deflection. If, however, first one and then the other electrode is touched by the thumb there will be an immediate and constant deflection of different sign. Here in both cases there would appear to be a circuit between skin and metal through the galvanometer to skin and metal, but a true circuit is not established until by contact of the thumb with an electrode a potential difference is brought about.

(3) For the third proof I cannot select anything better than that given by Dr. Longridge's two cases.[1] One was of paralysis of the median nerve below the seat of an injury in the left forearm. Upon galvanometric examination he obtained no deflection from the thumb or first or second fingers, but after resecting ¾ in. of the nerve, which

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[1] *Brit. Med. Journ.*, 1916, i, p. 717; cf. *Lancet*, 1917, i, p. 62.

was densely fibrosed, and joining the ends, he obtained deflections of 5, 55 and 120 mm. respectively. The second case was a division of an ulnar nerve, and the deflections in this instance were the exact complement of those I have given.

The second statement made by Professor Bayliss was to the effect that neither static charges nor induction play any part in the phenomena.

As a matter of fact the human body is so sensitive to induction that if an electrical current left it as quickly as it entered it—if it were not in fact for inductive embarrassment—a human being, when in the circuit of a sensitive galvanometer could be used as a receiving instrument in wireless telegraphy.

As regards the average electrostatic capacity of man under normal conditions of exterior insulation, it may safely be put at between 3 and 4 mfd. Whether the body is also sensitive to induction is easily determined. If a person is placed in contact with a dead earth of low resistance for at least five minutes before testing, the sign of current will always be peculiar to that person, and may be verified any number of times during a course of years. The E.M.F. may vary in accordance with the degree of fatigue or state of health, but the sign never. Let that person, however, go within a quarter of a mile of the District Railway or a Tube, or within 100 yards of an electrified tram line and, if he is tested again, without previous earthing, neither the sign nor the E.M.F. will be normal.

The third point: "'Nerve-leaks' are merely places at which the skin is moist."

This is not so. In neurasthenia the hands are usually moist. The characteristic deflection of neurasthenia, as shown by the hand to hand method, is feeble excursion combined with fluctuation of sign: 30 or 40 mm., varying from one side of zero to the other, from negative to positive, and vice versa. In anæmia there is always a small but *constant* deflection. In shell shock, so far as I have observed it, the hands are wet and the deflection small, but in all cases of acute local pyrexia with a dry, hot skin, there is a rapid off-scale deflection, and the rapidity is in accordance with the degree of local temperature.

[The author then dealt with the use of paraffin in the treatment of local pyrexia, and the difference between the oil bearing his name and that ordinarily sold as medicinal paraffin. He stated that a rock oil of the paraffin series was necessary if his theory concerning its mode of action was correct. If it were exposed to sunlight or ultra-violet rays there was a distinct falling off in the therapeutic value of the rock oil, although no difference in resistance could be detected by a galvanometric test. He had devised an apparatus for the further treatment of the oil, and since that time there had been no failures which could not be attributed to faulty diagnosis.]

My next point concerns the penetration of the hydro-carbon oil I have selected as being most suitable for the purpose. Take two cases: one, say, a slight bronchial affection with no appreciable rise of temperature, and the other, of acute lobar pneumonia with a temperature of 105° F. or so. Apply the dielectric to both in the form of a pad of plain absorbent cotton wool soaked in the oil, covering with oilskin, and watch the result. In the first case the pad may remain wet for a week; in the second case it will be nearly dry in a few hours. I have long held that certain phenomena—e.g., the currents exhibited by animal and vegetable skins, mucous membranes, &c.—were due to air-charge and that the different deflections could be accounted for by the varying relative inductive capacity of some portion of the substance of such skins, &c., in the presence of moisture. In order to resolve this question an apparatus was made by passing two insulated flexible wires, terminating in two steel needles, through the bed-plate of an air pump and arranging the connexions so that the electrodes could be earthed in air or *in vacuo* by moving a switch, and galvanometric readings taken, both in air and *in vacuo*, without disturbing the object under examination. As the assistance of a physiologist of standing was not available, the experiments were confined to vegetable physiology as being a more familiar subject. Numerous tests were made, first with a view to ascertaining the influence upon the readings of polarization. This was found to be a negligible factor. It will be sufficient to relate two results which have been checked and rechecked:—

The first experiment was with an onion, which has an E.M.F. of 0·086 volt, and upon inserting one needle in the foliage end and the other in the root a full initial deflection was obtained, in air, of 2,750 mm. At the end of three weeks of short-circuiting it was still 2,250 mm.; the fall of 500 mm. being possibly due to polarization. The needles were inserted in a similar onion in exactly the same manner and a deflection of 2,750 mm. again obtained. It was then earthed *in vacuo* for twenty-five minutes, at the end of which time *it gave no deflection at all.* Earthed in air for the same length of time the deflection fell only 295 mm.

The second experiment was with an apple longitudinally divided. One half was tested in air and gave a deflection of 3,080 mm. It was then earthed *in vacuo* for forty-five minutes and upon the galvanometer being switched into circuit the deflection had fallen to 275 mm.; a loss of 2,805. The same half was then allowed to rest in the open air until it had regained its initial E.M.F. It was then again earthed for forty-five minutes in air and tested in air. It did not lose a millimetre, but registered at the conclusion of the experiment 3,080 mm. as before.

Similar results were obtained with leaves; laurels yielding up their electrification after ten minutes earth *in vacuo*, hydrangeas in from fifteen to twenty minutes, and so on, and if these results can be verified by experiments with animal tissues, it is more than probable that some accepted theories will have to be very seriously reconsidered.

DISCUSSION.

Professor W. M. **BAYLISS**, F.R.S. (communication by letter): I would emphasize the fact that the electrical conductors in the body are electrolytes in solution in water, not "metallic wires." The water itself has so high a resistance as to be practically a non-conductor for the very low electromotive forces available. Any satisfactory theory must be based on this fact of ionic, not electronic, conduction. Further, I may recall the recent work of Dr. Waller, showing that "nerve-leaks" of varied kind can be demonstrated in normal persons in response to emotional disturbances. There is also the work of Forbes and Wells in the United States, in addition to the older work of Tarchanov, to which I referred on the previous occasion, all showing the same fact.

Dr. **HERNAMAN JOHNSON**: I have used a d'Arsonval pattern galvanometer; the scale and electrodes were of the type recommended by Mr. Baines.

(1) "Hand to hand" deflexions were first tested. Reversals occurred in more than half the cases tested. It was noted, however, that uneven pressure was capable of altering the sign, especially when either thumb jammed tightly into the thumb-piece. The instrument was obtained in the hope that it might aid in diagnosing genuine neurasthenia, shell shock, &c. In this respect, the results were disappointing. Hand to hand deflexions in neurasthenics were certainly variable and unsteady. In testing for "leaks" with one electrode on the forehead and the other touching various spinal points, the most diverse results were obtained, not only in different patients suffering from neurasthenia, but from the same patient at short intervals.

(2) *Effect of Moisture.*—The effect of moisture is certainly great, but it is possible to over-estimate its importance. Obvious perspiration always greatly

enhances the deflexions, but when the sweat is removed with a dry, warm towel they came down at once. I made numerous tests to see if the deflexions could be still further reduced. I can readily believe that a hot, dry, inflamed skin gives large deflexions, although I did not make any actual tests with inflamed parts.

(3) *Deflexions in Cases of Cut or Damaged Nerves.*—I confirmed the experiments of Dr. Nepean Longridge, to the effect that no deflexions are ordinarily obtainable over the skin area supplied by a cut ulnar or median nerve. It would be possible to make a galvanometric diagnosis in the case of a completely severed ulnar or median. No information is obtained, however, which cannot be got from sensory tests. Still, in cases of suspected malingering in children and mentally defective persons, the method might be useful, being entirely objective. In the case of a sutured and partially recovered nerve the deflexions are present, but less than those of the sound side.

(4) *Effect of Contact Currents.*—That inducted effects may be readily produced in the unshielded Kelvin astatic galvanometer is well known. The d'Arsonval is, however, shielded, as previously described. I tried many experiments to see if I could detect any difference in the readings while coils and large motors in the next room were turned on and off, but I was never able to see any effect on the galvanometric readings.

(5) *Experiments on Fruit and Leaves.*—These gave constant results. Stems and roots were always negative, and pulp and other parts positive.

Conclusions.—The effects observed on the galvanometer cannot be explained on the sole grounds of differences in the electrodes and differences in the degree of moisture applied to them. Certain readings appear to be characteristic of certain pathological conditions, and may prove of value in diagnosis, and, possibly give the key to successful treatment in some cases. Nothing in my experiments can be regarded as supporting any particular theory as to how the galvanometric effects are produced, nor was any diagnosis reached which could not have been arrived at by other methods. But they sufficed to show that, in common parlance, there is "something in it."

Section of Electro-Therapeutics.

President—Dr. G. HARRISON ORTON.

The Treatment of Cases with Injury to Peripheral Nerves.[1]

By NOEL H. M. BURKE.

(ABSTRACT.)

ELECTRO-THERAPISTS are agreed as to the necessity of electrical treatment in cases of peripheral nerve injury. They are not agreed as to the precise methods to be used. Among those who practise in other branches of medicine there is a wider difference of view. Some advise electricity, some do not believe in it, some give no treatment at all beyond rest. This is explained by the prevalent ignorance of the proved actions and possibilities of the various forms of current, by adherence to the groove of fashion and custom, and by the failure of electro-therapists adequately to publish their views. It is proved by a review of the literature. Electro-therapists should exchange views more fully, and should give more publicity to their views.

In order to decide on the usefulness of any forms of treatment it is necessary in the first place to consider the exact nature of the problem. The nerve may suffer concussion, compression, partial or complete division, or the toxic form of injury exemplified by poliomyelitis. As a result the muscles suffer lack of nutrition, and are easily stretched. The antagonistic muscles weaken from disuse, and may contract if not dealt with. Lack of nutrition appears in other soft parts, and leads to chilblains and ready formation of ulcers. Adhesions may form in joints and tendon sheaths.

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[1] At a meeting of the Section, held February 15, 1918.

The actual change in the muscle consists in a degeneration leading to replacement of muscle by fibrous or fatty tissue—seldom to a complete extent. The changes in the nerve are different in each class of injury. In concussion there may be only molecular disturbance, or there may be interstitial hæmorrhages and effusions, with changes in the nerve fibrils but no rupture of the nerve sheaths, and no Wallerian degeneration. In compression the nerve picture is similar, but often presents a condition resembling chemical nerve block.

Partial division combines these two conditions with those of complete division; the latter leads to Wallerian degeneration and regeneration, and this process of recovery will be completed if fibrous tissue does not impede the way. The general lack of nutrition can be dealt with by massage, but it alone is not sufficient. Warmth is useful, and can be applied by lamps, by whirlpool baths, or by diathermy. The passage of the galvanic current has been proved to exert, by means of its chemical action, a very beneficial effect on the circulation and nutrition. The rhythmic sinusoidal current has a similar and perhaps more efficient action.

The question of producing contractions is open to some argument. There are good reasons in favour of it, and perhaps some equally good ones against it—one being that very definite and immediate improvement can be obtained by electrical means in severe cases which have lost all muscular excitability. Contraction is an important, but not the only, means by which electricity can help in the cure.

Postural treatment is indispensable, but it can do no harm to the muscles to allow them to reach their normal length occasionally, when careful movements are performed for the sake of the joints and tendon sheaths. Muscles are elastic and not rigid structures, and when paralysed should as a rule be replaced by elastic and not rigid supports.

Treatment of the nerve, whether in concussion or compression, should be actively performed, and consists in passage of properly directed galvanic current. This is advised by many writers, and is eminently reasonable when the proven action of the current is remembered. X-rays are useful in resolution of scar tissue in and about nerves. Pain can frequently be relieved by electrical means or by heat.

In complete division and suture it is sound practice to treat the wound in the nerve by the galvanic current, which is known to be useful in the case of wounds in other places.

There is authority and scientific reason for the belief that this treatment stimulates regeneration of the nerve.

The control of the case is important, and should be done in co-operation and not in water-tight compartments. The physio-therapy must be entirely controlled by the physio-therapists, but the scheme of treatment must be arranged in consultation with the surgeon and the doctor in charge of the patient. The secret of success is co-operation.

DISCUSSION.

Dr. W. J. TURRELL: To maintain the nutrition and contractile power of a muscle with injured nerve supply, it should be made to perform its function of contraction; in this way alone can tone, elasticity and contractility be maintained. The method of provoking the contraction is important, and it is right to avoid action of healthy muscles at the expense of their paralysed opponents. This can be done by adapting to treatment Lapique's method of testing with condensers in parallel, a system which has been in use at the Radcliffe Infirmary at Oxford for seven months, with very pleasing results. Large muscles can be treated in groups, but for the intrinsic muscles of the hand individual treatment is necessary. For this purpose I have a special device for obtaining a galvanic current which can be made to surge at any desired rate. With regard to duration of treatment, it has been stated that at first not more than from six to ten contractions should be excited at a sitting, owing to the production of fatigue. Neither on practical, nor on physiological nor pathological grounds can I agree with this. Fatigue after these exercising galvanic treatments is quite negligible, and to cause six or ten contractions is absolutely useless.

Dr. W. ROWLEY BRISTOWE: Neither the time taken for regeneration of the nerve fibres nor the completeness of the recovery can be affected to any appreciable extent, and I question the statement that any good is done by the simple passing of the electric current. Nor can I see how a case of pain due to compression of a nerve in scar tissue can be in any way benefited by diathermy or the galvanic current. There is no value in the current except as a stimulus to produce contraction. Besides postural treatment the chief necessity is to promote contractions, at first by interrupted galvanism, later by combined galvanic and faradic currents, as recovery of response begins, and finally by the graduated contraction method with the faradic current. In the early stages little rather than much should be done, as there is real danger of fatigue. Heat should be applied as a preparation, for either massage or electrical treatment: the only advantage of moist heat is that it lowers the electrical resistance of the skin. The great use of massage is to remove œdema, and to promote nutrition. Re-education is a most important part of after-treatment.

Mr. R. C. ELMSLIE: I can remember details of a case which shows that there is something to be said for the elastic method of support, in preference to a rigid one allowing no movement at all. I am not one of those who would lay down a hard and fast rule in favour of the latter. With regard to electrical treatment, surgeons find some difficulty in appreciating its claims. They understand the value of provoking contractions, but not of some of the other forms of current, as described in the paper. They have seen many cases of contracture and deformity developed during treatment by electrical means, and this leaves them in doubt as to the complete utility of the method. I agree that co-ordination is to be desired between the several departments concerned in the treatment of these cases.

Dr. JAMES MENNELL: The surgeon in charge of a case should at least define his aim and objective in prescribing massage treatment, or the work of the massage department of a hospital is rendered difficult. Co-operation between the surgeon and the physio-therapist is to be advocated very strongly. Muscle fibre possesses two inherent characteristics: contractility and elasticity. When the former is lost there is every reason to maintain the latter by mobilization, and Professor Langley's recent paper is therefore to be welcomed. The further advantage of combining postural treatment with mobilization is that the joint sense is maintained, and therefore recovery of co-ordination is easy, and that ultimate recovery is hastened by reducing stiffness of joints to a minimum. The massage applied to these cases acts by maintaining vascularity and by removing waste products, and must be performed by the lightest possible touch. In flaccid paralysis vigorous massage can produce great evil by injuring the plain muscle of the arterioles, and so leading to dilatation and deficient circulation. In the later stages exercises, skilfully controlled, and with particular attention to co-ordination, are of great use, but re-education involves much mental strain for the masseuse, and if inadequately performed it were better left alone.

Section of Electro-Therapeutics.

President—Dr. G. HARRISON ORTON.

Adjourned Discussion on "The Treatment of Cases with Injury to Peripheral Nerves."[1]

Dr. STANLEY BARNES: I speak not as an electro-therapist, but as a neurologist, who has seen a great number of these cases in the military area of Birmingham, and can speak of what seem to be, in bulk, the best means of treatment by relatively unskilled assistants, and of the results that accrue. Diagnosis of nerve injuries is bad, especially with regard to the median nerve, in many hospitals. The most glaring case is that of a pensioner who was sent to me last week for musculo-spiral injury. Though he had been seen by eighteen doctors during fifteen months, it had not been discovered that the musculo-spiral was the only sound nerve in his arm! I want to emphasize the fact that when a nerve is severed, the muscle always degenerates to a certain degree. This condition can be maintained indefinitely, without further degeneration occurring, but its maintenance depends on prolonged treatment. By treatment I mean something that will keep the nutrition of the muscle up to this level. The most important thing is to maintain the local circulation, but to do this for a short time daily is not enough. The limb must be kept warm all the time. I think a bucket of hot water is as good as *eau courante*. I doubt if the effect of the simple galvanic current is in proportion to that of hot water and massage. There is a quite erroneous idea that voluntary power can return in a few days after suture surely this is not meant by the statement, "definite and immediate improvement can be obtained without provoking contractions"? With the statement that a paralysed muscle must not be allowed to resume its normal posture for a single instant, I disagree greatly; it is doing much to prevent recovery of these cases. The muscles should be kept relaxed—within limits and the limits are, that all the joints of the limb must be allowed to remain mobile. None of these cases should be put up in a fixed position, all should be taken down and moved passively in the early stages, and actively in the later. Later the activity should include all the muscles of the body—herein lies the advantage of workshops and gymnasia.

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[1] At a meeting of the Section, held March 15, 1918.

Dr. G. COOPER: The treatment of peripheral nerve injuries falls under two heads—the restoration of nervous control, and the maintenance meanwhile of muscular condition. Diathermy, by increasing the flow of blood round the area of growth, may indirectly assist regeneration of a nerve, but beyond this it is difficult to see how electro-therapy can assist in the restoration of nervous control of paralysed tissues. The maintenance of the "condition" of the muscle is largely a question of evoking artificial contraction. This can usually be done by the method of surging galvanism described by Cumberbatch, or by the condenser method of Lapique. Good results have been obtained at Netley by both methods. Fatigue is an important question, and the determination of its onset is uncertain. Much of the massage so consistently carried out in hospitals is probably useless: the mechanical action seems to be problematical, and the benefit may be due to pressure acting as a stimulus to produce actual contraction of the muscle. The whirlpool bath provides an excellent preparation for treatment.

Mr. L. B. RAWLING: The cases of nerve injury sent from Mesopotamia to India were different from those from France. The conditions found at operation were worse than expected clinically, but I am beginning to get a clearer idea of what to expect now that I am relying on the advice of Dr. Cumberbatch. Surgeons do not usually understand a report on electrical reactions: what they really want is the electro-therapist's opinion on the advisability of operation. My views as to the results of nerve suture are gloomy: end-to-end suture is the only useful method to be aimed at, and it is often impossible.

Mr. R. H. JOCELYN SWAN: I regard the operation as only an event in the treatment of these cases, and the pre-operative and post-operative treatments as of equal or even greater importance. It is a great mistake to operate before treatment has put the limb into a condition which allows of success in the future. Any treatment which maintains the nutrition and contractile power of the muscles is to be employed, and warmth and massage are perhaps the most useful of these. I cordially agree that the teaching of prolonged immobilization is to be opposed: proper relaxation of paralysed muscles must be maintained, but the splint should be removed and passive movements carried out every day. Post-operative treatment should begin as soon as the wound is healed, and should be continued until cure is complete. It is, therefore, pernicious to discharge a soldier to the uncertainty of treatment which depends on his willingness to attend for the necessarily long time. I used to have the same gloomy views on prognosis as Mr. Rawlings, but my outlook has improved, particularly since the after-treatment has been carried out.

Probably 75 per cent. of musculo-spiral cases are definitely improved by operation, while the least hopeful are the ulnar nerve cases. I do not agree with the early operation, but prefer to wait till at least three months after the wound. When primary suture is impossible, it does not matter how late nerve suture is performed, provided that treatment preserve the condition of the limb. I do not agree with the universal condemnation of nerve-grafting. Though not the operation of choice, it is often the only possible remedy for the condition found, and I have obtained several good results from it. I had hoped to obtain some guidance in the estimation of the degree of nerve injury in the first few weeks after wounding, and also some suggestions for the treatment of causalgia.

Dr. G. B. BATTEN: There is so little knowledge of this subject in most hospitals, and so much has been shown in the course of this discussion, that I suggest that special means should be taken to publish it in a form which permits of wide distribution.

Dr. HERNAMAN-JOHNSON: Last time a discussion was raised with regard to direct treatment at the site of injury, and Dr. Cooper said that no electrical treatment would have effect in stimulating regeneration of nerve fibres. But we have to consider whether anything can be done to make the wound more healthy. The surgeon operates for the removal of fibrous tissue which has grown up round the nerve, and a common cause of failure is that this fibrous tissue has reformed round the nerve. If anything can be done to check that, it is an advantage in the ultimate result. Although nothing can be done, electrically, to hasten the growth of nerve fibre, we may prevent the destruction of the growth. Galvanism and X-rays get rid of superfluous fibrous tissue; one sees that very obviously in the case of keloid. We know the effect of these measures on the surface, and there is good reason for supposing that it goes somewhat deeper.

Mr. NOEL BURKE (in reply): Mr. Elmslie said that orthopædic surgeons only appreciate those methods of which they understand the rationale. I hope my paper gave sound rational explanations for some of the methods he suspects. He complains of cases having developed contractures during treatment. That is an argument in favour of my plea for co-operation in treatment. Dr. Bristowe suggests that surgeons should learn some electro-therapy and then prescribe the treatment they desire. It is no more right for the surgeon to prescribe to the electro-therapist than for the latter to say in what way the surgeon should perform his operations. There has been general agreement as to the dangers of prolonged fixation, and on the necessity of rational use of postural treatment. In reply to Dr. Barnes, I do not claim immediate cure or return of voluntary power by non-contracting methods. What I have often seen, and am sure of, is improvement in the general condition and nutrition of the part, and often a return of electrical response which was previously unobtainable. I agree with those who mentioned the time required for recovery after suture. I never expect anything measurable under three months in the upper limb, and four and a half or five in the lower: the improvement in condition mentioned above is not readily measurable. In reply to Mr. Jocelyn Swan, I think the early diagnosis can only be made by the method of "treat, and wait and see," for six or eight weeks at least. The treatment of causalgia is unsatisfactory: the ordinary nerve pain from fibrous adhesions can be relieved, temporarily at any rate, in many cases. I cannot withdraw this statement, and it has been confirmed by Dr. Turrell and Dr. Hernaman-Johnson. The latter also endorses my claim for the correctness of applying galvanic current to the site of nerve suture. The disputed points should be settled by comparative tests done on a large scale at the orthopædic centres.

Section of Electro-Therapeutics.

President—Dr. G. HARRISON ORTON.

Surgical Diathermy.[1]

By E. P. CUMBERBATCH, M.B.

(ABSTRACT.)

DIATHERMY has been increasingly used in surgery during the past few years, but little has been published on the subject. The object of the present paper is to invite a discussion on the methods of performing diathermic cautery, the complications, and the immediate and remote results.

Diathermy is used to destroy new growths, innocent and malignant. The destruction is the result of heat developed within the growth by means of an electric current passed through it. The current is one of high frequency oscillation, and it must be able to attain a strength of 1½ or 2 amp. This current can effectively raise the temperature of the tissue to a degree sufficient to destroy it without at the same time stimulating the excitable tissues or producing chemical (electrolytic) changes.

Malignant and innocent growths can be destroyed by applying hot instruments to them such as the galvano-thermo-cautery or the Paquelin cautery. These instruments however char the tissues at the points of contact and the destruction spreads for a very trifling distance around because the tissues are very bad conductors of heat. The tissues conduct the electric current well, and as the heat is generated along the path of the current the tissue will be coagulated wherever the current density is sufficiently great. In simple thermo-cautery red-hot instruments are used, the heat is difficult to regulate, the tissues are charred, and the spread of the heat is very slight. In diathermic cautery a cold instrument is used, the heat can be regulated by varying the strength of the current, the tissues are heated to a degree just sufficient to coagulate them, and the spread of the heat is considerable. Diathermy can produce coagulation of the tissue *en masse*.

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[1] At a meeting of the Section held April 19, 1918.

APPLICATION OF THE DIATHERMY CURRENT.

Electrodes.—In most cases two electrodes are used—an *indifferent* electrode and an *active* electrode. The former is a metal plate, about 7 in. square. It may be placed on the chest with a folded towel soaked in strong salt solution intervening. The active electrode is much smaller: it may be a disk, $\frac{1}{8}$ in. to $\frac{1}{4}$ in. in diameter. It is placed in contact with the growth. When the current is passed between these electrodes its density (and consequently its heating effect) will be least at the indifferent electrode, greatest at the active electrode. Consequently, coagulation will begin at the latter electrode and spread downwards towards the indifferent electrode as far as the region in which the current density is just sufficient to produce heat enough to cause coagulation. When the current is started and its strength gradually increased, the following events take place. First, bubbles of occluded gas escape from the tissue in the region of the active electrode. Next, the tissue whitens from coagulation. Then steam escapes, and soon after, crackling sparks are emitted from the edges of the active electrode. At this stage the tissue immediately under the electrode has become dry and does not conduct the current, so that the latter has to leap over the dried tissue on to the moist conducting parts beyond. The current should be switched off before sparks appear because coagulation will spread no further when the tissue is dry, and because any muscle in the vicinity will contract violently. The depth to which the coagulation has extended at this stage depends upon the area of contact of the active electrode with the tissue. The larger the area of contact the deeper the penetration. In some experiments carried out by the writer on excised tissue,[1] it was seen that coagulation spread below the electrode to a distance equal approximately to the diameter of the latter. In the case of living tissue the distance would be less because the circulating fluids would dissipate the heat from the regions where the current density would, in the case of non-vascular tissue, be just sufficient to cause coagulation. If the tissue is less moist coagulation would extend less far because the resistance of dry tissue is greater than that of moist tissue; the former would be more quickly heated to dryness and coagulation would cease to spread further. This is seen in the case of bone. Almost immediately after the current is switched on sparks appear because the periosteum is at once dried and then ceases to conduct the current.

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[1] See *Archives of Radiology and Electrotherapy*, December, 1914.

Practical Difficulties in performing Surgical Diathermy of Malignant Growths.—The chief difficulty is the uncertainty of the depth to which coagulation has extended. It is necessary, whenever possible, to proceed beyond the limits of the growth into healthy tissue. One method of doing this is to coagulate the tissue, scrape away the coagulated portion, apply the diathermy again, till, finally, healthy tissue is reached. This is not a satisfactory method because much bleeding will occur when the lowermost strata of the coagulated tissue are reached. A method which has been adopted by the writer is to use as an active electrode a scalpel insulated except at the terminal half-inch of the blade. The latter part is plunged into the healthy tissue beyond the growth and the part around it is coagulated. It is slowly advanced onwards, the cutting edge moving only into coagulated tissue. This method requires practice. One of its advantages is that the growth can be removed at the operation, thus avoiding the slow process of sloughing off afterwards. In successful cases the operation can be almost bloodless.

The Use of Two Active Electrodes.—When a large fungating mass of growth is to be destroyed it is advisable to use two active disk electrodes and place them on opposite sides of the growth, and coagulate it in its entirety. The scalpel electrode may then be used to excise it at its base.

Electrodes for Special Parts.—For diathermy of growths causing stricture of tubes, such as the œsophagus, a special electrode is used at St. Bartholomew's Hospital. It is composed of an ebonite rod with a metal cheek. The rod fits the stricture and the cheek rests against the growth and serves to conduct the current into the latter. For diathermy of the larynx electrodes with special handles are used.[1] For diathermy of intravesical growths a long wire, insulated, except at its extreme tip, is used. It is passed along the side channel of a cystoscope intended for the catheter from the ureter.

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[1] See *Archives of Radiology and Electrotherapy*, January, 1915.

Precautions to be taken during Diathermy.—Diathermy is no respecter of tissues, and any part which lies on the path of a sufficiently strong current will be destroyed. It is necessary to proceed with special caution when in the region of *blood-vessels* and *bone.*

(1) *Blood-vessels.*—Blood is constantly circulating, and if the flow is rapid, as is the case in vessels much larger than capillaries, the blood will flow across the path of the current before there is time for it to coagulate. The vessel wall, being a fixed tissue, will, however, be coagulated. Consequently, when the slough is separating later, there is always the possibility of hæmorrhage. Thrombosis may have taken place during the formation of the slough, but if the latter is artificially aided to separate, the thrombus may be detached and hæmorrhage will occur. If from an artery, the hæmorrhage may be severe. A likely source of hæmorrhage is the lingual artery, after diathermy of the tongue for carcinoma. Severe bleeding has occurred in four of the cases treated at St. Bartholomew's Hospital. Two were cases of carcinoma of the tongue. One was a case of rodent ulcer of the temporal region. Ligature of the artery before the diathermy would prevent this complication.

(2) *Bone.*—If bone lies close to the electrode there is always a risk of destroying the periosteum. If this happens the dead bone must separate before the cavity can be filled with granulation tissue, and there may be constant pain and possibly continuous septic discharge resulting from septic infection before the separation of the sequestrum. A patient was treated by the writer for a very intractable rodent ulcer of the scalp. The edges and base were destroyed by diathermy. The periosteum did not escape, and at the present time (April, 1918), presents an area of dead bone 2 in. by 1½ in. with granulation tissue, apparently healthy, at the margin. The bone became septic and there was much pain. The patient refused surgical operation for removal of the sequestrum. More than three years have elapsed since the operation, and there are no signs of recurrence of the growth. To avoid the destruction of bone the writer has, in such cases, used a much smaller active electrode and applied the diathermy to the edges only of the ulcer, carrying the coagulation to a relatively short depth. A patient with a rodent ulcer of the temple measuring 1 in. by ¾ in. was treated in this way. An electrode, ending in a metal ball ⅛ in. in diameter, was used, and the edges only were coagulated. The operation was performed on February 21, 1918. The ulcer completely healed except

at one spot, and the periosteum was not damaged. The spot mentioned was again treated by diathermy under local anæsthesia.[1]

RESULTS OF DIATHERMIC CAUTERY OF MALIGNANT GROWTHS.

(1) *Immediate.*—Patients do not suffer from surgical shock after diathermic cautery. On the day after the operation there is usually a rise of temperature, but it rarely lasts for more than a day, and the patient has no discomfort. On the third day the patients are usually able to get up. The ability to get out of bed and walk about the ward or in the open air without discomfort or any bad effect after the removal of massive growths within the throat is remarkable. The patients are able to swallow fluids on the day after the operation and eat solid food very soon after. With regard to the part treated by diathermy, œdema is a noticeable feature. It is noticed during the operation. A copious discharge of lymph occurs and may last for twelve or even twenty-four hours.

(2) *Later Results.*—After separation of the slough (which takes from one to three weeks, depending on its size) the residual cavity fills rapidly with granulation tissue. Mucous membrane grows over very rapidly, skin more slowly. An interesting feature of the new tissue is that it does not contract afterwards, nor does it form adhesions with adjacent structures.

Complications.—Hæmorrhage is an event to be feared when the coagulation has extended to the region of an artery. Severe hæmorrhage occurred in four cases treated at St. Bartholomew's Hospital, but was not fatal in any case. Considering the very large number of cases treated at this hospital, the complication cannot be regarded as frequent. Septic broncho-pneumonia has occurred in a very few cases only, and is less frequent than after cutting operations on growths of the mouth and pharynx. Sepsis followed by fatal septicæmia has occurred in one case only. The patient suffered from carcinoma of the cheek. There were enlarged glands in the neck. Pus was evacuated from one of the latter on the ninth day, and the patient died on the eleventh day. Streptococci were discovered in the blood. Œdema of the glottis is likely to occur when diathermy is performed for growths in the region of the larynx. Tracheotomy has to be performed, sometimes during the operation, sometimes later.

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[1] The patient was last seen on June 17, 1918. The ulcer had completely healed.

Duration of Life after Diathermy of Malignant Growths.—The case, previously mentioned, of rodent ulcer of the scalp, was treated by diathermic cautery in March, 1914. The patient is still alive (April, 1918), and is able to get about and do work. A case of the tonsil, tongue, and mucous membrane of upper and lower jaw, was treated by Mr. Harmer with diathermy in November, 1911. The diathermy was repeated, for recurrent growth, in July, 1912, and again in June, July, October, and December, 1913, and again in March, 1914. The patient died in July, 1914. He had been free from discomfort up to the last three months of his life. Another patient had a carcinoma of the tongue, 1 in. by ⅔ in. This was treated by diathermic cautery. Death occurred fifteen months later from a rapidly growing fungating mass in the neck, but when she was last seen by the writer, a few days before her death, the tongue was free from palpable or visible growth. Other patients have survived for shorter periods, but in nearly all there was freedom, for a considerable time, from symptoms, such as, in the case of growths of the mouth and throat, expectoration, salivation, pain and discharge. All the cases mentioned were surgically inoperable. Diathermy should certainly be tried for surgically operable growths, considering the quickness of the operation, the absence of shock, the short stay in bed afterwards, and the relative freedom from complications.

Diathermic Cautery of Innocent Growths.—Diathermy is an excellent method of treatment for certain non-malignant growths. For papillomata of the bladder it seems to be the treatment *par excellence.* A case of nasopharyngeal fibroma had been treated surgically by Mr. Rose. It recurred and was then treated by diathermic cautery. The same treatment was repeated two months later, and again four months later. The growth was effectually destroyed, without hæmorrhage. For nævi which cannot be treated by other methods, diathermy has proved to be very successful. The first patient treated by diathermy in England was a girl with a large pulsating nævus on the side of the tongue and floor of the mouth. Diathermy was performed by Nagelschmidt, at St. Bartholomew's Hospital, on October 21, 1910. Four years later the patient was seen by the writer, and there was no trace of nævus on the tongue or floor of mouth. There was a patch on the pharyngeal wall. This was treated by diathermy under a local anæsthetic. The writer has treated many cases of nævus by diathermy and has found the method most suitable for nævi underlying mucous membranes.

DISCUSSION.

Dr. WILLIAM HILL: My experience of this method is rather limited, but I have formed a very high opinion of the value of diathermy in some malignant growths. My experiences of surgical diathermy have been in cases of tonsillar disease, and three or four in the pharynx. I have never used it in the œsophagus. If we go much beyond the confines of the growth we shall get perforation or destruction of the œsophagus and mediastinitis. But I have used diathermy in the region usually called the œsophagus, and which may be clinically so regarded, namely, the deep, or post-cricoid pharynx. There, although no cure has been effected, the results immediately and for some considerable time afterwards have been excellent. But there have been complications. There is marked dysphagia afterwards, which can be overcome by the use of intubation. In one instance, the first in which I had Dr. Cumberbatch's assistance, there was a good deal of œdema of the larynx, because the growth was not only behind in the pharynx, but it invaded the ventricular band of the larynx, and I did not like to leave the case without doing tracheotomy. In another case I did intubation to ensure not being called up in the middle of the night on account of respiratory trouble. Another complication which may be formidable is that which occurred in my last case, when Dr. Cumberbatch was present, and I was assisted by Mr. Norman Patterson. In that case we had a big sparking. It was a case of epithelioma of the tonsil and pillars, extending to the base of the tongue. The palate was very much coagulated superficially, and a big slough formed which extended 2 in. from the area of the operation. If it was due to the sparking, it emphasizes the fact that a small amount of sparking is not right, and much sparking causes a considerable burn. The white patch in this case looked like diphtheria: some tissue sloughed away far from the malignant growth. But the man is none the worse for it, and now, two months afterwards, there is no recrudescence, and he is very satisfied with what has been done. Another point concerns diathermy and surgical removal at the same time. In the case just referred to, the growth was removed by the scissors and then diathermy applied: the mass was not left to coagulate and slough away. When I have been at work Mr. Harmer has sometimes given me timely reminders that enough was being done at a particular spot. My wish has been to see the whole growth disappear before I have finished with the case. In one case, consequently, when I did not take Mr. Harmer's advice, the posterior surface of the cricoid was exposed, and I pictured to myself the destruction of the perichondrium, and that there might be a sequestrum and all kinds of complications. But it granulated over, therefore it is possible the destruction of perichondrium and periosteum, if it is not too wide, can be recovered from. I once had a lot of bleeding which it took a long time to stop. It was in the arytænoid region. As we had not done tracheotomy, the patient had to breathe in the ordinary way: so we could not plug, and it was not an easy thing to pick up vessels in the bleeding area where charring had been going on. A certain amount of charring may possibly be a good thing. In a case in which I helped Dr. Cathcart I removed the charred portions, and there was no particular bleeding there.

Mr. NORMAN PATTERSON: I have brought two patients. One is a man from whom I removed the anterior two-thirds of the tongue, using a diathermy knife. He was transferred to me by a colleague as inoperable. I operated nearly two years ago and now there is no sign of disease. The other is a woman, aged 58, who had carcinoma of the right antrum and palate. A year ago I excised the upper jaw and found that the growth invaded the ethmoidal region: this I could not remove at the time. Later I destroyed the ethmoidal extension by diathermy. No reappearance of the disease has occurred. With regard to the case in which I was associated with Dr. Hill, the extensive sloughing was, I think, partly due to preliminary ligature of the external carotid. Dr. Cumberbatch has mentioned that in devascularized tissues penetration is deeper than in tissues where the circulation is free: this seems a strong point in favour of preliminary ligature. In cases of growths in the neighbourhood of the lower jaw I am glad to see a bony sequestrum come away afterwards, because this is a sign that destruction has taken place beyond the margin of the disease. In the tongue case shown I operated with a knife cautery such as Dr. Cumberbatch describes. I cut a deep gutter through the tongue behind the growth and excised it *en masse*. I show a specimen of a portion of the hard and soft palate removed in the same way. Using a diathermy knife makes the operation fairly rapid and almost bloodless, especially when the main vessels of supply are controlled by ligature. This kind of operation I have been doing for over two years and it might be called diathermy dissection. I have not been so fortunate with regard to hæmorrhage as some. Comparatively recently I had a case in hospital which died of hæmorrhage, and since then, in cases of epithelioma extensively involving the base of the tongue, tonsil, and faucial pillars, I have carried out preliminary ligature of the external carotid. It is especially indicated when the patient is in the nursing home and no expert aid is at hand for controlling urgent hæmorrhage: the case is somewhat different in a hospital patient, where a house surgeon is at hand. I have seen good results in the treatment of lupus by diathermy: it is also a good aseptic method of exploring for brain abscess. It greatly lessens the dread one feels in exploring through the meninges. If you pass a knife through normal meninges, you are liable to set up meningitis, whereas the diathermy terminal coagulates the tissues on its passage through them, and the danger of meningitis is much lessened. Septic remains of tonsils may be destroyed by this method. I have had one case in which three attempts were made to dissect out the tonsils, but septic tonsil remains still existed. I applied diathermy with a completely satisfactory result. I have also used diathermy to destroy the remains of a dermoid cyst of the nose, and in the case of a thyroglossal cyst. Both these cases were operated on by dissection with unsatisfactory results.

Dr. McIlraith: Diathermy is particularly beneficial in malignant diseases of the tonsils. One man, whom I am showing, came to King's College Hospital with a large ulcerating, foul and hæmorrhagic mass in the situation of the left tonsil which he had had for two years. It began with soreness in the left side of the throat, a continual choking sensation, and difficulty in eating and drinking. He had lost flesh considerably, was very cachectic, and seemed very ill. There was a moderate amount of glandular enlargement in the neck. We determined to treat it, not by cutting it away completely, but by doing it gradually, and allowing the coagulated tissue to separate in due course. First I removed the main mass of the growth, then the lingual portion, and afterwards the outlying portions, with the aid of the suspension laryngoscopy apparatus. A month later, as there was some recrudescence along the anterior pillar of the fauces and extending almost to the uvula, I removed the whole of the anterior pillar. The absence of adhesions and the small amount of contraction are marvellous. The rapidity with which fœtor disappears and the small amount of sepsis of the wounds when diathermy is employed are also remarkable. Pain is lessened and the patient becomes rapidly more comfortable, even when only a portion of the growth is removed. This patient was operated on a year ago and so far shows no signs of local recurrence. Unfortunately the glands on the right side of the neck suddenly enlarged a few days ago, but X-ray treatment has already caused a marked reduction in their size.

Dr. Irwin Moore: I am showing two water-colour drawings of a patient, aged 75, who was suffering from epithelioma of the right tonsil, anterior faucial pillar and side of the tongue, on whom Mr. Norman Patterson carried out complete removal *en masse* by means of the diathermy knife, after having first ligatured the external carotid artery. The patient was not in very good health and infirm, yet he suffered only slight inconvenience from the operation, and made a satisfactory recovery.

Mr. Cecil R. C. Lyster: I have had considerable experience with diathermy in cases of large masses of carcinoma which were utterly beyond any hope of deriving permanent good from ordinary surgical interference. The patients were dying of general septic poisoning, the symptoms of which in many cases were removed by the application of diathermy before the end came. Hæmorrhage has been a difficulty, but I have never seen it extensive enough to cause much trouble, even when the bleeding occurred from big vessels. One patient had a huge growth at the back of the scalp, nearly as large as his head: it was sloughing and smelt badly, and was altogether in a terrible state. All the septic portion was removed by diathermy, and a mass of coagulated surface was left behind, with a soft clotting of the big vessels. There was some bleeding, but nothing more than an ordinary dressing would stop. I also remember two cases in which fungating masses of the cervix uteri were removed by this method, and there again the hæmorrhage was not of much consequence. Treatment with diathermy has been most successful in papillomata of the bladder. I have assisted Colonel Johnson in several cases, which gave rise to no difficulty, and they all turned out well. The bladder-wall afterwards appeared to be quite normal. I have treated, and am still treating, a considerable number of cases of lupus and lupus erythematosus: the last named is very intractable, and so not much can be said as to results. But, generally speaking, diathermy is one of the most promising methods in electro-therapeutics.

Dr. F. HERNAMAN-JOHNSON: Does Dr. Cumberbatch consider that amputations can be done by diathermy in cases where ordinary surgery is too risky to life?

Dr. G. B. BATTEN: I heard Nagelschmidt's paper, and he said nothing as to how long it took for the cavity produced by a diathermy operation to fill up with granulations and heal over. He described removal of the breast, and left the impression that the last thing one saw of the case was a sloughed mass: he did not tell us what happened afterwards. Neither does Dr. Cumberbatch tell us how long it takes for the cavity to fill up. In the treatment of subcutaneous nævi, what would determine Dr. Cumberbatch to employ diathermy in preference to the application of carbonic acid freezing? Dr. Cumberbatch also says that he has had difficulty in getting needles insulated which would go through the skin without destroying the insulation. I suggest the use of the material employed in wireless work—iron or hard copper wire covered with black enamel. The insulation is sufficient for high-tension wireless, and I think it could be sharpened by a file and be available for nævi.

Dr. DAN MCKENZIE: The apparatus we have been using at the Central London Throat and Ear Hospital, is the small pattern supplied by Schall. It has been in constant use now for about four years and is satisfactory. The method we had of testing the working of the machine before operation was to lay upon the broad electrode a piece of raw meat and then to apply the pointed electrode to it. But the introduction of meat rationing has rendered the method no longer possible! Diathermy in our hands has been used chiefly for cancer of the pharynx, and as a remedial agent it has proved serviceable. I do not think, however, that we have so far had a case in which the disease has not recurred. But in one case which I showed a couple of years ago at the Section of Otology of extensive epithelioma of the auricle and mastoid region there has been so far no recurrence. And this result is the more gratifying since the soft tissues over the mastoid region down to and including the periosteum were destroyed by the heat. The bare area left was grafted, and healed up very well. The auricle was not entirely removed, only that portion of it being removed upon which the epithelioma was situated. The patient was a man aged over 70. We regard diathermy as an agent of the utmost value in keeping patients with throat cancer alive and comfortable.

We attack epitheliomata inside the pharynx without hesitation save when they have invaded bone, and the consequence invariably has been that the wound has healed up without reaction, with little or no pain, and with a smooth thin scar. Not only do we attack the growth inside the pharynx, but also in otherwise inoperable cases when the glands have become involved, and even when the growth has extended beyond the glands into the neighbouring tissues. Mr. Stuart-Low first applied diathermy to cancerous glands in the neck in this way, and I have followed his example freely, with great advantage to the patient. I have also applied the diathermy needle in repeated and deep punctures in the zones between the primary growth and the infected glands. Lastly, I have recently been following the growth even after the skin is involved, destroying as much of it as can be reached. The results have been encouraging. It seems that old, extensive, and advanced disease can be dealt with, and will respond favourably even when the operation is not complete, in which respect it contrasts with any cutting operation. There is no doubt that we have contrived to postpone the fatal issue in many cases, and that we have been able to relieve pain. We have not had so far any fatalities directly attributable to the diathermy. One or two cases showed a rise of temperature in the twenty-four hours following operation, but it was slight and transient. The majority show no general reaction whatever. Locally, a certain amount of œdema is noticeable after the application, but the amount of pain felt is obviously negligible, as patients are able to sit up and eat next day. Our exposures vary in duration from five to fifteen minutes only. I have tried diathermy in one case of pharyngeal sarcoma, and was disappointed with the result. The patient was a man aged 50, and the disease began in the left tonsil. The glands were involved, and the case was inoperable by ordinary methods. One application of diathermy was made to the tonsil, but so upset was the patient by the subsequent pain that he declined any further interference. And the rate of progress of the disease was certainly not appreciably slowed by our effort. In addition to malignant disease I have used diathermy for breaking down synechiæ in the nose (under local anæsthesia which is not very satisfactory). Avoiding contact with bone one finds that such synechiæ do not recur after diathermy as they do after other methods. I lately also tried it for one of those obstinate cases of adhesion of the soft palate to the posterior pharyngeal wall, but although I got a free passage into the nose it closed up again, and the patient, a girl aged 15, is still unable to breathe through the nose. Another condition in which I have tried it by multiple needle puncture is congenital nævus of the nose in an infant. We have made four applications, and the swelling is decidedly reduced in size. I have also destroyed enlarged tonsils in adults by diathermy, leaving intact faucial pillars and a smooth scar. As there is no hæmorrhage this plan may come to be more extensively employed. On the whole we have found diathermy of considerable value, especially in the so-called "inoperable" cases of epithelioma of the pharynx.

Dr. CUMBERBATCH (in reply): If sparks fall on the tissue, there is very little penetration of heat: there is merely a charring of the surface without destruction of the tissue for more than a trifling distance below. Actual removal of the growth by cutting around it in tissue coagulated by diathermy will, in suitable cases, yield the best results. Recently we had, at St. Bartholomew's Hospital, a case of malignant growth of the soft palate, in which we were able to remove the whole growth and part of the soft palate in the way mentioned, with very little bleeding, and the case is progressing satisfactorily. I am glad to hear Mr. Norman Patterson recommends ligature of arteries before diathermy. When this is done, there is much less fear of secondary hæmorrhage. Mr. Lyster mentions the coagulation of massive growths. In some similar cases I have obtained very good results. One was a large mass of growth which had appeared in the region of the scar following a Halsted operation for carcinoma of the breast. The growth was of the size of a coco-nut. A year after the operation by diathermy, the site of the growth was occupied by normal skin. There was a small sinus at one end. With regard to inoperable growths of the cervix uteri, I have treated seven such cases by diathermy, but they were so far advanced that I did not expect to get a good result: the uterus was fixed, and the growth had extended into the pelvic cellular tissue. When it is in the broad ligament it is out of the reach of the electrode, so the treatment is bound to fail. In all those cases which I have treated by diathermy, except one, there was increase of bleeding afterwards. With regard to Dr. Hernaman-Johnson's suggestion to employ diathermy to remove extremities, the chief difficulty would be the prevention of hæmorrhage. The diathermic cautery scalpel would seal the capillaries and the minute arterioles and venules as it cut them across, but vessels of larger size would have to be ligatured, with the possible risk of spread of infection. Nagelschmidt mentions the treatment of infective periostitis by diathermy to destroy the bone and periosteum. I should be afraid to try such a treatment because if any pyogenic organisms escaped destruction they would grow and the suppuration would probably increase. Massive nævi cannot be so effectively treated by CO_2 snow as by diathermy because the cold spreads for such a short distance beyond the site of application of the snow. In answer to Dr. Batten's suggestion, needles coated with the covering of which he speaks might be tried, but the insulation would probably be rubbed off as the needles were pushed through the skin.

Section of Electro-Therapeutics.

President—Dr. G. HARRISON ORTON.

The Irritant Nature of War Scar Tissue.[1]

By EDRED M. CORNER, M.C., F.R.C.S.

PREVIOUS to this war, scar tissue was only troublesome on occasions. It consisted of a matrix of fibrous tissue in which were few round cells and still fewer organisms. These latter signs of irritation grew progressively fewer and fewer as the scar tissue grew older. Finally, when it became painless the inflammation had subsided. There can be no tissue of greater importance than this to the surgeon. In war wounds, and especially in amputation stumps, these conditions are increased and exaggerated. The presence of inflammation is great, and organisms are plentiful. They exist in amputation stumps far from the wounds, the inflammation and its causes being carried there probably by the lymphatic stream. In this way metallic masses, metallic powders, particles of vegetable fibre and silk, and organisms have been detected. In one case micro-organisms were grown from the nerve end three years after the wound was healed. The patient was a young man who had had his arm amputated through the humerus. He wore an artificial limb for over two years. Then painful swelling appeared on the inner side of the arm, preventing him from wearing the artificial arm and causing him, *three years* after the healing of the wound, to seek relief at St. Thomas's Hospital as a discharged man. The swelling on the inner side of his arm was his nerve endings, which were excised, and one, touched only by forceps, was dropped into culture tubes and grown, anaërobically, by Captain Weir. From this grew a mixture of staphylococci, colon bacilli, and a diplococcus, which

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[1] At a meeting of the Section, held May 17, 1918.

Professor Marinesco showed at the Royal Society of Medicine. This shows that the scar tissue of war wounds is irritative in varying degree. Spontaneously it can give rise to abscesses or be very slow to heal. It cannot surely and safely encapsulate or render harmless any foreign body such as ligature silk (fig. 1), which tends to escape upwards to the tissues above, particularly those around the vessels or in the nerves. Or it may escape outwards forming an abscess or a sinus. Silk should never be used in war wounds. It may be a carrier of much irritation and many organisms. The wound may be in the leg, the amputation may be through the knee, and the ascending irritation may cause inflammation in the muscles, arteries, veins, lymphatics, bone, nerves and connective tissues of the thigh. Each of these is innervated and a nerve, no matter its function, meeting a focus of irritation gives the sensation of pain (fig. 2). This is where the help of this Section will be most required. The causation of this pain, being understood, its occurrence will be lessened and its treatment should be undertaken

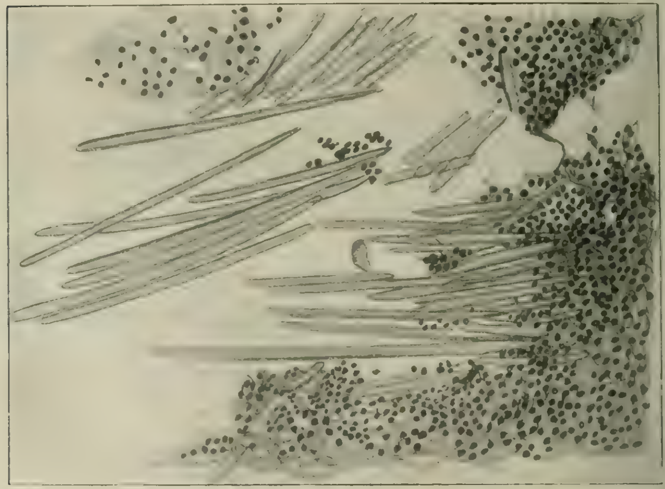

FIG. 1.

Silk ligature in scar tissue. (C——.)

rather by the electro-therapeutist than by the neurologist or the surgeon.

Whenever a nerve is cut it must regenerate. No one can stop the regeneration—it is like the flow of a river. Its tide flows on and meeting an obstruction pools up and up until the obstruction is passed. This mass is a true regeneration neuroma. It is well to know of it, as a regeneration neuroma may be responsible for the recurrence of pain in an amputation stump.

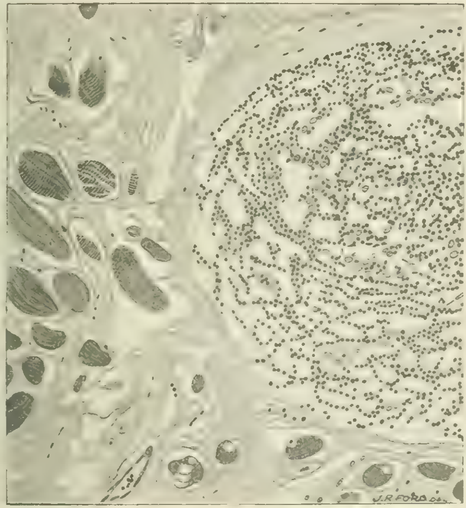

FIG. 2.

Nerve trunks and a nodule of inflammation. (R——.)

As nerve fibres regenerate they branch and branch, every dichotomy making it more likely that the fibres will meet irritant connective tissue, completing the equation: Nerve + focus of inflammation = pain. Moreover these nerves are at first naked axis-cylinders unprotected by any myelin sheath, and so peculiarly vulnerable to the attack of any focus of irritation.

It must also be borne in mind that the regeneration in amputation stumps is purposeless. Elsewhere it is purposeful, aiming at innervating the parts that have been deinnervated. In amputations the deinnervated parts have been removed and goal of the regeneration taken away. The new regeneration fibres are like sheep without a shepherd, and innervate every structure in the amputation stump—vessels, muscles, bone, &c. In this extensive course they are practically certain to meet irritant scar tissue.

I want to show you how the irritation streams off the upper surface of this scar tissue like smoke streams off the fire. It ascends the limb along every connective tissue plane and enters the substance of every structure whose end has been left open, causing both an endo- and a peri-inflammation. Hence the endoneuritis, of ascending nature, and the adhesions of perineuritis. Hence also the myositis of the brown muscles, the osteomyelitis of the bone and the new periosteal outgrowths from irritation of the bone ends. The principal channel by which irritation ascends is along the vessels. Here we get adhesions, arteritis, endarteritis, periarteritis, and thrombosis (arterial, venous and lymphatic). In this way the large neurovascular bundles on the inner side of the upper arm are accounted for. In the thigh no homologue has been sought. It exists in Hunter's canal and its contents. No more suitable chimney can exist, up which the irritation of infective inflammation can ascend. It ascends along the popliteal artery to where that structure pierces the adductor magnus. There the surrounding inflammation makes the muscle adherent to the vessel and a bridge for the inflammation and infection to extend to the territory of the obturator nerve. Hence the painful masses and the outgrowths of bone on the inner side of the femur. Above the aperture in the adductor magnus the ascending inflammation infects any ligatures on the femoral artery and vein. Should those ligatures be of an unabsorbable material like silk they will remain as "internal setons" and perpetual irritants. Moreover, the contraction of the adductor magnus is directly communicated by the adhesions to the internal saphenous nerve with painful consequences. Usually the femur is externally rotated so that the adductor magnus acting as an extensor contracts with every movement of the stump. Hence the great and persistent pain often put down to neurosis.

It is very difficult except by a clean and distant amputation to conquer this pain. A reamputation means the loss of much power and usefulness to the patient. Therefore I would suggest that the work of this department should aim at meeting this trouble. That is why I have come and explained exactly the nature of the tissue that doctors are called upon to treat. On their labours much of the comfort and usefulness of the patients' after-life must rest.

Finally, I want to lay emphasis on the point that healing and closure of the wound does not signify that treatment has ended, nor that processes which may lead to trouble have ceased. It would be better if your treatment were begun before the wound had healed and continued more vigorously, with massage movement and vibration in addition, afterwards, and for some time. Then let the artificial limb be fitted.

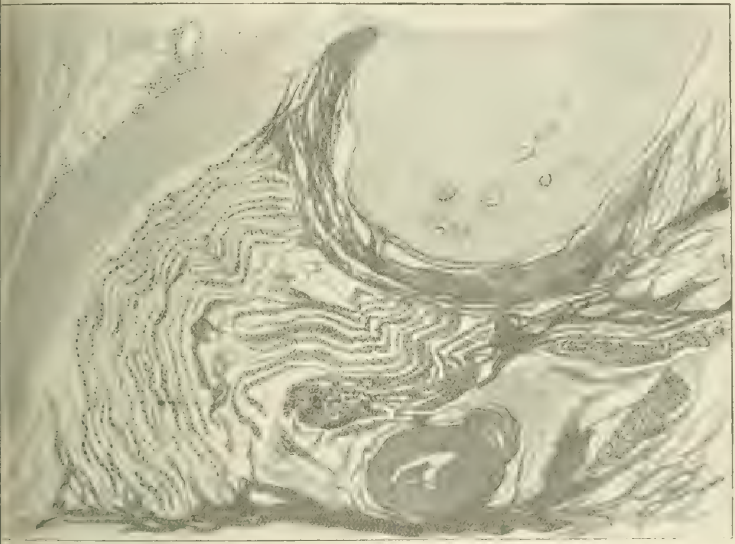

FIG. 3.

Inflammation around bony spur of tibia (not relieved by treatment of nerve trunks). (T——.)

DISCUSSION.

Mr. NOEL BURKE: In the hospital at Epsom in which I have been working, we did, I think, in a rough unscientific way, recognize the peculiar character of the scar tissue which results from the wounds of war. As long ago as 1915 we began to call it by the name of "war-tissue." Some of the cases that were mentioned by Mr. Corner had pain due to traumatic neuritis, and these can usually be relieved, temporarily at any rate, by the galvanic current. There is no doubt that scar tissue can be softened by this means, as also by X-rays and by radium. The secret of treatment of these conditions of excessive scar lies chiefly in their prevention. If one can get these wounds to heal quickly one will have less scar tissue produced, and to make them heal quickly is one of the greatest properties of electro-therapy. Whether by Russ's method, by ionization with salicyl, iodine, zinc, or other ions, by high frequency effleuve, or by a combination of ionization and ultra-violet light, as described by Dr. Turrell two years ago, no wound of soft tissues should fail to heal rapidly under electrical treatment; and the healing is done with the minimum amount of scar tissue, and that which is formed is more soft and subtle than in ordinary cases. Of course one cannot ionize a sequestrum, a piece of shell, or of khaki tunic, or similar gross foreign body; these the surgeon must remove; but it is possible that the treatment may stimulate the tissues sufficiently to restore their power to encapsulate small foreign substances such as the shell-dust and iron particles that have been described to-night. If the surgeons will only send their septic wounds early to the electrical departments for treatment, I am sure that these troublesome results of war-tissue will become more rare.

Dr. HERNAMAN JOHNSON: Mr. Corner's paper represents a new surgical departure—a definite request for the co-operation of electro-therapeutists in surgical matters. There are no two branches of practice where co-operation is fraught with greater benefit to the patient. The congested, unhealthy, painful and adherent scar so frequently seen may often be rendered supple and painless by a combination of X-rays and diathermy. The rays should be heavily filtered, and given in very small doses, two to three times a week. No visible reaction occurs. The galvanic current I have also found of use. The particular substance used to moisten the pads does not seem to make much difference when the skin is unbroken; but if iodine is used, the correct method is to employ the negative pole soaked in a 2 per cent. solution of potassium or lithium iodide. As regards the treatment of suppurating wounds, the best results are obtained if electricity and radiation are employed *at the bedside*. It is infinitely more easy to *prevent* the formation of unhealthy scar tissue than to deal with it when established. In the early stages when a wound is inflamed and painful the high candle-power incandescent lamp is advisable, but the glass must be of suitable quality. Later, the tungsten arc and X-rays may be employed. Zinc ionization is of service where great sluggishness exists. The negative pole is dangerous where open wounds are concerned, general disturbance being apt to follow. A wound that is carefully watched and treated will heal with the minimum of scar. If at any period it seems to be "hanging fire" after appropriate physical measures, it should be surgically explored.

Dr. ETTIE SAYER: From our point of view success in wound treatment depends, of course, upon beginning electricity immediately after infliction. The fractures, motor-car smashes, &c., which occur in civil practice do so infinitely better than anything seen in military work, because the "psychological moment" is not lost. *Adhesions:* I would define traumatic adhesions as "formations in a wound due to the absence of electricity from its initial treatment." Wherever adhesions can be remedied they ought to have been prevented. Wherever X-rays, radium or ionization can dissolve and disperse adhesions then those adhesions ought to have been prevented by electrical breezes in the early hours of treatment. The absence of static machines from those big wards in the French coast hospitals is one of the tragedies of the war—for they could so easily be installed. There is no wound of flesh or bone to which an electrical breeze in addition to the ordinary resources of surgery is not an additional comfort. To treat flesh wounds without electricity is like treating fractures without massage. The prejudice against the latter is past, but the prejudice against the former has still to be overcome. Concerning *iodine ionization*—especially for those cases with inflammatory foci, the best method is that of Professor Leduc, of Nantes. Give 15 gr. of potassium iodide one hour before treatment. The presence of iodine circulating in the blood can now be demonstrated by the patient spitting on a piece of starch paper. Circulating iodine carries a negative charge, so the positive pole—which happens to be the salutary one for these wounds—is applied, and nascent iodine is thus liberated in its vicinity. For quiet hard adhesions a few chlorine ionizations are often most valuable. In other cases X-rays act like a miracle.

Mr. E. M. CORNER (in reply): It is generally believed that an amputation puts a speedy end to the patient's illness. So it does in about 85 per cent. of the cases, but only too frequently an amputation is the first stage in a long and painful illness. I hope that by combined electro-therapeutic and surgical work this will be avoided.

A Mobile X-ray Wagon Unit.

By Howard C. Head.

(ABSTRACT.)

The present plant, which has been equipped by Messrs. Siemens Brothers and Co., Ltd., and constructed on an Austin motor lorry, embodies all the latest and up-to-date improvements applicable to this type of plant. This particular chassis was chosen on account of its low loading line, as this enabled most of the apparatus to be utilized in conjunction with the portable X-ray tent without removal from the car. The ordinary type of chassis is too high to permit of this and requires a step ladder to enter it. Owing to the low loading line the radiologist is able to manipulate all the switch-gear without requiring the apparatus to be moved into the portable X-ray theatre, and, further, as all the weight is kept low, the car is practically immune from side slip. The body fitted to the chassis is of panelled teak, this being chosen as the car is for service in Mesopotamia, and measures 12 ft. 9 in. long, 7 ft. wide and 7 ft. high, and is divided into two compartments, one the photographic dark room and the other the room containing the X-ray apparatus. The dark room, which measures 3 ft. 4 in. long by 7 ft. wide, is equipped with every detail required for carrying out the photographic work, and in addition the portable X-ray theatre constructed of a framework of steel poles and tie-rods is erected at the side of the car, over which a light-tight tent is fitted. The X-ray tent measures 12 ft. 9 in. long by 10 ft. 6 in. deep, and between 7 ft. and 9 ft. high, and is carried on the roof of the car.

A special feature is the electrical equipment of the vehicle. A 3-k.w. generator is placed to one side of the engine and is coupled thereto by means of a rocker chain, and gives an output of 20 amp. at 150 volts when the engine is running at 1,700 revolutions per minute. The generator can be decoupled when the car is running on the road, and is only recoupled when it is necessary to work the X-ray apparatus, or to charge the special battery of accumulators. The battery of accumulators is fitted in two special cupboards at the back of the driver's cab, and the doors of the cupboard are fitted with louvres

to ensure perfect ventilation. Owing to this arrangement it is impossible for acid fumes to enter the car. The battery is constructed of special ebonite composition boxes, the material being selected to withstand tropical climates. It consists of thirty-eight two-compartment 4-volt units, making seventy-six in all of the sealed-in type, and the capacity of each cell is 80 amp. (intermittent) or 10 amp. for two and a half hours, or 30 amp. for five minutes. The radiologist is able to draw his supply either direct from the generator, or from the battery alone, or from the battery and generator in parallel, so he is therefore provided with a supply of energy capable of undertaking the quickest work and the most difficult cases.

The X-ray outfit includes a 16 in. spark heavy discharge induction coil with variable primary, three interrupters—namely, an electrolytic interrupter, centrifugal mercury interrupter and a dipper mercury interrupter, all connected up to one switchboard, so that any one of the interrupters can be used at will.

Some of the apparatus in the equipment of the car has been selected of the standard War Office patterns, so that spares may be obtainable easily and quickly, if necessary.

Among the special additions are a Coolidge control outfit and Coolidge tube, water-cooled tungsten tubes and a special stereoscope attached to the roof, by which stereoscopic radiographs may be viewed in the car itself.

The whole of the control of the generating plant and the X-ray apparatus can be undertaken from one position, that is at the switchboard, by the operator in the portable X-ray tent, that is to say, the engine can be started by using the generator fed from the battery as a self-starter. The generator or battery, or both, can be operated by closing the necessary switches on the switchboard, and any of the interrupters can be connected to the coil for carrying out the particular X-ray work in hand, and the whole plant shut down and the engine stopped. Further arrangements are made so that X-ray work can be done either in the car itself, in the portable X-ray theatre, and by means of cable drums the X-ray control board, induction coil and couch can be taken into any existing or temporary hospital and all work carried out there.

It is possible that such a unit would be of great utility after the war for use in scattered areas or mining districts where no established X-ray plant is available.

(1) A Portable "Snook" Apparatus. (2) A Transformer for heating current of the Coolidge Tube.[1]

By E. E. Burnside.

(ABSTRACT.)

[A full report of these papers will appear shortly in the *Archives of Radiology and Electrotherapy.*]

(1) In describing the portable Snook apparatus, the author said it was difficult to do so without to some extent going over the principle of Snook interrupterless machines generally, some details of which were no doubt already familiar to members of the Section.

He exhibited some slides illustrating the rectifier of the machine, which showed very clearly the manner in which the high tension current is converted into a uni-directional one by a simple revolving disk.

Referring to these machines he pointed out the advantage of an alternating current supply main over a continuous current one, mentioning that such currents are now certainly to be preferred, rather than being considered the bugbear of the radiologist as hitherto.

The author described experiments carried out by Dr. Reginald Morton some years ago to ascertain the best fraction of the alternating current wave to produce the maximum X-ray efficiency with the minimum heating effect in the tube.

Reference was made to the small alternating current apparatus recently produced in America for use with the Coolidge tube, but which was at present limited in application, as it would work a Coolidge tube only, whereas the Snook transformer would work any ordinary tube.

The Snook apparatus over an induction coil is mounted on castors for wheeling about hospital wards, and is fitted with handles enabling it to be carried upstairs by two men without difficulty.

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[1] At a meeting of the Section, held January 18, 1918.

(2) The second paper described a type of transformer designed for heating the filament of a Coolidge tube, the adjustment of which was mounted on the trolley switch table of the Snook, or other high tension apparatus. This enables both the penetration and quantity of rays to be controlled from the side of the couch or screening stand.

The following communications have also been made before the Section :—

EXHIBITION :

DAN McKENZIE, M.D., and ROBERT KNOX, M.D. : "Stereoscopic Radiograms to illustrate the Anatomy of the Temporal Bone, and particularly the Fallopian Canal." October 19, 1918. Printed *in extenso*, with illustrations, in *Archives of Radiology and Electrotherapy*, June 1918, p. 18.

DEMONSTRATIONS :

M. LUBOSHEZ : "X-ray Films and their Development." November 16, 1917.

H. M. JOHNSTON and W. KELSEY FRY : "Slides and Radiographs to illustrate the Technique employed in the Radiography of Injuries of the Lower Jaw." January 18, 1918.

C. W. S. SABERTON, M.D. : "Demonstration of a number of Lantern Slides showing Opaque Food in the Stomach." January 18, 1918.

H. C. HEAD : "A new Tungsten Lamp for the production of Ultra-violet Radiation, and a new Diathermy Apparatus." February 15, 1918.

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SESSION 1917-18

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1918

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Section of Epidemiology and State Medicine.

President—Dr. G. S. BUCHANAN.

(*December* 7, 1917.)

PRESIDENT'S ADDRESS.

Epidemics of the Eastern Campaigns.

By G. S. BUCHANAN, M.D., Hon. Lieutenant-Colonel R.A.M.C.

WHEN in 1915 the Section appointed me its President I was away in the Mediterranean, and as events proved it was impossible for me to do my duty to the Section during any part of last session. This is consequently the first occasion I have had to thank the Section for its nomination, the honour of which I am deeply sensible; the more so as, in the old Epidemiological Society, whose work we continue here, my father was an original member, and President from 1880 to 1882, and I have myself found so much help and inspiration from the meetings and discussions of the Society and Section ever since my membership of them.

The principal work of the Section has always lain in the study and discussion of the broad facts of the incidence and natural history of epidemic diseases, and I have thought it might be useful in this address to give a brief review of those which, within my experience, have been most in evidence in the Eastern campaigns. My opportunities for knowing them have been obtained as a member of a Committee, described under various names, but usually as the Medical Advisory Committee, which was dispatched by the War Office in May, 1915, under the chairmanship of Colonel W. Hunter, for service with Surgeon-

General Babtie in the Mediterranean area—meaning, at that time, Egypt and Gallipoli—with a wide reference for consultation on the prevention of epidemic diseases.

After preliminary work in Egypt this Committee went to Mudros, and to each of the several occupied areas of the Gallipoli Peninsula. In November, 1915, the Committee was sent to Salonica, where British forces had lately been landed, and visited the corner of the Balkans which lies above Lake Doiran. A more comprehensive visit to the forces in Macedonia was made in the following May (1916) the interval being occupied by work in Egypt. Later in the year the bulk of the Committee, with Lieutenant-Colonel Andrew Balfour as Chairman, was dispatched to Mesopotamia, which we reached at the end of August. Here our inquiries took us up the Tigris as far as the outskirts of Kut-el-Amara, which at that time was the furthest point of the British force; up the Euphrates to a corresponding point; up the Karun River, and to various base areas. The period was that of the reorganization and strengthening of the Mesopotamia forces which preceded the great and successful attack on the Tigris front at the end of February in this year, and the subsequent advance to Bagdad. A return via India and Egypt completed the work of the Committee which returned home last May after two years' duty.

I feel that in some ways it might be more profitable to the Section if from all these wanderings I had picked out a single disease or a single epidemic for observations. But any really intensive local study of this kind was hardly practicable under the conditions of the work, and my available notes on any one outbreak have too many lacunæ to be satisfactory. And I believe that on the other hand a review of some general impressions of all the main epidemics met with will not be without interest; there is value sometimes even in a *catalogue raisonné*. I will arrange it by first referring to epidemics of excretal origin, dysentery, cholera, and so on; next, malaria and trench fever; then epidemics which, like sandfly fever, are particularly Eastern; with a note, finally, on the prevalence of diseases due to food deficiency.

DYSENTERY AND DIARRHŒA.

I may begin with dysentery, and dwell on it somewhat longer than other diseases, on account of its ubiquity and certain special features which it presents. The great dysentery outbreak of the Eastern campaigns was, of course, that at Gallipoli in 1915. It was dysentery and associated diarrhœa, together with diseases of the enterica group, which at once claimed attention on arrival in Egypt and in Mudros that summer, among troops stationed there, and particularly among the men invalided back from the Gallipoli Peninsula. And on reaching that war area one came to realize the magnitude and intensity of the epidemic of intestinal infections which prevailed on each of our three portions, Cape Helles, Anzac, and Suvla Bay, during the summer and autumn of 1915, which was causing men to be poured off on to hospital ships by the hundred, sometimes by the thousand, every week, and which was mainly epidemic dysentery.

No other outbreak of dysentery at all comparable in magnitude has, fortunately, since occurred in any of the other Eastern war areas. Salonica has been far more satisfactory in this respect; dysentery was introduced into the Salonica force at, or soon after, the original landing towards the end of 1915, and some local prevalence resulted, which soon disappeared in the winter months. A recrudescence appeared in certain units in May, 1916, but the incidence was not great, and generally I understand that the prevalence of this disease in Macedonia has rather resembled the low level hitherto attained in France than that met with in the other Eastern war areas. In Egypt, through 1915 and 1916, there were a number of local outbreaks of dysentery, though few of any great severity, together with a general sporadic prevalence which only lessened in the winter months, November to March. In Mesopotamia we learnt, on arrival in August, 1916, that there had been much epidemic dysentery in the earlier part of that year, both among the troops taking part in the operations for the relief of Kut and at Basra and other base areas. During the time I was in Mesopotamia, in the latter part of that year, the dysentery record was, however, relatively good; there was a steady sporadic incidence of cases (represented by from 0·1 to 0·2 admissions, originally reported as dysentery, per 1,000 of the total force each week) and no very severe local epidemics.

I should have been glad to illustrate these very vague statements about dysentery prevalence by a series of figures and curves for the different fronts, but although this could be done with some confidence for Egypt in 1916, and for the latter part of that year in Mesopotamia, the series would still be too incomplete (particularly as regards Gallipoli and Mesopotamia in 1915) to be satisfactory, and would require too many reservations. It would also entail a use of figures as to the strength of the forces which might not be considered advisable even at the present time. It is well, however, to give some indication of seasonal prevalence. That at Gallipoli, beginning at various dates after the different landings, reached a maximum in September, and fell very sharply at the end of October. No very pronounced seasonal curve was observed in Mesopotamia up to December last. In Egypt, as already said, the period of low prevalence was the cooler months, December to March, a somewhat abrupt rise taking place in the curve in March and a corresponding fall at the end of the year. I am not one of those who find an explanation for all dysentery infection in the activities of the fly, much as I have seen of the massive attack which these insects can make on the soldier, and on his latrines and his food if they are accessible. But I am certainly disposed to agree with those who attribute the seasonal rises and falls which I have mentioned to the increase or diminution of fly activity. The correspondence was close; no other explanation fits the facts more nearly; and the explanation is in itself almost self evident to those on the spot. I think the term "fly activity" preferable in this connexion to "fly prevalence." Towards the end of the summer, in the hot climates in question, flies may be extraordinarily abundant, but they are often in a state of torpor. They just hang in one place, dropping down to their nearest meal, with none of the spirit of adventure which gives them their infecting ability in the earlier months.

While referring to dysentery statistics, it may be useful to point out that their comparability may be seriously impaired by different customs in the use of the term. This not only arises in times of stress, such as at Gallipoli, which do not favour accurate observation and diagnosis, but also may result from administrative practice. Naturally if, in pursuance of a general order, every case, once it is labelled dysentery, is sent overseas, or if, to take another illustration, no case is to be termed dysentery without a laboratory diagnosis, there will be inevitable artificial restrictions or enlargements of the term, to the despair of the ordinary statistician. The practice was finally adopted in Mesopotamia of applying the term dysentery to every case with symptoms of enterocolitis in which blood and mucus had been found, provided that the fact of passing blood and mucus had been ascertained by inspection of the stools. This purely clinical interpretation, which I believe is now generally adopted, seems the one most likely to give useful results. It recognizes that in dysentery, as in other infectious diseases, there are cases of all degrees of severity, that some are quite capable of being cured locally, and that no one hard and fast administrative line need necessarily be applied to the whole group. The argument that it increases the number of cases and so, to a superficial observer, "looks bad" for the health record of the force, is not one that ought to weigh; if it is given weight it may be counterbalanced by the correspondingly low rate of case mortality which can be adduced.

So far I have spoken of this disease as an entity—as of a single malady to be thought of, epidemiologically, as just "dysentery." The proceeding at first sight will seem out of date, when account is taken of the distinct specific causes of the clinical condition above defined—the Shiga and Flexner groups of bacilli, the *Entamœba histolytica*, and more rarely certain other protozoa of facultative pathogenicity. It has however substantial justification when one considers the broad results of the mass of work on the subject which has been carried out at base and field laboratories in the examination of the dejecta of dysentery cases. I have not heard of a single outbreak or local prevalence of dysentery being found to be associated practically with Shiga's bacillus alone, or with Flexner's bacillus alone. Always the laboratory experience has been the same—figures showing no doubt that one or another of these groups has predominated at a particular time or place, but invariably a substantial mixture of both. Not only so, but usually there were other associated cases in which various organisms of the same main group were recovered from the fæces, that in respect of sugar or agglutination reactions conformed neither to the Shiga nor to the Flexner organism, and whose pathogenicity is insufficiently known.

It is in this connexion interesting to note that in France the recent bacteriological findings also indicate the simultaneous operation during dysentery prevalence of a number of different bacterial types—in other words, that a local outbreak of dysentery is to be referred to a plurality of original infecting cases or carriers of different sorts, and not merely to the original chance introduction of infection by some one case or carrier.

But, it will be said, amœbic dysentery at least is quite another matter; the origin and spread of a protozoal infection is on a different plane to bacillary infection, and its epidemics should surely in some cases be distinct. This was certainly what I expected to find when going out in 1915, knowing very little of dysentery on the practical side. And at first it seemed true as regards Gallipoli. Many observations were made as soon as the dysentery cases reached Mudros hospitals, and others on the Peninsula itself, or as men were being passed on to the hospital ships, from which one gathered on good pathological authority that free amœbæ in the greatest abundance were being found, not only in the freshly passed mucus of case after case of undoubted dysentery, but also in the fæces of many cases merely classified as diarrhœa. Admittedly the discrimination between the pathogenic *Entamœba histolytica* and the saprophytic *Entamœba coli* was almost impossible in the free amœbic form, and it had not been seriously attempted. But when the histological findings pointed to an intense epidemic, characterized by the presence of an abundance of amœbæ in practically every recent case examined, this seemed of little consequence. Moreover, certain fatal cases had presented the post-mortem characteristics of amœbic dysentery, and the cysts of *Entamœba histolytica* had been found in the stools of many cases from the Peninsula which reached Alexandria. As a working hypothesis, at any rate, here were grounds for regarding the whole outbreak as one of acute amœbic dysentery in which amœbic infection had been originally contracted by the troops whilst waiting in Egypt, and had spread wholesale and rapidly under the conditions met with in Gallipoli; with my colleagues I signed one or two reports in 1915 in which this hypothesis was fully accepted. In view of subsequent experience I do not now think this was a correct view of the case. One understands now that the disintegration of tissue and lymphocytosis of acute bacillary dysentery may produce mucus with macrophages closely resembling free amœbæ, and that when towards the end of the year more thorough bacteriological examinations were made of dysentery cases from the Gallipoli Peninsula, dysentery bacilli were freely found. In the many epidemics which have been studied since, in Egypt, Mesopotamia and elsewhere, there has not been one which showed a preponderating acute amœbic infection. Moreover, the extensive experimental work subsequently carried out in Egypt, besides that done for the Medical Research Committee and in other laboratories at home, has showed no such excess of chronic carriers of *Entamœba histolytica* among dysentery convalescents from the Gallipoli Peninsula as might have been expected if the nature of the Gallipoli outbreak had been so dominantly amœbic as was supposed.

In subsequent epidemics in other areas in which good protozoological work has been carried out and the matter tested by search for and identification of cysts (to which as a rule the protozoologist in search of amœbic infection limits himself), it has practically always been found that a substantial proportion of dysentery cases have yielded evidence of *Entamœba histolytica*, but always a minority—seldom over 20 per cent.

The significance of this figure is however largely discounted by the discovery that cysts are to be found in a large proportion of men not known to have suffered from dysentery at all. This was fully shown in Egypt, where Wenyon and O'Connor found over 5 per cent. of healthy men to show histolytica cysts in a single sample of fæces, and it has been confirmed elsewhere for men returning from the East. Histolytica cysts have even been found in healthy men who have never been farther than France, and (to judge by the recent Liverpool reports) in some who have never been outside England.

With the new work on carrier cases in mind, and with the laboratory data afterwards available in Egypt and in Mesopotamia, one thus comes to regard amœbic dysentery rather in the light of a concomitant of an epidemic due to other infections, highly important to know of for purposes of treatment and in the interests of the individual sufferer, but still always subsidiary to the main infections which produced the outbreak. The presumption was that men in these Eastern campaigns frequently ingest histolytica cysts, and give a lodgment to the resulting amœbæ which will at most produce a passive ulceration and seldom enough destruction of tissue to cause any very definite symptoms—unless or until there supervenes an acute intestinal disturbance, by irritating food, by food poisoning, by any of the ordinary causes of diarrhœa, or still more by a Flexner, Shiga, or other bacillary dysentery infection. When this happens, a definite amœbic ulceration may follow, with all its clinical consequences. The disease may then, in the absence of effective treatment, run its usual protracted course, the effects of the amœbic ulceration in such cases replacing or overshadowing those of the original bacillary infection or the initial irritation which brought about these results.

From such a view as this—and I believe it is after all quite an old one—it must by no means be deduced that the prevention of amœbic infection is of subsidiary importance; no one who has become familiar with the havoc on men's activities which may be wrought by chronic amœbic dysentery—minimized as it is by modern successful methods of administering emetine—could encourage such a belief. On the matter of prevention, however, there are fortunately few directions in which amœbic and bacillary dysentery differ. The most important is probably their liability to transmission by water under field conditions: the ordinary chlorination of water supplies liable to fæcal contamination, such as one part of free chlorine per million for half an hour, which suffices to kill the bacilli, has no effect upon the histolytica cyst, which has been shown to be resistant to a ten-fold strength. I do not mean by this to lay too much stress on water carriage of dysentery infection. I believe it operated in Gallipoli and occasionally in Mesopotamia, but it was certainly only a subsidiary cause. All the evidence points to a very short viability of the dysentery bacillus in water—twenty-four to forty-eight hours at most—and in this it contrasts with enteric fever. On the other hand the cysts, like bacillary infection, have been shown to be carried freely by flies, and they are obviously liable to be spread, like bacillary infection, by all or any means which entail the direct or indirect contamination of food or drink, or of men's fingers, by fæcal matter, or even by its being swallowed when dried as dust.

One always comes back to this, that the prevention of dysentery, all dysentery, first and foremost is a question of practical field sanitation in its widest and most knowledgeable sense, measures which ensure that fæcal matter is at once burnt or deposited in fly and wind-proof pits or otherwise immediately and safely disposed of; measures for the detection of places which breed out flies, and for their capture; measures for the safety of drinking water, and the cleanly preparation and storage of food, the precautions at the cookhouse and messing places and so on. What this means, in infinite care and attention to details, often apparently trivial, by specialist, medical, and combatant officers and other ranks cannot here be specified; one can only pay a tribute of admiration to the results in the many places where these things were thought of and done to the greatest extent circumstances allowed, often under the greatest difficulties, discouragement and danger, alike in base areas, on lines of communication, and in the advanced lines, which often had better work to show in these directions than the base.

The association of epidemics of dysentery or of continued dysentery with failure to attend to these matters, from whatever cause it arose, was too close to escape even the most casual observation; one could give illustrations readily enough from Gallipoli, from West Mudros, and other areas.

Irritating and badly cooked food no doubt predispose to dysentery, and prompt treatment of any intestinal irritation is an important matter in the prevention of this disease. The Medical Advisory Committee emphasized in more than one of its reports that a timely dose of castor oil is one of the most essential prophylactic measures.

Another measure of the greatest value has been found to be the temporary removal at the earliest time of any diarrhœa cases, especially if they be men who have anything to do with the handling of food; their treatment in field ambulances or, in times of an epidemic, in a hospital unit specialized for the purpose.

Finally, there are the measures for the detection of carrier cases and preventing their return until the carrier condition is cured. This measure, whether directed to bacillary or to amœbic carriers, must of course be reasonably subordinated to military requirements. Its utility probably lies mainly in its excluding men from return to active service who, though apparently convalescent, are still suffering from some intestinal disturbance and are very likely to break down again when exposed to field conditions. I think this is specially the case as regards amœbic infection. Much could be said for the view that in future no routine examination for cyst carriers is necessary except in cases where the man is himself out of condition or suffering from chronic intestinal irregularities. In such cases search for and cure of the amœbic condition is essential. No doubt also the introduction of infection is prevented to some extent, but it cannot be represented—certainly not on Eastern fronts, and from recent inquiry I think this is true also in France—that the security of the forces against dysentery depends on maintaining water-tight exclusion of carrier cases or of the convalescent dysentery cases which remain carriers. It is a good practical rule in field sanitation to regard every healthy man as a possible carrier and take precautions accordingly.

ENTERICA.

The general use for epidemiological purposes of the term enterica, to cover diseases produced by the typhoid and paratyphoid bacilli, may be justified on grounds similar to those in favour of the generic term dysentery. So considered, one may say that enterica was met with in all the campaigns under consideration, in circumstances roughly parallel to the occurrence of dysentery, but with some important differences. As a whole there were far fewer cases of enterica than cases of dysentery. Except in Gallipoli and Mudros in 1915, enterica was also less widely diffused, and there have been many regions and important units which have remained almost free from enterica cases, certainly from anything which could be called enterica prevalence. In this matter again the Macedonian force had a good record. Egypt, on the other hand, showed a considerable prevalence of enterica in 1915, and also in the earlier part of 1916, at a time when a large number of troops returned from Gallipoli had reached Egypt. For Mesopotamia I have few data until the summer of 1916, when diagnosed enterica was of a low order of prevalence, contributing among the British troops a weekly rate of new admissions which was under 2 per 1,000 for the whole force, and falling steadily to 0·5 per 1,000 in December, while the corresponding Indian figures were considerably lower.

For purposes of ordinary preventive work in the field, and for stopping the spread of the disease when cases occur, it is of little consequence whether the enterica is typhoid, paratyphoid A, paratyphoid B, or combinations of these infections. In the matter of protective inoculation, however, the distinction is all important. In the Eastern areas, as no doubt elsewhere, one has learnt never to accept without close inquiry percentages of incidence of the different varieties of enterica, still less to compare those obtained by different methods. It is safer to judge from thorough sampling examinations at selected laboratories than to mass those from different laboratories, even when one excludes from consideration all serological tests and judges solely by cases in which the actual organism has been recovered, either from the blood or from the excreta.

Taking massed figures, however, the results from Gallipoli (1915), from Egypt (1916), from Salonica (January to May, 1916), and from Mesopotamia (August to December, 1916), were very much of the same order: of 100 enterica cases in which the organism was recovered the figure for true typhoid was about 20, that for paratyphoid A anything between 50 and 70, and for paratyphoid B 10 to 30.

It must not be assumed that these are proportions which hold good throughout: there were numerous cases for example in which a local prevalence has been almost wholly associated with paratyphoid A, without concomitant paratyphoid B, or typhoid. More rarely an outbreak almost exclusively paratyphoid B has been found—one such occurred in a section of the Eastern force in Egypt in 1916. As regards inoculation, I would here, without attempting figures, merely record the opinion, held I believe universally by all who had looked into the subject, that the anti-typhoid inoculation to which the great majority of the troops had already been subjected before reaching the Eastern areas and its systematic renewal is primarily responsible for the relatively small amount of true typhoid which prevailed. When one considers the conditions in Gallipoli at the time when so much infection of excretal origin was present, and was being manifested wholesale in dysentery and paratyphoid, it would be imperative, did one not know about inoculation, to seek for some special explanation of the extraordinary relative immunity from true typhoid. Antityphoid inoculation however supplies that explanation. The same may be said of the other fronts where experience has proved that similar causes of infection were operative. Another noteworthy point is that true typhoid, when it has occurred and been verified by the recovery of the typhoid bacillus, has so often been mild or anomalous in type. I have only seen one large collection of true typhoid cases with the regular clinical history that one used to associate with typhoid: characteristic charts, relapses, perforation, early and late hæmorrhages, and so forth. This was in 1915 in the recently arrived New Zealand force, which, as is well-known, was subjected to inoculation in New Zealand with a strain of vaccine that was almost certainly inert, and produced no local or general reactions.

The large amount of paratyphoid infection in Gallipoli was one among many powerful arguments for the introduction of the present triple (T.A.B.) vaccine, inoculation with which became general in 1916. I am not in possession of data sufficiently recent to be able to speak of its protective effect against the paratyphoid infections, though I believe it to have been considerable. Figures of typhoid incidence before and after the general T.A.B. inoculation, worked out by Colonel Hunter and Major Willoughby in Egypt, certainly pointed strongly in this direction, and the whole evidence available after the war will be of the greatest interest. It was anticipated from a knowledge of the shorter duration of the serum reaction after attacks of paratyphoid A that the immunity conferred by a paratyphoid A vaccine might be slighter or shorter in duration than that conferred by the typhoid vaccine against typhoid. Several cases in Mesopotamia, in which men were attacked by definite paratyphoid A within one or two months of their inoculation with triple vaccine, pointed in this direction; it should however be added that the vaccine used at that time in Mesopotamia was, as regards paratyphoid A, only half the bacterial strength of that used at home and in Egypt.

CAMP JAUNDICE.

Not so long ago I should have been disposed to class camp jaundice with dysentery and typhoid as a disease probably due to the same kind of causes, to insanitary conditions leading to food contamination, and the carriage of infection of excretal origin by flies or in other ways. One was led tentatively to this view in Gallipoli, when, during the autumn of 1915, the general epidemic of sickness due to dysentery, enterica, and diarrhœa became complicated by a considerable and widespread prevalence of jaundice. In previous campaigns, epidemic jaundice has been associated with insanitary conditions of camps, while some observations made on the French forces at Cape Helles by Sarraillhé and others pointed to the conclusion that the jaundice there prevalent was a manifestation of infection by paratyphoid A or a similar organism of the enterica group. Since then it has been shown, I think, that this association was probably one of coincidence and not cause and effect.

As regards the British forces at Gallipoli at any rate it became clear that the jaundice and diarrhœal group of diseases were not running parallel, the former continuing to increase at a time when the diarrhœal group had undergone a marked decline. Since then I have been naturally on the look out for association of camp jaundice with dysentery and enterica, but have been able to detect little or no association between them.

On all the fronts one met with occasional sporadic jaundice, much as I presume one gets at home, but definite epidemics of jaundice were rare. A small outbreak occurred in Salonica in November, 1915, mainly in men of a division which had come from Gallipoli, and this continued during the cold months of December and January. A short outbreak was also noted in one division in Mesopotamia in course of the very hot weather in June, 1916, affecting some 550 cases, and lasting altogether about six weeks. This epidemic arose shortly after the division had returned from operations in the more forward area and was going through a period of relative rest with more abundant diet. The history was one of general and almost simultaneous occurrence and progress of jaundice throughout the divisional camp, in the same sort of proportion in all its constituent units. It suggested some generally operative cause, but did not point to case to case infection. Some other outbreaks occurred in Egypt, as to which I had little opportunity of inquiry, and I fear I remain as puzzled as before as to the explanation of these epidemics. None of them, so far as I am aware, comprised cases in which the jaundice was symptomatic of Weil's disease or the icterohæmorrhagic spirochætosis which has been described in France; spirochætes were searched for in jaundice cases in Mesopotamia, but without success. Even if in future it was to be shown that camp jaundice is associated with spirochætal infection, many special points of the ætiology of this disease, particularly the local limitations of its epidemics, would apparently still need to be explained.

CHOLERA.

It is remarkable that so little has been seen of cholera in all these great campaigns—for great each one has been by reference to all earlier war standards—campaigns which have brought our forces, including large Indian contingents, as well as a host of native labourers and followers, into contact with the Turk and the Arab in regions which have experienced sweeping epidemics of this disease in the past. In Gallipoli in 1915 cholera was one of the epidemics which was most anticipated, but it did not come. In Macedonia the cholera of the 1912 Balkan campaigns was a recent and disquieting memory. Ever since then cholera was believed to have lingered in the Balkans, at any rate in the neighbourhood of Adrianople, though its type was anomalous and possibly decadent. There was, however, no cholera among the allied troops of the Salonica force when they got into contact with Turks and Bulgars at the end of 1915, nor, so far as my information goes, has there been any since. Egypt for many years in peace time has strained every nerve to keep out cholera, especially by the regulation of the pilgrim traffic, so successfully that it is some fifteen years since that country has had more than an occasional sporadic importation. The pilgrimage was shut down during the war, but would not the enemy on the Eastern and Western fronts, or our own troops in transit from India and other endemic centres, bring us cholera infection, or at least import unsuspected cholera carriers with all their possible consequences? That there was solid ground for these apprehensions, and for the various precautions to which they gave rise, was shown by two small outbreaks in Egypt in 1916, one in a hospital unit to which infection appeared to have been brought by convalescents arriving from Mesopotamia, the other due to infection from Turkish sources incurred in course of operations east of the Suez Canal. These cases, not more than forty altogether, fortunately represented the whole story of cholera in Egypt from the beginning of the war to March of this year, and I believe the recent history has been equally good.

Mesopotamia in the present year has, I believe, had almost an equally satisfactory record of cholera. It was otherwise however in 1916, during which some 1,700 cholera cases (British and Indian) occurred in the Mesopotamia Force. The bulk of these cases occurred in an epidemic between April and June and I have little information about them save that a portion at least was traced to infection of water obtained from a cutting off the Tigris, which remains on our maps labelled as "Cholera Creek," near which, at the time the epidemic arose, two Turkish prisoners suffering from cholera were captured.

In the months which followed, July to November, 1916, there was a continued sporadic incidence of cholera, on a relatively small scale, both at Basra and at various points along the Tigris. There must have been numerous carriers of the disease and the fact that it nowhere became widely and suddenly epidemic is, I believe, mainly to be attributed to the circumstance that water in nearly all cases was taken from the Tigris, and to chlorination. The volume of water in the Tigris was so great that any infection would be largely diluted; moreover for several months its muddy waters are full of suspended mineral matter, 90 per cent. of which subsides on twenty-four hours' standing. As regards chlorination, although some irregularities were inevitable when hundreds of small plants, mainly pits dug in the ground, revetted with sandbags and lined with tarpaulin, had to be separately treated, yet as a whole it provided systematic sterilization on a comprehensive scale, more often efficient than not, and steadily improving with experience and better material. One of the greatest difficulties was due to the variations in the chlorine content of different tins of bleaching powder, and particularly to its rapid loss of chlorine once the tin had been opened. A very useful advance was made when the Central Hygiene Laboratory took to supplying standardized concentrated solutions or suspensions mixed up by hand churns, which kept their strength. Later on, with the development of large water services at the base, various methods of admitting chlorine into the supply in amounts automatically proportionate to the flow became available.

How far the other fronts owed their escape to similar water precautions—as a rule marvellously thorough—how far to absence of infection, and how far to inoculation, is difficult if not impossible to say. While it is true that the existence of cholera among the Turkish forces opposed to us was on several occasions suspected and once or twice proved, I heard of little evidence of any widespread epidemic among them. In this connexion it was interesting to find from statements of prisoners in Mesopotamia, and particularly in Egypt, where a very careful inquiry was made on the point, that they had been inoculated against disease a great many times, and particularly often against cholera.

It is difficult to appraise the evidence which any of the campaigns afford in favour of inoculation against cholera. In Mesopotamia it was certainly the fact that the prevalence of cholera in the early part of 1916 occurred before the systematic inoculation of British and Indian troops with anti-cholera vaccine. The fall in cholera roughly coincided with increase in this inoculation, but at the same time it became possible to introduce vast improvements in other measures of preventing infection. In Gallipoli, Salonica and Egypt, anti-cholera inoculation was general practically from the beginning, going frequently in some units nearly to 100 per cent. standard, and was systematically renewed.

A question brought before our Committee at the outset in 1915, and often since, was whether cholera inoculation should be done straightway or should wait for the appearance or at least substantial threatening of the disease. The evidence of the short duration of protection—like all other evidence as to the protective value of cholera vaccine, mainly dependent on the Greek accounts of 1912—made this an important practical question. The Committee invariably advised systematic inoculation in advance of any known threatened infection, and its renewal from time to time, preferably at least every twelve months, and I believe on a balance of the arguments that this contention has been sound. In the Mesopotamia force a weaker vaccine had been used than elsewhere, and a trial of a stronger vaccine of 12,000 millions in the double dose was to be made.

MALARIA.

The incidence of malaria on the various fronts, their anopheline history, the steps taken to combat the formidable epidemics which have occurred and to prevent those which have not, would require an address by itself, and I can only make a few passing observations here. Personally I missed the great historic epidemic of the war, the malaria which began suddenly in Macedonia in June, 1916, when the British forces, leaving their entrenched line north and east of Salonica, penetrated the Struma Valley and other baneful regions and held them against the Bulgars. Only one month before, when our Committee had left Salonica, the prospects of escape from serious malaria on the ground then occupied were quite good. The country behind the lines was even then a large one, but the question of malaria had received very close attention from the beginning. Areas specially swampy and liable to malaria had been worked out, some sections had been efficiently drained, while quinine prophylaxis for the troops and certain native populations and other measures were well in hand. There were weak points in the defence and a rise of the malaria curve was anticipated, but there was every reason to hope that it would produce no large amount of sickness. As is well known, the advance necessitated by the military situation in June changed all that, especially in the Struma Valley. Here the regular incidence of malaria among the inhabitants of the native villages was even more virulent than usual and causing a notable mortality. It is a broad plain with luxurious vegetation and endless marshy hollows. Mosquitoes swarmed, the chief vectors of malaria being apparently *Anopheles superpictus* and *maculipennis*. A man's daily bites were not to be counted by units but by the score: malaria spread with great rapidity; the weekly evacuations for a time ran into four figures, base hospitals and hospital ships filled, and cases began to pour into Malta and many presently to be sent home. The epidemic on these fronts declined in the autumn, but never wholly subsided. I understand that apart from many relapses new cases occurred in the winter months, some among men who had only recently come out from home. If so, the experience was similar to that met with in Basra and Mesopotamia, where the common notion, that after a certain season is over risks of malaria cease, had to be considerably modified. There was always a residuum of adult, already infected, anophelines ready to emerge on a warm day, especially from tents and bivouacs which were not opened up, and to bite vigorously even in January: the curve of freshly infected cases was flattened but not extinguished.

With only partial hearsay knowledge I do not attempt to specify the nature of the efforts which have been made in Macedonia to combat the malaria danger, especially in the present year, by changes of ground where practicable, alterations of clothing, netting on a scale never before reached, quininization of carriers, and so on. One knows enough to realize that the complete story of the fight against malaria in Macedonia, when it comes to be written, will form an epic full of fine work and no little self-sacrifice, which has at least partly had its reward by the later improvements of the malaria record.

The epidemic malaria with which one met in Mesopotamia in 1916-17 was fortunately on a much smaller relative scale. From Kut to near Kurna—and I imagine the same to be true between Kut and Bagdad—the bare Tigris plain will not as a rule support even the mosquito. In the few areas where water is run off the river for irrigation it is quickly absorbed or evaporated; there are few channels of stagnant water and few or no marsh areas. It is otherwise when you descend to the junction of the Tigris with the old channel of the Euphrates and reach Kurna, where the date palm groves begin to be abundant on each side of the river. I could never ascertain why—save for a venerated but disreputable plum tree—Kurna was credited as the Garden of Eden, but if that garden was there, our first parents needed no other excuse than the mosquito for seeking the quickest available body covering and leaving the district. All the way down past Basra to the mouth are also irrigated palm areas which would permit an abundant mosquito-breeding if it were not for the tidal oscillations in the creeks and channels, which keep it partly in check. In the summer of 1916 it was noteworthy that the malaria met with in all parts of the Mesopotamian area could most often be traced back to an original infection contracted in the Basra to Kurna zone.

The reported new cases between August and October were usually from 2 to 4 per 1,000 (British and Indian alike) each week, while much escaped notification through the unfamiliarity of the medical staff—mostly men who had still to gain their tropical experience—with the clinical results which malaria infection may produce. No doubt in 1916 malaria contributed considerably to the mass of miscellaneous sickness which was recorded as due to the "effects of heat" or even to heat stroke.

In Egypt the Suez Canal region needed and received close expert attention in consequence of the existence of large areas in which *Cellia pharoensis* and other anopheline carriers in Egypt, bred out, particularly certain areas irrigated or swamped by blind outflows from the Sweet-water Canal. Certain of the occupied oases in the south and west were also notorious for malaria. Its greatest prevalence in these regions appears to have occurred in the autumn. Relatively to the forces concerned the extent of malaria has been slight, a fact largely to be attributed to the attention which has been given to anti-malaria measures.

Personally I was throughout in the position of having to learn the rudiments of malaria prophylaxis from those who had made the subject a life's work, and was fortunate indeed in having the tuition of my colleague, Lieutenant-Colonel Andrew Balfour. Like many others I had to learn that anti-mosquito work does not consist in merely sending about a few men with spades and an oil drum; it must be done thoroughly by regular and adequate labour units if it is to be of any use, and supervised by someone who can think on the ground in terms of the mosquito.

Save in a few places, such as the areas near Salonica, or special regions on the Suez Canal, the exigencies of the campaign usually put the efficient destruction of breeding grounds out of practical possibilities. One had to rely on measures for preventing the mosquito bite; for the removal, or at least the thorough cinchonization, of human carriers of the malaria parasite, and on prophylactic doses of quinine for the healthy men. The latter is a most difficult and controversial subject, facts on which I hope will later on be contributed to this Section. One of the best statements of the case for quinine prophylaxis, from Panama experience, had, in 1914, been given by Craig in the U.S. War Department Bulletin, and the importance and value of this prophylaxis had strong support from many other authorities. Our Committee advised its adoption during the malarial season in troops specially exposed to risk, where possible in doses of 6 gr. daily. That there should have been such intense malaria in Macedonia, notwithstanding the use of quinine by the ton for this purpose, has been disappointing, and has led to doubt being expressed on the soundness of the practice, and also to further investigations—like those recently reported by Lieutenant-Colonel James—of the stages at which the parasite becomes quinine resistant. The objections raised are not wholly removed by evidence that the prophylactic administration of quinine has been haphazard, irregular, or in insufficient dosage. There is plenty of such evidence. Sitting at home one might well think that in a body of men under military discipline nothing should be more easy, save in periods of active engagements, than to secure that they take a given tabloid or draught at a given time once a day; one has only to see the problem in the field to realize its great practical difficulties. All the same one cannot get away from the facts of heavy incidence of malaria on troops in which the prophylactic administration was well carried out, no doubt in areas where the dose of infection was large and frequently repeated. One has to balance between the view that the incidence would have been still heavier but for the prophylactic, and the conclusion that the prophylactic use of quinine is but a broken reed to lean upon.

I can only say here that on such evidence as I have been able to study I think that the preventive use of quinine for healthy troops in malarial areas in such doses as those mentioned, was, and is, a right measure to adopt, though I trust that from the large amount of work which is now being done on the subject, both at home and in the military laboratories abroad, we will soon have some decisive and conclusive evidence as to the best method of procedure in this respect.

TRENCH FEVER AND LOUSE-BORNE DISEASES.

When the Medical Advisory Committee were at Salonica in May, 1916, trench fever had recently been discovered. A great number of cases of pyrexia of uncertain duration and accompanied usually by great tenderness or pain in the shins, unbearable at night, had come to notice during the previous four or five months. They had been closely studied, particularly by Major Hurst, R.A.M.C., who was at that time a consulting physician to the Salonica force, and it had been recognized that most of the cases which were being diagnosed as pyrexia of uncertain origin had clinical features in common. A kind of agreement had been arrived at to describe certain types of this pyrexia of uncertain origin as P.U.O. (*a*), (*b*), &c., but the arrival of papers by McNee, Renshaw and Brunt[1] showed that in France these types had been identified, and were being described as varieties of a single epidemic disease, "trench fever." It appeared moreover that the disease, under the name of "gaiter pain fever," was known to the Austrians, among whom it had been epidemic in Galicia, and there could be no doubt that the Salonica force had been suffering from the same disease in its various forms. Here, as in France, all gradations were met with between the short or acute type—a sharp fever of about a week, usually with considerable tibial pains, with at most one or two secondary and less severe relapses, and the long or chronic form in which very regular recurrences of fever were met with over many weeks.

A revision of the entries of "P.U.O." in the books of field ambulances and base hospitals showed that the disease had occurred in December, 1915, in troops of a division coming direct to Salonica from France; that by May all the divisions in the force had been affected to a greater or less extent, the forces at the front relatively considerably more than those in the base areas. Individual attacks were as a rule mild enough, but the total effect of so much temporary invaliding on the efficiency of the force was considerable, and in a minority of individual cases recovery was very protracted. Many battalions and units had only had a few cases altogether, but some had been severely attacked, the epidemic usually spreading rapidly for the first two or three weeks in the affected unit, and afterwards being followed by a dropping fire of cases.

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[1] *Journ. R.A.M.C.*, April, 1916, pp. 490-521.

It was of course a large assumption that all these cases diagnosed as "P.U.O." had been in fact trench fever, but I think it must have been very close to the truth. With the definite recognition of trench fever it became possible to get more exact information and largely to discard the term P.U.O. It is a term which in any case should seldom or never be used in field ambulances which keep their cases only a short time; undiagnosed cases may be sent down as "fever undiagnosed," but the term "pyrexia of uncertain origin," which is accepted as a final diagnosis, is rather one for base hospitals when they have exhausted all their resources of identification. Our hopes at Salonica that closer ætiological study could be made of the disease were frustrated by its rapid diminution after May and by the all-compelling demands of the malaria epidemic. I have not been able to ascertain the extent to which it has been prevalent again in the present year, but certainly one has heard little of it.

The outbreak to which I have referred was from eighteen months to two years ago, and since then much has been done in France to study its ætiology and method of spread. We have learnt that the infection is inoculable and is contained in the corpuscular elements of the blood and have had theories of the disease as a spirochætosis.

The whole matter at the present time stands referred to a special investigating committee in France, whose information is far more recent and comprehensive than anything that was obtainable at Salonica in the early days of its recognition. The louse was there suspected as carrying the infection, and I believe the possibility of transmission by the louse has been demonstrated more or less convincingly by actual experiments. In Salonica it seemed that if any blood-sucking insect was concerned it was in all probability the louse, as in December and January there were few biting flies, midges or mosquitoes. A difficulty, however, was caused by the many attacks of officers and others who had no signs of lousiness. This can of course readily be explained by the assumption that one or two casual lice, to the bites of which anyone may have been exposed without knowing it, are enough to carry the infection. But on such an assumption one would have expected many more multiple cases of trench fever in the same dug-outs or bivouac than in fact was the case. A few pronounced groups of cases were met with, but hardly more than would have occurred as the result of chance from a general cause common to everyone. As a rule, when a unit was attacked, even when severely attacked, it was rare for more than one man to be affected in a group of men in the same bivouac. Individual susceptibility might then be invoked, but it was difficult to make much assumption of a rare susceptibility when the heavy incidence in certain units was considered. It was, moreover, somewhat surprising that this disease, if louse borne, is not known to have accompanied typhus and relapsing fever. One may hope that these and other difficulties in interpreting the spread of trench fever on a louse-carrying hypothesis may be met by further knowledge.

There is, as far as I am aware, no better hypothesis in the field. The suggestion of rat infection, which I believe has been made, would not have been supported in Salonica, where rats were few. There was very little accompanying catarrh, or indication of spread by respiratory infection. As a practical question, on present knowledge, the active employment of measures for delousing, together with the early detection and removal of cases, seem to be the chief measures of prevention.

Having learnt something of trench fever in Salonica, I naturally inquired for evidence of its existence in subsequent visits to Mesopotamia and to Egypt in 1916 and 1917. One or two suspected cases were spoken of, but the disease, if present at all, had certainly shown none of the rapid spread in epidemic form which was characteristic of it in Macedonia, though lice were common enough.

On all these fronts the question of delousing troops as a protection against relapsing fever and typhus was constantly arising. Typhus, happily, has been practically non-existent; relapsing fever occurred among Indian troops in Gallipoli and Egypt and very slightly in Mesopotamia. In Egypt relapsing fever is a common disease of the native population, especially in the cooler months of the year, and in 1916 a formidable epidemic occurred among the Egyptian labour corps, which attained its maximum in April and thereafter rapidly declined. Only some 200 British troops were attacked. Here isolation and rigid measures of disinfection usually sufficed to keep relapsing fever within small compass. It is noteworthy that few of many panaceas against lice which were put out at the beginning of the war have stood the test of time and practical experience. Nearly always one came back to current steam for clothing and blankets, or to cresol or petrol emulsions. For delousing by steam one needs to get away from prepossessions in favour of elaborate disinfecting apparatus or even the ubiquitous "portable Thresh." Current steam can be used much more efficiently, expeditiously and economically in improvised apparatus of a much simpler kind. The railway steam disinfecting vans introduced in the Suez Canal area at the instance of Colonel Hunter are examples in point, enabling the kits of several hundreds of men to be disinfected at one time by turning on steam from the locomotive which takes about the vans. The vans have been of incalculable value in preventing the spread of relapsing fever in Egypt.

One learnt early that effective delousing of a unit is one of the most troublesome duties that a medical officer can undertake: it is not that it is usually difficult in itself, but it needs a thoroughness and devotion to detail, and endless trouble to secure the co-operation of combatant and other officers which many a beginner does not realize, and from which many an old hand recoils. But the work is well worth doing and persisting in, and apart from removing anxiety as to typhus and relapsing fever, and possibly trench fever, it has its reward in freeing men from what to many is a very real source of discomfort and loss of self respect.

Sandfly Fever.

The sandfly was troublesome in Gallipoli in the autumn of 1915; at Cape Helles much more so to the French who inhabited or used old stone buildings in Sed-el-Bahr, than to our own troops living in recently made dug-outs or, in the case of hospitals, under canvas. True sandfly or pappataci fever was conspicuously epidemic in the late summer on the French side. Though our own troops were troubled with sandflies here and in Egypt (I have no facts as to Salonica), I only came thoroughly to realize sandfly fever when I met with it in abundance in Mesopotamia in August and September of the following year. At that time sandfly fever, and "P.U.O.," due very largely to sandfly fever, were keeping up the sickness-rate, and accounting for about one-third of the total sickness from all causes in the British troops. Its period of epidemicity was from July to October, and it often spread with great rapidity through the units affected. The actual duration of the fever is usually only a few days, and it had of itself few after-consequences besides those of muscle fatigue and exhaustion, but the epidemic, nevertheless, had considerable military importance. At a critical time of reconstruction and reorganization men were constantly and most inconveniently being put on the sick list, and the attack coming at the end of the trying hot weather seemed, like all otherwise trivial maladies at that time, to be met by little or no resistance.

There was little to be done in the way of prophylaxis except to try to keep off the ubiquitous sandfly. Mosquito nets, if under eighteen holes to the linear inch, were often little more than traps for the sandfly, which could be found in the morning fully gorged along the top edges and in the corners of the net. On the other hand, a fine mosquito net, twenty-two holes to the inch or more, was really effective at night, though the favourite biting hours of the sandfly were often at the end of the day when nets could not be used and one had to trust to repellents. Of these experience seemed most to favour smearing the exposed surfaces with vermijelli, which was available as an issue for louse destruction. It was more lasting than the volatile oils, and needed renewing only once or twice in an evening.

BILHARZIA.

Bilharzia is a disease with which ordinary workers at preventive medicine in England are little concerned, but one naturally read and inquired about it on reaching Egypt where it is so common. A severe bilharzia infection is a serious and crippling disease, with its accompaniments of hæmaturia and painful micturition due to the constant passage of the ova through the bladder wall or of the results of rectal bilharzia, and it was the more to be apprehended as several months are required after infection for symptoms to develop. The text-books at the time carried one to the stage of realizing that the infection once acquired is very rarely cured; its results could only be palliated, and even with treatment the soldier would often be deprived of his fighting efficiency. They gave a life history of the parent worm in the human body, which inhabits the blood stream and gives off its ova, and added, merely as a conjecture, that outside the body the embryo went through a further cycle, probably in the water snail.

In Egypt in 1915, however, the Bilharzia Mission sent out by the War Office had already succeeded in carrying knowledge a stage further, and had demonstrated that at least two quite distinct freshwater molluscs of the genera Planorbis and Bulinus were the intermediate hosts of the parasite; and further, that the ova, when ingested by these molluscs, go through various stages resulting in the emergence of free swimming cercariæ which subsequently gain entrance to the human body by being swallowed or by piercing the skin. These researches gave the opportunity of a sound system of prophylaxis based on ascertained facts, such as that the degree of risk of a given source of fresh water can be established by search for the implicated molluscs, that the cercariæ, though they can work through a sand filter, can only survive in water for about forty-eight hours after leaving the snail host; that while practically unaffected by bactericidal doses of free chlorine, and by many other dilute disinfectants, cresol in quantities such as 1 oz. to 60 gal. destroys the parasite rapidly. The system promptly established on such facts was deservedly successful, and bilharziosis has not occurred as a serious epidemic among the troops. It consisted essentially in securing adequate delay of the water before drinking in places like the Suez Canal zone, which are dependent on the so-called Sweetwater Canal, or similar long channels from the Nile, and in inhibiting all fresh-water bathing except in special pools where the water was duly treated with cresol. However laudable the motive, no inhibition on bathing or drinking was likely to be a popular measure in a hot and thirsty land, and it speaks well for the organization that it has been so successfully and thoroughly insisted on. The rare cases have been traced back to neglect of the rules with much nicety: in one case it was possible from the description given by a group of soldiers suffering from bilharzia in a base hospital to go straight to the pool which they had frequented months before, there to find an abundance of the snail hosts, some of which contained the parasite. The foresight displayed in regard to this disease, and the results obtained, must be regarded as one of the most gratifying chapters in the preventive medicine of the war.

LEISHMANIA.

The Oriental sore which results from leishmania infection in Mesopotamia often goes by the name of "Bagdad sore" on account of its frequency in that city. As you are aware, the complaint is often very intractable notwithstanding various modern treatment, such as intravenous injections of tartar emetic, and it leaves most disfiguring scars on the face and other exposed surfaces which are usually attacked.

When the outskirts of Kut and the Shatt-el-Hai formed our furthest western limit, it was noticeable that the Arabs, adults and children, who bore the characteristic scars, had usually come from higher up the river, and at one time it was thought that no active leishmania infection went on in Lower Mesopotamia among the Arabs or was to be found among our own troops. This view had, however, to be corrected on the discovery made or confirmed by Lieutenant-Colonel Wenyon of several isolated cases or small groups among Gurkhas or other Indian troops at the end of 1916.

I have no information from Mesopotamia since Bagdad was occupied to gauge the extent to which our forces have since been attacked; one would expect a material increase.

Apart from early treatment, and the separation of known cases, or at least keeping the sores covered, no special preventive measures seem applicable. The sandfly is suspected as a carrier, though it is difficult to imagine that there can be greater prevalence of sandflies in Bagdad than in many places in Lower Mesopotamia. In any case, as I have already said, the prevention of sandfly bites is no easy matter. The forces in Egypt and the Mediterranean have, I believe, been free from leishmania. It is noteworthy that bed-bugs, which have been suspected as carriers, are among the few insect pests which do not seem to thrive in Lower Mesopotamia.

DISEASES DUE TO FOOD DEFICIENCIES.

In the Eastern campaigns there have, of course, often been far greater difficulties in rationing and feeding troops than have occurred in France and Flanders. It is marvellous, not that failure of supplies has sometimes occurred, but that it has occurred so seldom. The good feeding of the troops as a whole must have contributed enormously to protect them from disease and enabled them to resist disease. Earlier in this paper I have spoken of the almost complete absence of typhus and the way in which relapsing fever, though its infection was often introduced, has been kept within very small limits. The good standard of nutrition has possibly had much to do with this in addition to the measures for suppression of the louse. As regards the medical service, it is remarkable on looking back that I do not recollect a single hospital or field ambulance—and I have visited hundreds—at which there was any shortage of the essential foods required.

At Anzac, in September, 1915, the whole force had for a long time suffered, not strictly from any actual shortage of foodstuffs, but from months of monotonous diet represented by the staple corned beef, biscuits and tea. Up to that time the supply of bread had been very scanty; fats, jams, &c., were decidedly rare, though these were more available later. Men in many instances had a distaste for the food they had, sometimes from bad teeth, and often from the absence of anything like pickles or condiments.

There was no doubt that the dietary in many cases had produced bad nutrition and in not a few cases this was associated with pronounced cardiac weakness. The forces at Suvla Bay, which had landed later, were more fortunate in this respect. I could not ascertain on any of the occupied areas of the Gallipoli Peninsula that cases had been evacuated suspected to be suffering from beriberi, but a few cases from the Peninsula which reached hospitals at Mudros were diagnosed and described by Colonel Willcox as due to this disease. Some scurvy appeared among Indian troops in Gallipoli, doubtless due to the same causes as in Mesopotamia, where again evidence of the result of diet deficiency came to notice.

In Mesopotamia the river transport and the hot weather in a measure governed everything, and during the period which followed the surrender of Kut a period supervened in which these obstacles were at their greatest. The British troops as a whole, however, suffered to no very serious extent in health from the restrictions in food and monotony of ration. In the year before, 1915, I understand that such conditions, greatly accentuated, had produced some beriberi among them; in 1916, however, the extent and duration of any anti-beriberi vitamine deficiency was fortunately not sufficiently great, and there were practically no cases of this disease. Had it been more prolonged, a powerful preventive would have been available in the form of compressed yeast tablets or "marmite," which in view of the 1915 reports had been sent out from home. The only cases of suspected beriberi which came to the notice of the Medical Advisory Committee was a group of some sixty cases of little severity at a camp of British troops near to Basra in November, 1916, the identity of which with real beriberi was open to some doubt, both clinically and epidemiologically. In this outbreak new drafts were affected in some instances within a very few weeks of their arrival in Mesopotamia, the diet at the camp having been liberal and varied, and in some of the cases having frequently included eggs.

The Indian troops in Mesopotamia, however much exposed to deficiencies or monotony in diet, wholly escaped beriberi, the explanation probably being that their stock cereal, atta, contains a good proportion of germ, and also that some anti-beriberi vitamine is contained in the split leguminous dried peas or beans issued to them as dhal. In regard to scurvy, however, it is very different. Very few scurvy cases occurred among the British troops. Their rations, though as deficient as the Indian in fresh vegetables, included much more fresh meat. Moreover, there was no question of general diminished nutrition ; they used canned and preserved foods freely, and in this way were better off as regards nourishing foods than the Indian troops. The latter were dependent on cooking every meal, and this in turn depended on a sufficiency of fuel in a land where every stick had to be brought up, possibly a hundred miles or more from the base. They included men who greatly objected to eat meat if available, either because of caste, or because of pyorrhœa which made meat eating painful. From such causes Indian troops suffered severely from scurvy—not, it should be noted, throughout Mesopotamia, as might be inferred from the observations on scurvy made in the Mesopotamia Commission's report, but in those areas of the Tigris where the difficulty of sending up accessory food supplies, meat, and fuel, was greatest.

At the base, and also on the Euphrates front, where fresh vegetables were obtainable, there was practically no scurvy save in one or two units, for which a special explanation was forthcoming. On the Tigris front it was interesting to find that of three separate and comparable Indian forces the smallest incidence of scurvy relative to strength between August and October was on the force the shortest distance from supplies, and the greatest on the force most difficult of access, while the middle figure for scurvy was that on the force midway. The epidemic on this front was of considerable magnitude, some 7,000 cases being reported, and it was serious in its results. During four months, July to October, the evacuation figures from scurvy in the Indian troops at the front was occasionally as high as 4 per cent. of the force in a week, and it was seldom below 1 per cent. The epidemic ceased only with the onset of cooler weather, the improvement of transport, and increase of supplies. Fresh vegetables then became to some extent available, and efforts were successfully made to secure greater supplies of fresh meat for the Indian troops. By the end of the year also much better issues of fresh vegetables were made right up to the front line. The advance to Bagdad happily brought the troops, during the last hot season, within reach of a large cultivated region, and for this and other causes there has been no repetition this year of the 1916 experience.

The Indian scurvy epidemic of 1916 will no doubt form a landmark in the history of scurvy, and has already given a new and important impetus to its experimental study—for example, the use of the antiscorbutic properties of dried legumes, which have been allowed to germinate before cooking. This method, fully worked out by Miss Chick, is of really great importance and it is possible that before the end of the war occasions may arise in which its adoption will be invaluable. Did time permit one would be glad to enlarge on some of the main features of the outbreak. It seemed fairly clear that during the scurvy-producing period those who escaped were getting their chief antiscorbutic from the fresh meat which was available, and from the occasional onions, which were about the only vegetable to be had. Various fascinating figures were collected as to the incidence of the disease on men who were meat eaters and those who were not, but they were greatly disturbed by the proportion of meat eaters who had not in fact taken or received meat. The difficulties of ascertaining what in fact had been consumed by the Indian soldier were often insuperable even to those who had worked with Indian forces all their lives, and it was unsafe to attempt to draw conclusions from the official ration scales.

Contrary to views often expressed, I could seldom get much evidence of troops arriving in the country who were already "on the scurvy line" as the phrase went. In two representative series of cases, about 250 altogether, the men who had reported sick in consequence of scurvy within three months of reaching Mesopotamia were under 8 per cent.

Another noteworthy point of the epidemic was the great difficulty in effecting any rapid cure of scurvy once definite symptoms had declared themselves. It was this that made the outbreak so serious from a military point of view, not only in cases which had got to the stage of hæmorrhages, but those at earlier stages when there was little but anæmia and spongy gums to notice. Some men of course were glad to have a disease which meant that they would be sent out of a detestable country. But others who were keen to be cured and to stay in Mesopotamia, native officers, old soldiers, kohars at ambulances and so forth, who willingly took special curative antiscorbutic diet and conformed to treatment, often failed in the end.

Pellagra has, if one may trust recent American work on its dependence on protein starvation, now again to be classified with the deficiency diseases. The only one outbreak which I heard of in the Eastern campaigns was not among the troops, but among Armenian refugees in Egypt in 1917.

OTHER EPIDEMICS, AND CONCLUSIONS.

If I have dealt mainly with epidemic sickness special to the East, it must not be supposed that the forces on the different fronts escaped the epidemic diseases common at home. It is true that scarlet fever, measles, and whooping-cough, if they occurred, certainly did not come prominently to notice, but diphtheria on the other hand was often rather extensively prevalent amongst British troops both in Egypt and Mesopotamia. Mumps was a source of much invaliding and sometimes spread with an intensity one seldom sees at home outside schools and institutions. Ghurka units in Mesopotamia were very severely hit by mumps, as also some Australian units in Egypt and native labour corps. Small-pox, as might be expected from the system of vaccination, seldom got any footing when introduced; its occasional prevalence gave reason for some careful consideration of methods for preventing the loss of strength of vaccine lymph when kept and sent about in hot climates. Cerebrospinal fever, though it did not spread widely, caused some anxiety in Egypt in 1915, especially among New Zealand and Australian troops, and in Basra at the end of 1916.

If I were to stretch somewhat the ordinary use of the term epidemic, I might add a great deal as to the waves of sickness which were occasioned by the effects of heat in the summers of Mesopotamia, or by cold as felt in the Balkans in the winter of 1915-16. I have, however, already given too long a list for this address. It will not, I trust, be thought that by referring to the epidemics which have occurred I have given them any disproportionate prominence and omitted to call your attention sufficiently to those which might have occurred but have been prevented.

The debt of the Army to preventive medical work has already been eloquently dealt with by lecturers and speakers at general meetings of this Society, and it is not necessary for me to describe the mass of devoted individual labour which has been put into the prevention of these diseases by those on the spot aided by those at home. These have been men in the field, in the hospital, in the laboratory, and at headquarters; not only the specialists, but men who have the knowledge and administrative capacity to balance up and apply the known facts as to the natural history of diseases to secure their most effective prevention in the circumstances of the moment: an applied epidemiology which is really one of the highest and most difficult branches of the science with which our Section is concerned.

All sciences have been called in to play their part in the War: those who have pursued epidemiology in the Eastern campaigns, like their colleagues in France and at home, have cause to rejoice in the evidence that their own science is not among the least of those which have aided the fighting forces and our country's cause.

Section of Epidemiology and State Medicine.

President—Dr. G. S. Buchanan.

(*November* 30, 1917.)

A Pandemic of Typhus in Serbia in 1914 and 1915.

By V. Soubbotitch.

(Surgeon-Colonel, Serbian Army.)

Typhus, like relapsing fever, was formerly entirely unknown in Serbia. It was during the winter 1912-13 that detachments of the Serbian Army crossed Albania as far as the sea, reaching Durazzo; others came by way of St. Jean of Medua and Alessio to take part in the fighting around Scutari. This expedition through the arid mountains of Albania, which no army had crossed up to that time, in a wild country entirely devoid of resources, was extremely difficult. The Serbian soldiers, who are pretty hardened as regards privations and fatigues of every description, have always said that the Albanian expedition was one of the heaviest tasks they had to carry out. It took place in the depth of winter, which in those parts is very severe, and there was no habitation to serve as a shelter for the soldiers, who were often forced by the cold and wind to take refuge in miserable huts and caverns amid swarms of Albanian lice.

The pathology of Albania was at that time entirely unknown. I will add that here we made acquaintance, not only with typhus and relapsing fever, but also with Malta fever, though of this but a few rare cases occurred.

It was among the soldiers of this expedition that the first cases appeared, and they were much more numerous than in the other units.

Fatal cases were not very long in making their appearance. At first they were put down to fatigue, exhaustion and privations, but it was soon perceived that it was a question of a disease with well defined characteristics, and that we were confronted with an enemy which had till then been unknown in our country. It was relapsing fever and typhus, two diseases endemic in Albania.

The inroad made by these fevers, however, was not excessive; the number of doctors who had been able to gain acquaintance with them had been comparatively small. When the summer came the diseases disappeared. In 1913, in our war against the Bulgars, moreover, the Bulgarian Army had brought us cholera, and we were overhasty in ceasing to be anxious about typhus, which indeed had not yet put forth all its terrible power.

The summer of 1914 marked the outbreak of war with Austria-Hungary. We had to repel a first invasion in the summer, then a second in November, 1914. As the enemy at the time of the first invasion had manifested great cruelty towards the civilian population, at the time of the second invasion a great exodus of the population took place from the invaded to the uninvaded regions. Between 250,000 and 300,000 people thus left their homes in flight before the enemy.

Typhus was then raging in the Austro-Hungarian Army, which at that time invaded our country. After the Austrians' retreat, our armies reoccupied the invaded countries and found themselves confronted above all at Valjevo, which had been the Austrian headquarters, with deplorable sanitary conditions, and at the same time with a large number of enemy sick and wounded. The number of Austro-Hungarian prisoners was likewise considerable, amounting to nearly 60,000 men. The movement of troops, the civilian population, refugees, war prisoners, and wounded, was very great. The roads were congested, the railways overloaded, and the means of transport for all the wounded and sick were, therefore, far from being sufficient. Buildings to accommodate all these people were wanting, and the task of sheltering them all was beyond our power. It must be added that at that time rain only stopped to give place to snow.

At the same time the recruits from the borders of Albania arrived. It was then (in December, 1914), that there began to appear and to multiply in the army, among the civilian population, in the hospitals, and above all among the prisoners of war, cases of illness which puzzled the doctors, who were overwhelmed with work owing to the number of sick and wounded. Some of them were called upon to attend to 500 to 600 patients and more. They spoke of a very grave form of influenza which took quite an unusual course, of a new disease—something unknown—while the number of sick increased very rapidly. Some doctors, having found the spirilla, asserted that it was relapsing fever. Others said that it was typhus. They were both right. There were two original foci of infection, the principal one in the north, at Valjevo, a region which had been occupied by the Austro-Hungarians, and the other, less important, in the southern departments, to which infection had come from Albania. These foci soon become merged in one another, and in the month of January, 1915, the whole of the Serbian country was flooded with typhus and relapsing fever. This epidemic raged during the months of January, February and March in full force. In April it began to die down, and it ceased in the month of June. In the army and among the war prisoners there were about 150,000 sick with the disease, of whom about 30,000 died. Doctors in particular were the sufferers. Out of 350 Serbian doctors 126 succumbed to typhus, the death-rate among them being as high as 36 per cent.

Concurrently with typhus relapsing fever pursued its course, though with a much lower death-rate. In some cases in which both fevers seemed to run their course at the same time the morbidity was very high, and the death-rate ranged in the case of typhus from 20 to 80 per cent. Those who succumbed were chiefly old men, debilitated subjects, sufferers from cardiac or renal diseases, and prisoners of war. Children were particularly immune.

As regards the propagation of the disease, I repeat there were not sufficient hospitals for isolating and even for suitably sheltering the sufferers, who were short of space, air, and every comfort. There were not enough doctors and nurses, and day by day their number declined. In a military hospital where some of my surgical duties were carried out—a hospital which in peace time had beds for only 400 patients—there were at one time 1,600, more than 1,000 of them suffering from typhus. Out of a staff of forty-two doctors, dispensers, auxiliary doctors, dressers and nurses there remained only three free from infection. There was no room and no air, and the saying of our forefathers that typhus is the disease of destitution and poverty was fully justified.

Inspired by the work of M. Nicolle, we began by combating the lice which indeed swarmed ; but here difficulties were encountered. In the first place the lice swarmed to such an extent that they were met with almost everywhere. Then again we often met men who were unable to understand the important part played by these parasites in the propagation of typhus.

The difficulties in the way of ridding the army of the lice pest during the winter were very great in view of the number of the parasites and the very few means available for attacking them. We therefore set about enlightening the population as to the danger of lice, above all in times of epidemic prevalence of typhus.

Care was taken to adopt precautions on the railways; all upholstered carriages and cushioned seats were done away with and replaced by perfectly plain carriages with wooden seats, which could be easily washed and disinfected after each journey. The number of passenger trains and travelling permits was reduced. Trains were formed of cars designed as steam disinfecting stoves, shower bath and bath cars. These trains were run along the railway, and the troops were taken to the stations, where the soldiers were able to take baths while their clothing was disinfected. These measures were attended with obvious success.

In the towns examination and cleansing of hackney carriages was ordered. Hotels and cafés were subjected to a more rigorous sanitary inspection. The opening of public places all day was prohibited: they were to remain closed two to three hours a day in order to be cleansed. Above all instructions were given that the floors were to be coated daily with petroleum.

In order to rid the army of lice, makeshift installations had to be resorted to, as modern appliances were not available. Hot-air ovens were arranged on quite a primitive system by digging holes on slopes. In these ovens fires were lit to heat the walls, and the opening was then closed by means of a double bottom wooden cover. To make sure that the heat inside was not too great, in default of a thermometer a piece of white paper was taken and inserted in the cavity, and was left there for some time. If it became yellow (burnt) the degree of heat was too high and it was necessary to wait a little. Finally, experience having been rapidly gained, we succeeded in producing the degree of heat necessary to kill the lice and the nits without injuring the clothes, which were placed in the oven resting lightly on a wooden grating. It need hardly be said that the hot coal was first taken out of the oven and placed in front of the furnace to catch the lice which might fall from the clothes. The persons working at these furnaces were warned to guard themselves carefully against the lice, because the lice, though remaining motionless during the cold, began to be lively as soon as they approached any source of heat.

The difficulty was to wash the undressed men and safeguard them against the cold whilst their clothing was in the ovens, for these men had no spare clothes and it was mid-winter, often far from any shelter. Whenever, however, there was an energetic man in command imbued with the importance of his task, he succeeded in utilizing all available means to rid his men of the parasite. Mostly it was done by multiplying the number of ovens; there was sometimes one or there were even two for each company.

In the towns, in the ambulances and the hospitals, different rough installations were made for the destruction of lice and nits by hot air, and at the same time for heating the water, because most of the steam disinfectors were more or less damaged, and there were no competent workmen to repair them. We were short of linen, of well-fitted wash-houses, and of plant for drying the linen.

Later on, simple rough appliances were also built by Colonel Hunter, by which steam disinfection could be done. On a round metal water tank, heated below, a barrel was placed with its bottom removed, of the same size and shape as the tank. At the bottom of this barrel was a grating, and in the cover, which was well fitted, supports were fixed to hook on the clothing. The cover was kept down with a few stones. To prevent the steam escaping, between the tank and the barrel a large circular hose made of strong canvas was placed, filled with coarse sand, and on this ring the barrel rested. This simple method allows of the whole of the steam being directed from the tank into the interior of the cask, where it reaches a degree of heat and pressure fully sufficient to kill both lice and nits in a short time. When the pressure becomes too great the steam lifts the cover like a safety valve. The articles on being taken out of the apparatus, however, are slightly wet, and those of leather are often damaged.

It seems to me that for destroying the lice (not for disinfection of soldiers' clothing) circulating hot air appliances, of which there are several designs, are preferable, particularly when they are so constructed that the temperature inside can be graduated and measured; at the same time they may serve for drying the linen.

For sick wards sulphur fumigation has been used with generally satisfactory results. As a preventive against insects recourse was had with some success to camphorated naphthalene powder, camphorated oil, and spraying petroleum or formol on the clothing.

To kill the lice the spraying of formol and benzine acts very quickly; insecticidal powder is insufficient, it stupefies them but they recover. Infusion of tobacco leaves also fails to kill them. We used all these means to combat typhus. Colleagues and missions, particularly from England, France, Russia, and the United States, soon came to our aid, and owing to their excellent training, and above all their devotion, greatly contributed to combating and suppressing typhus. Unfortunately they too were not spared by the disease, many of them were attacked, and some died.

A patient suffering from typhus is said not to constitute any danger to his environment when there are no lice present. This opinion, put forward by persons who know the disease well, seems to me correct, especially when the patient is under good hygienic conditions, and I was able to verify it myself in a fairly large number of cases. Such a patient is not in the least dangerous to his doctor and his nurse. This applies to well-equipped hospitals, especially those not overcrowded. But a doctor who has to work amid an environment overcrowded with typhus patients where there are lice is particularly exposed to infection; he will do well not to neglect any precaution in his defence.

Having worked for several months in a hospital which was of necessity under entirely unfavourable conditions, and where almost all the doctors and nurses had been attacked by typhus, I took special interest in this question of individual prophylaxis. It need hardly be said that baths and changes of linen and clothing are of importance. For visiting patients with whom contact is unavoidable, it is desirable to make use of a suitable dress. A blouse or overall, fitting tightly at the neck and wrists, where a band of cotton wool sprinkled with camphorated naphthalene powder and sprayed with formol or petroleum can be placed, will be useful. With regard to the colour of the overall, I think that black should be preferred; in the first place the lice are more readily detected on a black background. On the other hand, I have also tried the effect on the lice of pieces of material of different colours; they generally preferred to seek shelter in bluish grey material (the colour of the Austrian uniform) and khaki, while they seemed to me to avoid black. In order not to pick up lice the overall must not be too long. It is useful to wear top boots when on this kind of duty. Some of our doctors also wore hoods and masks attached to the overall as used in certain plague hospitals. It is a good thing to wear rubber gloves. Though personally I did not contract typhus, and was never bitten by a louse, I know of cases where very competent colleagues who had neglected nothing, and who had asserted that they had never been bitten, fell ill with typhus. One who was thoroughly well acquainted with this disease, an excellent bacteriologist and a great partisan of Nicolle's views, who was the chief physician of a typhus section, and asserted he had never been bitten by a louse, was also attacked by typhus and died.

A number of our doctors are of opinion that the biting louse is the chief transmitting agent of typhus, but that it is not the only one. I am not aware that it is possible to be bitten by a louse without perceiving it. Generally people say that they feel it quite distinctly, and I have even known colleagues who when this happened to them, immediately injected $2\frac{1}{2}$ per cent. carbolized water at the point of the bite, hoping thus to neutralize the virus. Or perhaps it may be through inhaling the detritus of the excrements or particles of the body of the lice, or by their introduction under the skin by friction, that the infection may also arise.

I was surprised by the large number of cases of typhus with surgical complications. I saw 130 cases of parotitis, which in the majority of cases was severe, suppurative, and even gangrenous; I have seen patients die from it. Possibly the want of care of the mouth may play some part in its causation. In 124 cases I saw gangrene of the toes, the foot and the leg. In one case only was there gangrene of the penis; sometimes there was partial gangrene of the external ear. In one instance only had I to treat secondary hæmorrhage in consequence of a traumatic aneurysm of the femoral artery in a patient on the eighth day, and who fortunately recovered.

DISCUSSION.

The PRESIDENT: Colonel Soubbotitch's account of the Serbian typhus has great epidemiological interest as well as a human aspect which will evoke sympathy and admiration from a British audience whenever the story is told. The simultaneous outbreak of typhus and relapsing fever in Serbia in 1914-15, formed one of the epidemics—perhaps the one epidemic—which most directly and profoundly influenced the course of the war. It was so sudden, and so overwhelming, that it might well be asked whether what was done, or even whether anything which conceivably could have been done at the time, would have very materially lessened the total of disease in those fatal winter months.

The bravest efforts were made to treat cases in hospital by the Serbian medical service and by the hospital units of the Allies. Magnificent work was done by the Scottish Women's hospitals—work recalled by the recent death of Dr. Elsie Inglis—by the unit taken out by Mr. and Mrs. J. Berry, and many others. But it seems doubtful whether the totality of all this work in regard to separation of the sick was able to a very material degree to lessen the mass of infection. Practically a whole nation was stricken, and a whole nation cannot be isolated in hospitals. In addition, certain general administrative measures were adopted about the time when the epidemics were at their height, and I believe that certain of them had a perceptible influence in staying the epidemic, particularly those taken at the instance of Colonel Hunter and the British Mission. They included not only the systematizing of the measures for delousing, but also the stoppage of the railway traffic and the cutting off of communication between the towns and villages. It would be instructive to know more of the facts available on this matter. Another question worth discussion is the nature of the influence exerted by malnutrition and deficient food. Did this operate directly by increasing the opportunity of men getting bitten by infected lice, as a result of the crowding in quarters and other conditions attending famine, or indirectly by making men susceptible to doses of infection which would have had no effect if their nutrition had been good? The association of relapsing fever with famine is so close that the latter explanation suggests itself for that disease, but it does not follow that the effect of malnutrition on typhus is exercised in the same way.

Mr. JAMES BERRY: The following is an account of the methods of dealing with typhus that were adopted by the surgical unit that Dr. Berry and myself raised and took to Serbia at the beginning of the typhus epidemic in January, 1915. I would lay stress on the necessity for shaving hairs from all parts of the body and of thoroughly cleaning the patients before admitting them to hospital. For disinfection of clothing the simple method of boiling in large tanks, constructed by the local tinsmith, was found best under the circumstances. Although typhus broke out again and again in the hospital it never occurred in any patient who had been more than a fortnight in hospital, and was never transmitted in my hospitals from one patient to another, because the patients had been completely freed from lice.[1] With regard to my more recent experiences on the Russo-Rumanian front, the Russian "Kipiatok," or hot water boiler, seen at so many railway stations in that country, and the "disinsector" so largely used in the Russian Army proved most serviceable in my hospital at Tecuci. This disinsector consisted of a simple oblong pit some 9 or 10 ft. deep, roofed with rough timber and about 2 ft. of earth. Two small brick furnaces and a few metres of stove-piping enable a temperature of 140 C. to be obtained. The clothes to be disinfected were laid on wooden staging. This method, which I used for months, was most efficacious in the destruction of lice and nits, although doubtless inferior to steam disinfection. It has the great merit of being quickly and cheaply constructed from local materials easily obtainable. The only special instrument required is a thermometer to be fixed inside the chamber, above the door, the readings being taken through a pane of glass.

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[1] "A Red Cross Unit in Serbia," (J. and A. Churchill), 1916.

Dr. MAY DICKINSON BERRY: The very high mortality, both in the Serbian epidemic and still more so in the recent epidemic in Rumania, is no doubt largely due to the fact that a large number of those attacked were in a debilitated condition from semi-starvation. In Rumania last winter there was great want of food among the whole population, and there was almost complete absence of milk, fresh or preserved, as well as of any other articles suitable for feeding fever cases. There is little or no evidence that typhus is transmitted otherwise than by lice. In the hospital in Serbia where I worked, cases of typhus were not removed from the general wards till the diagnosis was clear—i.e., till the rash had appeared. They were nursed in a ward in the same corridor without any special precautions except against lice. No case of infection could be traced to another case in the hospital; all those which broke out were within the period of infection from the date of admission.

Section of Epidemiology and State Medicine.

President—Dr. G. S. BUCHANAN.

(*January* 11, 1918.)

On some Special Problems of the Italian Medical War Services.

By Major Cav. FILIPPO DE FILIPPI, Hon. K.C.I.E.

BEFORE dealing with some of the sanitary problems of the War in Italy, I would like to explain briefly the principles which govern the alimentary rationing of soldiers and sailors in Italy to-day, and which, so far as I know, we have been the first to put into practice. I need hardly emphasize the importance of the problem both from the point of view of economics and that of the health and efficiency of the troops. The ration adopted in all the European armies has been based upon the conclusions of the German school of metabolism—from Voigt, Pettenkofer, &c., to Rubner. This school reigned undisputed until about ten years ago. The Germans had fixed the amount of proteid, fat, and carbohydrates to cover the amount of calories needed in the various conditions of life, and their figures were undisputed. According to Rubner a normal man doing moderate work needs 2,631 calories, which must consist of the following amount of food stuffs: 123 grm. of nitrogenous substances, 47 grm. of fat, and 337 grm. of carbohydrates.

In strenuous work the calories required increase to 3,653 and the quantities of nitrogenous, fat and carbohydrate matter to 165, 70 and 556 grm. respectively. I will not dwell on the objections and the criticisms to which the researches of the German school have given rise; it will be sufficient to remind you that Chittenden's comprehensive researches have established beyond any doubt that the German school had given an exaggerated importance to proteids as compared with carbohydrates. Chittenden proved that students, athletes and soldiers in camp can reduce their albumen rations to 50 to 60 grm. a day, corresponding to 8·5 to 9·5 grm. of nitrogen, without the slightest disturbance of their metabolism or general health, and even obtain an increase of body-weight. This is less than one-third of the German nitrogen ration for strenuous work. Chittenden's researches have been repeated with approximately the same results by many physiologists. The average amount of albumen accepted as necessary by modern biologists is 1 grm. per kilo of weight. The whole surplus ingested is pure waste, and represents an excess of work for the intestines, liver and kidneys. A young man, aged under 25, in course of development, as the soldier generally is, would require a small increase in the above amount. The fundamental principle governing the modern theory of alimentation is that the function of albumen is a constructive one: that is to say, it goes to replace the waste of the tissues. The normal heat-producing foods supplying the necessary energy for muscular work are fat and carbohydrates. The amount of carbohydrates which can be assimilated by the organism is almost unlimited, as the excess goes to create the glycogen reserves which are at the disposal of the organism as necessity arises. It follows that the work ration has mainly to be based on a large supply of carbohydrates in the form of starch, which, by the way, is also always and everywhere far more economical than animal proteids.

So far as I know these results have not yet been applied in the armies, all of which have adopted rations containing a vast excess of proteids, in many cases surpassing the numbers given by the German school. While 100 grm. of proteid might be considered amply sufficient, the old war ration in Italy contained 151 grm. of animal protein. The same is the case in France; in Germany the rations reached 181 grm. of albumen; in England the enormous amount of 256 grm.[1] The differences between these quantities are so great that they cannot be accounted for by the different needs created by race and habit. The physiological error is probably attenuated by the fact that the soldier does not eat the whole of his ration.

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[1] These data I have taken from the report which General Rho submitted to the First Inter-allied Medical Conference held in Paris. From the facts which were brought to light in the discussion following the delivery of this paper, it appears that the number given for the proteid amount in the British Army ration is incorrect. It also appears that the quantity of animal protein in the British ration has been considerably reduced since the beginning of the War.

These short notes will suffice to justify the steps which have been taken to institute a more rational feeding of the Italian Army and Navy, founded on modern scientific facts. The question was raised in Italy as far back as 1912, during the Italo-Turkish War, by General Rho, the head of the medical department of the Italian Navy, but no decision was taken at the time. However the present war is in many respects a great teacher. The enormous increase in meat consumption in Italy, following upon the general mobilization, produced at once a rapid diminution of the live-stocks of the country, which acted better than scientific arguments in promoting an examination of the new point of view. A committee of physiologists and military hygienists was brought together to investigate the whole question and inspect the depots, barracks, shops, &c., in order to ascertain the conditions under which the ration was consumed.[1] Their conclusions were so clear and striking that they were at once accepted, and the amount of animal protein in the ration was brought down to a judicious limit, allowing for a wide margin beyond the theoretical data. The present ration corresponds to 3,300 calories per day; with the exception of the troops operating in the mountains during the winter months, for whom an increase has been provided.

With the diminution of the meat, substitutes such as cheese were also introduced in small quantities, and the ration was rendered more varied and palatable. The change was adopted in April, 1916, and the reports from the Front and from the Navy have been without exception entirely favourable. I may add that many military doctors have made the former excess of meat in the diet of the soldier responsible for various dyscrasic diseases, such as eczema and scurvy, which have of late become much more rare.

The saving effected in the slaughter of cattle by the new diet amounts to over one million head per year.

Recently, owing to the general scarcity of food in the country, an experiment was made to reduce still further the ration of the Italian soldier. We tried to substitute for the meat an equivalent amount of nitrogen in the form of cheese three days a week. But what was more important still, we reduced the bread amount in the ration. As was to be expected, the change very soon proved detrimental to the strength of the men, and we came back to the full ration. The present ration is composed of 700 grm. of bread; 350 grm. of fresh meat, which two days a week is replaced by either 200 grm. of dried cod or 266 grm. of sausage; 150 grm. of macaroni or rice; 50 grm. of cheese; 150 grm. of potatoes or 80 grm. of dried vegetables; 200 grm. of fresh vegetables; 20 grm. of roasted coffee; 30 grm. of sugar; and about half a pint of wine. To this is added three times a week a portion of oranges or dried fruit. This ration contains about 80 grm. of animal proteid, and yields 3,300 calories.

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[1] At the Paris Conference mentioned above reports were submitted by Professor Colonel Baglioni for the army, and by Colonel Belli for the navy.

With this ration, our army alone needs to-day the same amount of meat that was sufficient for the whole nation before the War, and the total amount consumed by the army is three times as large as the amount of meat consumed in Paris, with its 2,800,000 inhabitants.

I do not propose to deal with the general organization of our medical services in Italy, which is practically the same as that of all the other armies in the field. My object will rather be to treat of some special problems which we have had to face at our Front. Some of them have been peculiar to the Italian Army; while some are shared in a larger or smaller degree with the other armies. I will first give you a few statistical data which may be compared with the statistics of the British Army, which has been fighting under entirely different conditions. These data refer to the first year of the War on the Italian Front.

The excellent sanitary conditions of the Italian troops at the Front are proved by the low sick-rate. This in the first months was 2·7 per 1,000; and it has later improved to 1·59 per 1,000. This is a considerable improvement on the 4 per 1,000 which was averaged in recent wars.

The proportion of wounded in the total number of the fighting troops has been from 5 to 7 per cent., which is considerably lower than the average of from 10 to 15 per cent. in recent wars, and greatly below the 22·5 per cent. which Fischer anticipated owing to the perfection of armaments. This low percentage is undoubtedly due to the methods of the present war and the relative scarcity of fights in the open field. But the prevision as to the severity of the wounds was far below the reality. The wounds from artillery fire (including hand grenades) have gradually increased from 47 to 55 per cent. of the total of wounds, leaving only 39 per cent. for rifle bullets. And even this high percentage has been considerably exceeded in many of the single actions. Bayonet wounds average only ½ per cent. In the Russo-Japanese war artillery wounds were only 15 per cent., as against 80 per cent. rifle wounds and 5 per cent. bayonet wounds. The victims of asphyxiating gas in the second half of the first year of the War in Italy had reached a percentage of 5·6 of the total losses. If we now come to the gravity of the wounds we find in the first six months of the War 31·6 per cent. of serious wounds and 55·4 of slight wounds; in the second half of the year there is a notable improvement; serious wounds decrease to 23·6 per cent. while the slight wounds rise to 63 per cent. The remaining 13 per cent. represent the very slight wounds needing no hospital treatment. Among the high percentage of serious wounds must be reckoned a large proportion of wounds of the head, chest, abdomen and pelvis, which represent nearly 40 per cent. of the total wounds. Finally, I will give the numbers of deaths in relation to wounded. In the first six months we reckoned them to be: 5·47 killed on the spot, and 11·2 early deaths in the field hospitals—making in all 16·7 deaths per 100 wounded. In the second half of the year we had 7·9 killed and 10·1 early deaths, making in all 18 deaths per 100 wounded. This percentage is much lower than in recent wars, in which it reached 25 killed per 100 wounded.[1]

So far I have been dealing with circumstances which are common to all the Allied Fronts. But in Italy we have had to fight on a Front of 470 miles in length, offering from the Alps to the sea the most varied climatic conditions, and above all every possibility of infection through contact with an enemy infested with all the epidemics of the Balkans. Besides this we had to deal with all the sanitary problems involved in the occupation of the enemy's territory where all local organization had ceased. We were well aware that the Austrian units on the Eastern or Isonzo Front were infected with cholera; therefore the outbreak of the epidemic among our own troops in the second month of the War was not unexpected. The first cases of cholera among Italian troops occurred in units occupying the trenches just captured from the enemy. The outbreak of this epidemic in a severe form was a serious complication, and added to the immense work of the early months of the War, in which every detail of the military organization was put to the severest strain. We were confronted by a triple problem: (1) We had to localize the outbreak and prevent its diffusion among the whole army; (2) we had to provide for the isolation and cure of the patients; (3) we had to prevent the epidemic from spreading into Italy. The problem was faced and radically solved by the combined efforts of the civil and military sanitary organization, whose united efforts won what was perhaps one of the greatest battles of the whole war.

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[1] D. Balutra, *Giornale di Medicina Militare*, 1917, p. 206.

The epidemic broke out among the first line troops early in August, 1915. Before the infected troops could be recalled it was obviously necessary to clean out and disinfect the first line trenches which otherwise would have become a permanent source of contagion to any unit sent to replace them. For this undertaking the work of the newly-created Prophylactic Sections proved invaluable. These were composed of specially trained men under the orders of a medical officer, who systematically and thoroughly disinfected every new trench occupied by the troops, even before it had been organized for defence or had become relatively protected from enemy shells. Necessarily the disinfection had generally to be carried on under the enemy's fire. I cannot speak too highly of the courage and the coolness displayed by these units. The men who were found to be infected were at once removed and samples of the fæces collected from every man in the inspected area and sent for examination to the advanced bacteriological field laboratories, so as immediately to isolate the carriers. And always and everywhere we have noticed that the decrease of the epidemic follows at once upon the detection and isolation of the bacilli carriers. In regard to the technique of trench disinfection, it varies according to circumstances. In trenches dug in the earth which would at once have absorbed liquid disinfectants, lime powder is generally sprinkled, darkened of course with dust or earth, to avoid making the trench obvious to the enemy. In the trenches of a more permanent construction, lined with cement, the walls and floor are sprayed with carbolic or soda solutions or lime-water, or even with acid sublimate in 3 to 5 parts per 1,000. In regard to the parapets of stones, so frequent on the rocky ground over which we had to fight, the best measure is free irrigation with carbolic or creoline solutions.

The isolation and cure of the patients, the isolation and bacteriological examination of suspected cases, the quarantine and examination of every man belonging to an army unit in which cases of cholera have occurred, and the establishment of a protective zone to prevent the spread of the epidemic into the country at large are all parts of the prophylactic organization which has been strictly applied to all the troops operating at the Eastern and North-Eastern Front, to the workmen and also to the civil population of the districts where these troops are quartered.

The men sent back from an infected area fall naturally under three categories: those actually suffering from cholera, those sick from other causes or wounded, and those in a normal condition. The first are sent straight to the advanced isolation hospitals, whence after being clinically cured and recognized as free of bacteria, they pass into convalescent hospitals. Those suffering from non-contagious diseases have to spend six days in special quarantine hospitals, while their freedom from the germ is ascertained. Finally the men in normal health returning from the Front are obliged to pass through an observation station where they are bacteriologically examined and where their clothes and every object they carry are sterilized, while the men themselves are cleaned and freed from parasites. In regard to the kinds of disinfectants used, they vary according to the objects to be sterilized—whether of cloth, leather or metal. The disinfectants are either chemical, such as carbolic acid, caustic soda or lime, calcium hypochlorite, potassium soap, formaldehyde, sulphur dioxide, or physical—namely, dry heat and steam. If the men are found to be germ carriers they are sent to hospitals where they remain under observation until pronounced germ free. The annexed diagram (fig. 1) gives a general idea of the distribution of the men back from infected areas of the Front. It shows the advanced zone; the zone to the rear of the armies; and the territorial zone. The men actually suffering from contagious disease are sent to the advanced medical stations [left-hand corner of diagram], whence they are dispatched to the isolation hospital; the men not recognized as actually infected are sent to the observation posts [centre of diagram], which are furnished with disinfecting apparatus, baths, laundry and bacteriological laboratory; whence those found to be negative are sent straight back to the territorial zone. The ordinary sick and wounded are sent to the advanced general medical station [right-hand corner of diagram]; those found to suffer from contagious disease being sent to the isolation hospital; and the remainder being forwarded to the observation hospital in the rear zone. Here the carriers are separated from those free from infection, the latter being sent to the territorial hospitals.

We have also special isolation establishments for prisoners of war, where an identical treatment is undergone. Here, in a few days' time, the 18,000 prisoners taken in Gorizia and over 25,000 captured on the Bainsizza attack were all examined.

The interior of Italy is protected from contagion by an advanced quarantine zone and a second wider zone stretching along the foot of the Alps as far as Milan, and from Milan skirting the Apennines along the Via Emilia to the Adriatic. No person can pass out of the

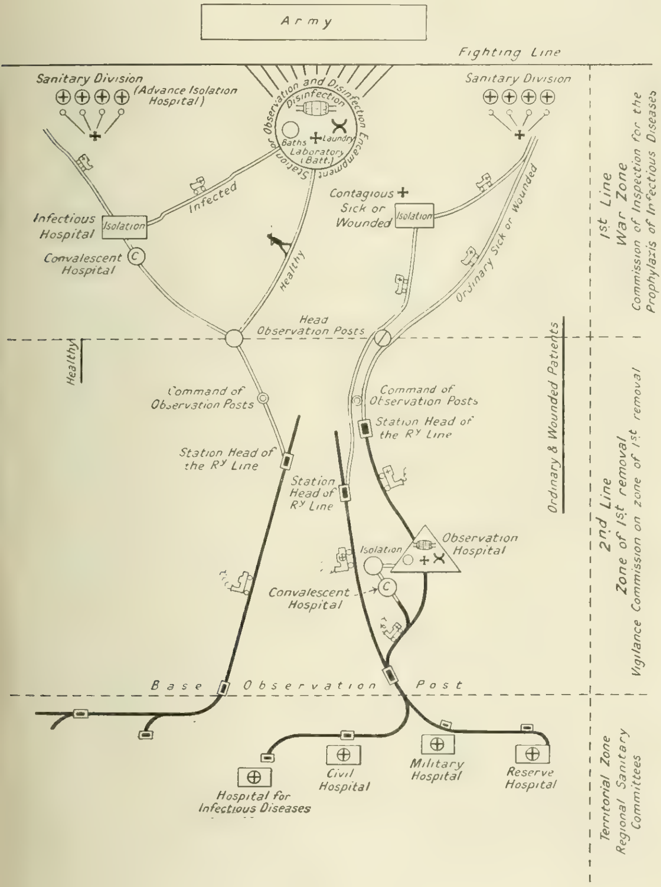
Army
Fighting Line
Sanitary Division
(Advance Isolation Hospital)
Station for Observation and Disinfection Encampment
Disinfection
Baths
Laundry
Laboratory (Batt.)
Sanitary Division
Infected
Infectious Hospital
Isolation
Convalescent Hospital
C
Healthy
Contagious Sick or Wounded
Isolation
Ordinary Sick or Wounded
1st Line War Zone
Commission of Inspection for the Prophylaxis of Infectious Diseases
Head Observation Posts
Healthy
Ordinary & Wounded Patients
Command of Observation Posts
Command of Observation Posts
Station Head of the Ry Line
Station Head of the Ry Line
Station Head of Ry Line
Observation Hospital
Isolation
Convalescent Hospital
C
2nd Line Zone of 1st removal
Vigilance Commission on zone of 1st removal
Base Observation Post
Hospital for Infectious Diseases
Civil Hospital
Military Hospital
Reserve Hospital
Territorial Zone
Regional Sanitary Committees

Fig 1

quarantine zones until two separate examinations have been made, proving him to be germ free. The maps shown contain the territory of the two quarantine zones; the first (Isonzo zone) shows the chain of stations forming a filter for the men coming from the Isonzo. Each of these includes an observation station, a convalescent hospital, an isolation hospital, a bacteriological laboratory, baths, and a disinfecting instalment, &c. There were also three stations for the observation of Austrian prisoners at Cividale, Manzano, and Sant' Antonio. The second map shows the larger quarantine zone, stretching west as far as Milan, and south as far as Forli; the flags mark the quarantine hospitals. A similar quarantine zone has been drawn round Bari, for the troops returning from Albania and Macedonia.

The movements of the civil population in the Isonzo districts occupied by our army were subjected to the same restrictions as were applied to the troops. These measures were supplemented by a strict surveillance of the drinking water, food, and hygienic conditions of the houses, barracks, &c. The supply of drinking water has required a special organization. Owing to the scarcity of springs on the tablelands to the north of Vicenza, by the Isonzo, and on the Carso, it became necessary to organize the transport of water by lorries or by pumps and water-pipes and by the building of reservoirs, &c. The troops on the Asiago tableland alone required 100,000 gallons of water per day.

It goes without saying that cholera vaccination is universally practised side by side with the typhoid and paratyphoid vaccination. The compulsory vaccinations in the army are performed in the following order:—

(1) Small-pox vaccination.

(2) Anti-cholera vaccination. This is performed ten days after the small-pox vaccination, and is given in two doses at five days' distance from each other. The first dose is 1 c.c., and the second 2 c.c. The reaction is generally very slight. The cholera vaccination is repeated twice a year—in the spring and in the autumn. The strength of the vaccine has been gradually increased from 1,000 million to 3,000 million germs per cubic centimetre.

(3) Typhoid-paratyphoid vaccination. This is made with mixed vaccine and is given in three doses at intervals of eight to ten days, beginning seven days after the second anti-cholera vaccination. The first dose is ½ c.c., the second 1 c.c., and the third 1½ c.c. The typhoid-paratyphoid vaccination is practised once a year, between December and March. The mixed vaccine is prepared on the basis of 800 million

of typhoid bacilli, and 400 million of paratyphoid B per cubic centimetre.

The prophylactic organization has never interfered with the military operations, and it has been completely successful. It has even been possible to send the whole of the troops operating in the front line on winter leave for a fortnight, without any danger to the country from contagion. Not one case of cholera has appeared in the interior of Italy.

In 1915, in a few months' time, we had to deal with over 14,000 cases in the army. In the following year the cases amounted to only 170, due to small outbreaks in two units, which were at once arrested. This sanitary organization has been so successful that it has gradually been extended from the Isonzo to the other territories of war, giving us complete security against any sudden outbreak of contagious disease. The diagram (fig. 2) illustrates the cholera epidemic between August 1, 1915, and January 31, 1916. The upper lines represent the numbers of cases notified daily, the lower ones show the number of deaths. The epidemic had several distinct periods of activity, the first extending from August 1 to December 15, followed by an interval of rest. In October there was a new wave, and another period (the worst) in November. In December it gradually subsided; and by the middle of January it was practically extinguished. Our military offensive on the Isonzo during these five months was so vigorously maintained that it is difficult to establish a connexion between these outbursts of the epidemic and the occupation of new ground wrested from the enemy. However I would like to draw attention to the fact that the November rise of the curve followed close upon our big general attack along the whole of the Isonzo front between October 21 and 27, during which period important ground was gained and over 5,000 Austrian prisoners were taken.

I cannot close this subject without referring briefly to the tumultuous exodus of the Albanian, Montenegrin and Serbian populations and of the Serbian Army with its 22,000 Austrian prisoners. The urgency of the immediate removal of this vast crowd, owing to the pressure of the enemy, made it impossible to adopt any prophylactic measure before taking them on board the ships. Cholera, small-pox, typhus and dysentery raged among the crowd of people whose organisms had lost all power of resistance through the exceptional hardships they had endured. They were all taken to the Island of Asinara, on the north-west coast of Sardinia, and the first steps towards isolation and cure could only be taken during the voyage. Everything had to be improvised on the island to meet the sudden emergency. When the crowd first arrived there were 200 deaths per day; after three days eight per day; and ten days later the mortality was *nil*. This episode makes one of the finest pages in the sanitary history of the war.

Among other infectious diseases the most serious of all, because of their immediate bearing on the efficiency of the army and their later influence on the community, are venereal diseases, especially syphilis. It is a difficult question, in which the hygienist has to reckon with social ethics, which are often in opposition to the methods he would wish to see adopted. The measures naturally come under two headings—preventive and curative. The preventive measures consist in a vigilant surveillance of prostitutes, and the prohibition of wandering prostitutes to enter the main military centre of the war zone. The medical authorities have been invested with the power to ascertain, isolate, and treat diseased prostitutes. In the vast majority of cases, the women voluntarily accepted these measures and obviously profited thereby, so that compulsory internments are now quite exceptional. With this is associated a prophylactic treatment of the men who have exposed themselves to the possibility of infection. Finally, dispensaries for the ambulatory or sedentary cure of venereal diseases are established wherever large numbers of troops are quartered. The results have been excellent. Cases of venereal infection contracted in the war zone have almost disappeared; and practically the only cases we have to deal with are those of soldiers who have become infected during their leave.

More than ten years ago the Italian Navy had adopted similar measures which proved very useful. Even small naval units which have no doctor on board possess the necessary equipment for individual prophylaxis—namely, potash soap, calomel ointment, and permanganate for urethral injection. The prophylaxis is obligatory, and is enforced by very simple means. Any infected man who is proved to have evaded the prophylactic treatment has to undergo a period of punishment as soon as he has been cured. There is of course no penalty for the man who has become infected in spite of the preventive measures. By these simple means cases of venereal diseases in the navy have been brought down to less than half of their former number, which was 136 cases per year and per 1,000 sailors. In a few ships where the measures were applied more vigorously the infection disappeared altogether for long periods at a time.

EPIDEMIC CHOLERA FROM AUGUST 1, 1915, TO JANUARY 31, 1916.

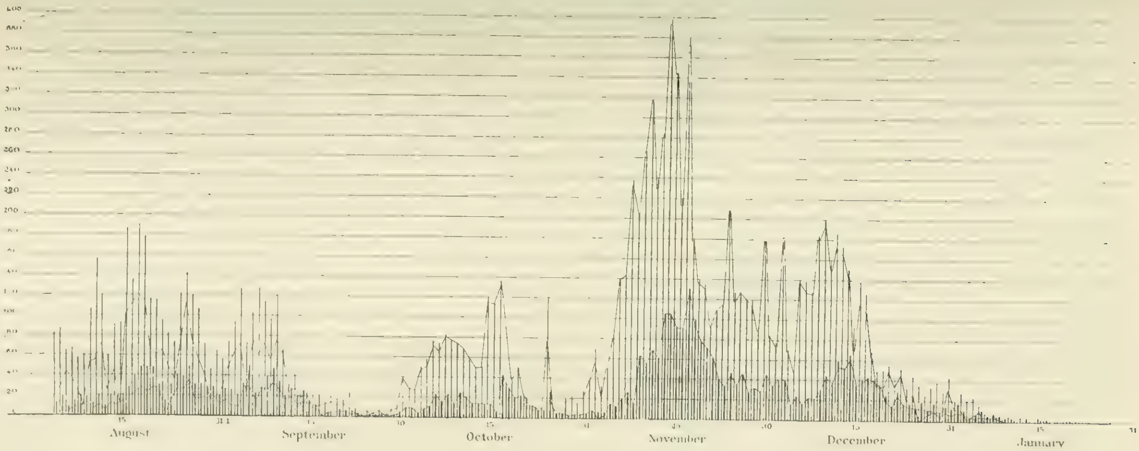

FIG. 2.

The thinner vertical lines show the daily cases notified collectively in the Isonzo Armies.
The thicker vertical lines show the daily deaths recorded collectively in the Isonzo Armies.

Another subject which has always specially interested Italians is malaria. The plain of the lower Isonzo and the lagoons round Grado and the outskirts of Monfalcone have always had a bad reputation in this respect, which we have found entirely justified. But Italy was well prepared for this emergency owing to the long experience gained in the malarial districts of the Peninsula and the islands. There, however, the malaria was fought on the spot among a population belonging to the infected districts, whereas the problem in its relation to the army was entirely different. Here we had to deal with malaria patients who in the natural course of events would be sent to convalesce in their homes, scattered all over Italy, with the danger of creating centres of malaria where the infection had not previously existed. A certain occurrence in the territorial hospital of Voghera brought home to us the reality of this danger. A few cases of recurring malaria had been put in the wards, upon which followed an outbreak of primary infection among the other soldiers in the same wards, who had never been in the war zone or in other malarial zones. A search was instituted and revealed the existence of female anopheles gorged with blood. I must add that the territory of Voghera had been so far entirely immune from malaria. We also noticed in other territories the importation of various clinical forms of malaria which had not previously existed there by troops who had returned from Albania and Macedonia. It, therefore, became imperative to institute special hospitals for the isolation and cure of malarial patients. In regard to prophylactic measures the state of war made it difficult to carry out drainage and general sanitation to any great extent. We have, however, undertaken the work at Vallona and Taranto; while for the lagoons of the Isonzo we have resorted widely to the use of paraffin in the stagnant waters. Other preventive measures have been the netting of windows in barracks, and the distribution of large doses of quinine among the soldiers equally with the civil population. We give as much as 80 cg. of quinine bisulphate per day. In the present year (1917) 250,000,000 tablets of quinine have already been provided for the purpose. An interesting fact relating to quinine prophylaxis is the complete disappearance of malaria in the islands of Maddalena and Caprera, off the Sardinian coast, following a long period of preventive immunization of all their inhabitants.

The problem of tuberculosis has become universally acute, owing to its increase in all the armies. I will not deal with it specially to-night, beyond mentioning that in common with our allies we have created special establishments for its early diagnosis, and for the isolation and treatment of the patients.

In conclusion, I should like to tell you something of the medical school which we have founded in the war zone. No doubt in England you are faced by the same problem as we are of providing a sufficient supply of doctors for the future needs of the country. In Italy the average age at which students graduate in medicine is 24 to 26, which means that since the first day of the War every student of the last four terms became a soldier, and even if appointed to the Army Medical Services he has had little or no opportunity of gaining the knowledge, the experience and the training necessary for a medical man. The majority of the university professors and their assistants, especially if clinicians, had also taken service. Thus both professors and students were serving together in the army. Colonel Tusini, Professor of Surgery at the University of Parma, had the excellent idea of taking advantage of the fact and worked out an admirable organization of a medical school in the war zone, divided between the University of Padua and a large centre of field hospitals at the village of S. Giorgio di Nogaro, which was just far enough from the front not to be under fire. This plan was sanctioned by the General Staff of the Army and the Minister of Public Instruction, and was soon in working order. There was a first term in 1916 and a second in 1917, extending through the five winter months.

The School of S. Giorgio di Nogaro was housed mainly in specially built barracks. It had a staff consisting of a director, twenty-one professors from the universities of the kingdom, dealing with every subject usually taught in the medical schools, and as many assistants. During the first term 1,325 lectures were delivered. The total number of students for each term amounted to about 2,000, of which 1,200 belonged to the school of Padua and 800 to the camp of S. Giorgio. These 800 students of S. Giorgio were quartered in eight large barracks under the superintendence of a major and six subalterns. At the end of the first term we had 460 candidates for the degree, of whom 232 passed brilliantly, 68 obtained the maximum of votes, and 26 passed with honours.

By the initiative of Lieutenant-Colonel Salvi, Professor of Anatomy of the University of Parma, and under the auspices of the Italian Red Cross, a collection of specimens is being made to demonstrate the anatomo-pathology and the traumatology of modern warfare. We have established two centres in the rear of the fighting line where the specimens are being collected and prepared for permanent conservation.

DISCUSSION.

The PRESIDENT (Dr. G. S. Buchanan) expressed the thanks of the Section to Cav. F. De Filippi for a most valuable paper. He would draw attention to two matters among many which specially invited discussion. The first was the reduction of the Italian soldiers' ration both in total calorie value and in protein constituents. Contrasting this ration with the British ration for troops in the fighting line it might be claimed that the Italian was sufficient physiologically, and that the excess of the British over the Italian was, physiologically, only so much waste. But first it must be established that the physiological basis is a sound one, and next it must be remembered that, so far, the British soldier had done very well on his excess. There is a considerable difference in the moral effect upon troops on the one hand of a ration which has to be accepted as the bare minimum which will permit a man to do good work and fight and on the other hand of a ration which the soldier (whatever he may say about it) knows to be in fact an abundant ration which leaves him a margin. It had been stated that the sufficiency of the reduced Italian ration was largely determined by the putting into operation, along with the new scale, of specially good schemes of instruction as to cooking and the avoidance of waste, and of the issue of appetizing condiments: it would be interesting to have information on these points. The statement that a reduction of meat corresponded to a reduction in scurvy, did not correspond with his experience in Mesopotamia, where fresh meat was a most valuable and sometimes the only available anti-scorbutic. The second matter noted for discussion was the system adopted for the prevention of cholera. The large scale of the laboratory work for detection of carriers was very striking. It seemed probable that the onset of the winter conditions was a most powerful aid to the reduction of cases, and acted partly directly by producing conditions unfavourable to the cholera bacillus, and partly indirectly by giving a period of natural quiescence of infection during which the medical service had time to turn round and complete the organization against the spread of the disease in the coming spring. In any case the Italian medical service was to be heartily congratulated on the success of the elaborate arrangements which had been so well described.

Dr. S. M. COPEMAN, F.R.S., opened the discussion, calling particular attention to the avoidance of waste of food which has been effected in home camps by administrative action since the beginning of the War, and to the satisfactory results which have ensued on the replacement of a portion of the daily ration issue by a money payment of 5½d. per man, which by allowing of the purchase of fresh vegetables, extra jam, fresh or condensed milk, cheese, &c., has had the effect of rendering possible useful variations in the dietary. He also referred to the serious consequences of epidemics of measles, mumps, whooping-cough and catarrhal diseases in young soldiers, and suggested that part of future prophylaxis of these diseases might consist of the routine use of inhalation spray chambers such as are employed in connexion with cerebrospinal fever carriers.

Professor E. H. STARLING, F.R.S.: I should like to make a few remarks on the subject of diet and especially on the dietary principles which inspired those who were responsible for drawing up the rations of the Italian Army. I would, in the first place, point out that the lecturer has evidently been supplied with ancient statistics with regard to the diet of the British Army. The British Field Service ration at the present time consists of protein, 168 grm., fat 190 grm., and carbohydrate 495 grm., with a total calorie value of 4,450. Although rich in protein, the British ration does not contain anything like the very large proportion stated by the lecturer. The Italian ration, which before the beginning of the War was 3,300 calories, with a meat ration of 2 kilos,[1] was cut down in February, 1917, to one with a calorie value of 3,000 and a weekly meat allowance of 1·65 kilos,[1] and it is stated that in this reduction the authorities were influenced by the work and writings of Chittenden. The lecturer has opposed Chittenden to the earlier German writers, Voit, Pettenkofer, and others; but Chittenden's conclusions are also opposed to the more recent and accurate work done by his fellow-countrymen, Atwater, Langworthy, Benedict, Lusk, and others. It is on the work of these physiologists and on that of Rubner in Germany and of Tigerstedt in Scandinavia, that our present very exact knowledge of the requirements of man are based. Chittenden laid down two main conclusions as resulting from his experiments:—

(*a*) That a man does not require the amount of protein stated in the dietary tables accepted at that time, and that it is better if he reduces his daily intake of protein from, say, 120 grm. to 60 or 70 grm.

(*b*) That when the protein is thus diminished the daily calorie intake of the body can also be diminished without loss of efficiency and with gain to the general health.

It is this latter conclusion which has given rise to the most misunderstanding, and which, if acted upon, can be actually dangerous to the national safety of the Allies. Chittenden points out that he himself can keep in health and maintain his weight on a diet containing not more than 50 grm. protein and 2,000 daily calories. In this, however, there is no novelty; his colleague, Mendel, required 2,400 calories and 70 grm. of protein. Mendel is a man of average weight and this figure, 2,400, is the figure accepted by all authorities for the metabolic needs of a man of average weight leading a sedentary life. Chittenden is a small, light man, past middle age, and leading the peaceful life of a savant. Great stress is laid on a series of experiments performed by Chittenden on a number of College athletes or soldiers, who on a diet with a diminished amount of protein, and in many cases a low calorie value, carried out their normal vocations including athletic exercises, physical drill, &c. The following points, however, should be remembered in drawing conclusions from these experiments. All the subjects lost weight. This was equivalent to adding a certain ration of their own fat to their daily diet, and this addition in the cases of one or two of them might amount to 100 to 200 calories per day. All are said to have suffered considerably from cold, although they were well housed, well clothed, and protected as far as possible from climatic exposure. At the end of the experiment all were trained "fine" and in every case they went back to their previous habits of diet. It was doubtful, in the case of the soldiers, whether recourse was not had to other foods as well as those accounted for in the diet tables. In no case was the actual amount of energy expended by the body measured—e.g., in the respiratory chamber.

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[1] Including bone.

It is evident that these experiments can give us no guide as to the amount of food required by soldiers on active service, exposed to cold and wet, often forced to sleep in wet clothes, and called upon occasionally to carry out tasks involving muscular efforts to the very limit of their endurance. It must be remembered that all our figures on the energy expenditure of man are based on experiments carried out at a temperature of 15° to 20° C. According to Lefevre every drop of 5° C. means an increased energy output of 300 calories. The average requirements of an adult man of average weight in a sedentary occupation is about 2,450 calories. If he is exposed to cold his requirements go up. If he does physical work his requirements increase in proportion to the work done. An energy output of as much as 9,000 calories per day has been measured by Atwater in the respiratory chamber. This was in the case of extraordinarily heavy work done by a trained cyclist, but under more ordinary conditions of heavy work 6,000 calories per day is by no means an uncommon quantity. This was the average expenditure of energy per day by a party of ninety-six men engaged in cutting wood during the winter in Sweden, and the average diet of a lumberman's camp in Canada in the winter has been found to be as high as 8,000 calories. Lefevre has estimated his own output during fifteen hours' mountaineering at more than a million kilogrammemetres, the heat production reaching 835 calories per hour.

The allowance of 4,450 calories in the British Field Service ration cannot be regarded as in any way excessive under the circumstances of the case. Even though the soldier may put on fat on this allowance when he is back at rest, he will expend more than this energy per day when he is in the trenches and engaged in active operations; and it will be of advantage if he has a store of fat to fall back on to supplement the probably insufficient ration which he will be able to get under these conditions. The allowance of 3,000 calories per day in the Italian ration of February, 1917, must be regarded as quite inadequate, and we can but approve the recent increases which have been made in this ration. But it must be borne in mind that even the increased ration of about 3,300 calories affords practically no margin, especially for soldiers exposed to the arctic conditions existing at the present time on the mountain front. For these men the ration will require to be supplemented if they are not frequently withdrawn to the milder climate of the plains, where, under conditions of rest and warmth, they can replete the wasted energy of their bodies.

I would emphasize the fact that the need for energy is not a matter of theory. The observations of Tigerstedt, Atwater, and many others show that the output of the normal adult when at rest in bed at room temperature is 1 calorie per kilo of body weight an hour. At rest in waking hours it is not less than 1·25 calories. The average weight of Italian soldiers may be put at 60 kilos. Given eight hours' sleep their minimum requirements are 1,680 calories, when no work is done. The ration leaves therefore 1,620 calories available for work conversion and for the maintenance of body temperature when exposed to cold. In such simple movements as those of pedalling a stationary bicycle, the efficiency of conversion is little more than 25 per cent. The ration of 3,300 only provides for work having a thermal equivalent of 405 calories, say 172,000 kilogramme-metres. Magnus-Levy and Blix have estimated the effective muscular work of the average mountain climber at from 30,000 to 60,000 kilogramme-metres an hour. One sees how easily the soldier on active service may be called upon for an output in excess of the provision, even when the temperature is not reduced to the arctic conditions which at present exist on the mountain front. In the case of the British Army, it cannot be denied that, for men engaged in sedentary occupation—e.g., clerks, telephonists, and the artisans attached to the various industries which have grown up round our armies, the full service ration may be in excess of the requirements. The saving of food resulting from the appreciation of this fact does not prejudice the health of the man, and makes for the economy of the nation.

Chittenden was perfectly correct in his conclusions that a man could retain health and perform work on a much smaller protein ration than is given in ordinary dietaries. Many other observers have conducted experiments which led to the same conclusion—viz., that the protein minimum is somewhere in the region of 30 to 35 grammes of protein per day. I should, however, dissent strongly from the conclusion that this reduction of protein is advantageous, although it may be well to know that the protein of our diet could be reduced considerably if need arises without the entire cessation of the work of the country. It must be remembered, however, that if the protein is to be reduced to a minimum—i.e., to the amount which will just replace the tissue waste—the protein must be of high biological value, such as is contained for instance in milk, meat and, strange to say, potatoes. If, as is normally the case, we draw the greater supply of our protein from cereals a larger proportion will be necessary. Various foreign proteins from our foods have to be dissociated into their constituent amino-acids in digestion, and from the mixture of amino-acids thus formed the body has to pick out for purposes of repair just those acids in proper proportions which are necessary to recreate the proteins of the body. A considerable margin of protein is therefore necessary to provide for this transformation. Moreover, it has been shown by Abderhalden that, although men can be maintained in nitrogenous equilibrium on a diet containing only 5 grammes of nitrogen per day, the most trivial disorder, such as a cold in the head, upsets the balance and leads to a loss of nitrogen from the body. Such a result is the basis, in fact, of the ordinary statement that an excess of protein is necessary to increase our resistance to disease. But we do not need to trouble about the proportion of protein in our food so long as the latter represents a sufficient number of calories. Any of the mixtures of food which make up an ordinary diet will be found to contain something in the region of 100 grm. of protein with a diet of 3,000 calories. No army, nor large mass of men, could be fed on the carefully selected diets, including large quantities of cream, which were made use of by Chittenden in his experiments. But protein, especially meat protein, has certain further effects which may be of distinct advantage to an army. Protein is the only foodstuff which is burnt up in the body in proportion to its intake. The other two foodstuffs if they are assimilated in excess are laid down as glycogen or fat in the body. Increase of protein or meat leads to increased production of heat in the body and is analogous to stoking a furnace more vigorously. This excess of energy, liberated in consequence of a meal of meat, cannot be utilized for muscular work. In cold weather, however, it takes its part in maintaining the body temperature, so that a generous meat diet, in virtue both of its protein and its fat content, presents a distinct advantage when given to men who are exposed to extreme cold or to inclement weather.

Finally, it must not be forgotten that the great majority of our soldiers are young men, many of whom have not yet finished growing, while a large proportion have had their previous growth retarded by insufficient food and insanitary conditions of housing. We have only to see the development, bodily, mental and moral, of these men during their six months' training to be convinced of the national advantages of a generous diet to the soldier. In war we pit the physique and moral of our men against those of the enemy and no effort is wasted which is aimed at the development of these qualities.

Brigadier-General MOLA (Military Attaché to the Italian Embassy in London) pointed out that there was a psychological aspect to be considered in the rationing of soldiers at the Front. The soldier must not be allowed to feel that food was being served out to him with stinginess; he was entitled to the generosity of the nation for which he risked his life. Where armies of the various allied countries were fighting side by side, as was at present the case in Italy, comparisons regarding the food supplied to his comrades were apt to depress the moral of the less favoured soldier. General Mola was of opinion that this element had had a good deal to do with the recent increase in the ration of the Italian soldier.

Major R. FARRAR, R.A.M.C.(T.F.R.), said that as regards rations, there had been a very marked improvement in the feeding of troops in training in England. In the earlier months of the War there had been a prodigious amount of waste, particularly of bread and of meat, and whole loaves and good joints of meat were often to be seen in the swill-tubs. At the same time there was little variety, the food was indifferently cooked, and the soldier usually supplemented his rations by suppers of food purchased or sent to him by friends. Since then the ration has been reduced from 20 oz. to 14 oz. of bread, and from 16 oz. to 12 oz. of meat, but the army cooks have been well and carefully trained, and judicious expenditure of the 5½d. allowed in lieu of rations enabled the provision of a generous and varied diet combined with a considerable amount of economy. He had recently analysed the "Economy Returns" and "A.B.48" forms of different units. In a large Training Reserve Brigade of Artillery containing about 2,000 men doing different classes of work, some light, some comparatively heavy, the economy effected by "under-drawing" amounted to about 1 per cent. of all classes of rations, and about 0·7 per cent. was saved out of the 5½d. by underspending. The men received a good and varied diet, and were so well satisfied that only about half of them availed themselves of the excellent soup which was provided at 9 p.m. Economy in cooking was very carefully exercised, all fat, even the grease left on plates, was carefully accounted for, and the dripping either used for cooking or in lieu of margarine, or sold to a contractor for munition purposes; bones were sold to the extent of about 12 per cent. by weight of the meat drawn; bread was scrupulously economized and the pieces left on the mess tables used either for rissoles or bread puddings. Practically no consumable food was thrown away in this unit, and the swill-tubs contained very little besides potato peelings and vegetable parings. He had carefully estimated the food purchased in canteens or outside shops by this unit and found that it amounted to about 3 oz. per head daily of farinaceous food in the shape of buns, &c.—a luxury consumption which would probably occur even if the bread ration were proportionately increased. The amount of proteid food purchased in the canteens was practically negligible. He had also compared two units, one a reserve unit of R.O.D. doing very light work—from one to two hours of moderate physical exertion daily—the other a Training School for N.C.O's., doing intensive training, which comprised six hours daily of the hardest possible physical training, all movements being done in double-quick time. In the former unit, that doing light work, economy of rations by "under-drawing" was effected to the extent of 5 per cent., in the latter rations were consumed to within 0·5 per cent. of the amount entitled. There was no waste, and the rations did not apparently fully satisfy the men's needs, for not only did the unit purchase food rather largely—to the extent of about 1s. per head daily from canteens and shops, but he had heard of them visiting other messes to obtain extra food. His investigations led him to the conclusion that the present home-training ration was very closely and accurately calculated to the needs of men in training, and might even be slightly inadequate to the needs of men doing exceptionally hard physical work.

Major Cav. F. De Filippi: My paper is in no sense an original contribution, but only a synopsis of other people's work and conclusions without any additions of my own. After fifteen years' interruption of biological studies I feel myself diffident in arguing on physiological questions with such authorities as have taken part in the discussion. I venture, however, to add a few remarks to the interesting contribution of Professor Starling. It seems to me that Professor Starling has availed himself of the occasion to submit to a searching criticism the most extreme conclusions of Chittenden and his school. Fortunately no army has been made the subject of a physiological experiment, and there is no relation between the figures of Chittenden and the actual quantities of protein adopted for the Italian Army. Professor Starling has made the main point of his discussion the total amount of calories needed to meet the requirements of the fighting soldier; whereas the chief argument treated in the paper was the amount of protein proportionate to carbohydrates in the ration. On that special point there seems to be no real difference of opinion. The discussion has certainly brought to light that a very considerable reduction has been made in the meat quantities of the ration adopted for the British Army at the beginning of the War. In regard to the total number of calories necessary to meet the requirements of the fighting man, I do not consider that it can possibly be greater than the amount needed for men engaged in the exploration of unmapped high mountain ranges. In the expeditions of the Duke of the Abruzzi, and in the one which I myself recently led in the Himalaya and Karakoram, a ration was adopted containing 3,500 calories, which proved amply sufficient for all, including the two Englishmen who were part of the last-named enterprise.

Section of Epidemiology and State Medicine.

President—Dr. G. S. BUCHANAN.

An Epidemiological Study of the Food Problem.[1]

By Mr. MAJOR GREENWOOD and Miss CECILY M. THOMPSON.

IT has been the policy of this Section to discuss the great medical problems of the war; we have had papers on the sanitation of camps by officers of our own and allied armies, upon typhus, its pathology, epidemiology and clinical aspects, also upon the presumed carrier of typhus and its bionomics. We have not, however, so far had a general discussion upon that wartime problem which admittedly surpasses all others in immediate and ultimate importance.

The present writers have for some time past had to devote a great deal of attention to certain aspects of the food position. They therefore hope that, even if their paper contains little which is not well known, some of the results will be of interest and at least stimulate the kind of discussion and criticism which dissertations upon food usually need and seldom obtain beyond the limits of a scientific society.

An attempt has been made to arrange the subject matter in logical order, but it has been impossible to avoid cross-reference, while the inevitable haste which attends the preparation of all papers in these times must be pleaded in extenuation of errors both of omission and commission.

PHYSIOLOGICAL CONSIDERATIONS.

It is not our intention to re-tell here a story which can be read in any modern text-book, but we may properly notice some points which readers have occasionally overlooked.

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[1] At a meeting of the Section, held March 8, 1918.

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Most of us who have engaged in the fashionable pastime of computing the energy values of diets, make use of the factors proposed by Rubner—viz., 4·1 calories per gramme of carbohydrate and protein, 9·3 calories per gramme of fat. These values somewhat exaggerate the energy yielded by a wartime diet. The protein figure is an average, and implies that the protein consumed is made up of 60 per cent. animal protein (available heat 4·21 calories per gramme) and 40 per cent. vegetable protein (available heat 3·97 calories per gramme). Rubner's value for fat is ultimately derived from Stohmann (Lefèvre, p. 836) and exceeds that of Atwater, who assigned only 9 calories to the gramme of animal fat and 8·5 calories to the gramme of vegetable fat. These differences can usually be disregarded. On the other hand the wastage between purchase and consumption is very important, since in practical rationing one always thinks in terms of purchased food. It is generally thought that a discount of 10 per cent. should be allowed, and this may be adopted when we start with ordinary articles of diet and then compute their physiological value. When, as in the extensive analysis of Slosse and Waxweiler, we are given the energy value of food absorbed, allowance having been made both for differences of wastage in each article and differences of absorption, and we know that the consumption of vegetable food was considerable, it is appropriate to assume a somewhat greater shrinkage than of 10 per cent.—i.e., to divide by something less than 0·9 in order to obtain the purchased value. This remark applies also to the predominatingly vegetable diets of Eastern races. In the next place something must be said on the question of equivalence. Assuming that the material need for protein is satisfied, are all calories, whatever their source, equally grist for the bioenergetic mill?

That the proximate principles are not replaceable isodynamically is a conclusion reached by different routes. Chauveau and his pupils held that glucose was the only source of muscular work, so that the musculo-energetic value of any substance depended upon its yield of glucose within the body. Rubner developed the theory of specific energies of foodstuffs. Lefèvre, whose magnificent treatise is less known and studied in England than it should be, has provided a careful analysis and criticism of these doctrines. We shall not repeat this, but merely comment upon the more popular theory of Rubner (we mean more popular in the sense of more talked about; lucidity is not the most prominent feature of Rubner's technical papers). The essential facts are these. An animal is placed under an environment which reduces to a minimum the heat needed to maintain its body temperature (in effect an external temperature of 33° C. fulfils the required condition). In these circumstances heat produced by the animal at rest measures the necessary energetic transformations of the bioplasm. If such an animal be provided with 100 calories in the form of protein it liberates 131 calories; isodynamic quantities of fat or carbohydrate lead to the liberation of 113 and 106 calories respectively. From this Rubner inferred that the total heat of combustion of glucose was, as Chauveau taught, thermodynamically utilizable or, as it has been put, that glucose possessed no specific dynamic action; intermediate cleavage products of protein were supposed to be oxidized with the production of heat only available *as* heat, not being convertible into work, a statement applicable, but to a lesser degree, to fat. From this it would follow that the substitution of carbohydrate for protein is, given a sufficiency of the latter to replace material waste of cellular substance, pure gain, an inference decidedly gratifying when there is a meat shortage. More recent inquiries have, however, made it probable that specific dynamic action is not so simple nor so limited as the above account suggests. Significant experiments are those of Lusk and his pupils (Lusk, pp. 240, &c.). It is known that glycocoll and alanin are completely convertible into glucose in a diabetic organism. Hence they should not exert a specific dynamic action if glucose cannot do so. Lusk found that they did in fact greatly increase the resting heat production. In one experiment the entire energy content of the ingested glycocoll reappeared as heat liberated by the dog studied. That the phenomenon is not due to the mere explosion of the molecule is shown by performing the experiment upon a diabetic (phloridzin-poisoned) animal. Here the energy content of the glycocoll is eliminated as sugar and urea in the urine; nevertheless the metabolism is increased. The suggestion is then that the extra heat liberated is the outward visible sign of cellular activity induced by some subtle agency. We must not infer—nor indeed did Rubner infer—that the substitution of glucose for protein is pure gain, physiologically regarded. It is, however, true, and has long been recognized to be true, that when either the external conditions are such that heat loss is impeded or the amount of work to be performed so great that the liberation of (mechanically) waste heat is sufficient in extent to jeopardize the body temperature, carbohydrate is really a better source of energy than protein. Under less extreme conditions, when, over and above the physiological service of the body and the requirements for work conversion, a margin of heat production is needed to maintain the normal temperature, the matters here discussed do not seem to be of great importance.

We have dealt with these physiological questions of energetics briefly, and shall be even more concise in our reference to the problem of a protein minimum. Upon this we may re-affirm the conclusion of Rubner—viz., that equilibrium is *possible* so long as 5 per cent. or 6 per cent. of the total calories intaken are derived from protein, subject, however, to the condition that different proteins are not equivalent, weight for weight. That there is no physiological advantage in restricting the protein intake to the level mentioned, but a risk of disadvantage, Professor Starling pointed out at one of our recent meetings. Such other evidence on this point as we desire to bring forward is not of purely physiological origin and therefore outside the scope of this section of the paper. We conclude by noticing such observations upon the metabolism of the fasting man as have been made by strictly physiological methods, that is, studies of starvation where food was withheld, but the fasting man carefully shielded from cold and not forced to work.

TABLE I.—SUBJECT L., HEIGHT 170·7 CENTIMETRES.

Only distilled water was taken during this fast (Benedict).

	DAY OF FASTING			
	1st	11th	21st	31st
Body-weight, kilogrammes	59·60	53·88	50·49	47·39
Rectal temperature at 7 a.m.	–	36·54	36·04	35·96
Pulse-rate (morning, awake)	74	61	59	60
Loss of flesh, calculated from nitrogen loss ...	213·00	308·00	238·00	208·00
CO_2, night, cubic centimetres per minute ...	165·00	128·00	112·00	115·00
O_2, night, cubic centimetres per minute ..	212·00	176·00	154·00	160·00
Respiratory quotient, night	0·78	0·72	0·73	0·72
Percentage of calories from protein	10·60	19·60	16·50	14·40
Calories per square metre (Du Bois), 24 hours	843·00	732·00	653·00	701·00*
Calories per kilogramme, 24 hours	24·20	22·10	20·40	22·60

* Previous day—661·00.

The most recent study is that of Benedict, published in 1915. We cite the results from Lusk (p. 97), collecting the more important in Table I. It will be seen from the value of the respiratory quotient that the source of energy was for the most part body fat and that the rate of metabolism distinctly declined as the fast proceeded, a fact obscured unless surface measurements are available. Beside this we may put the observations of Magnus-Levy (Lusk, p. 476) upon a neurasthenic youth who had partially starved himself for over a year. They indicate that "if a man whose metabolism is normal has been reduced in weight by 30 per cent., his metabolism when he is fed with a low dietary may be reduced 44 per cent. below the actual normal level, or 33 per cent. below, as measured by the normal square metre of surface. If a liberal diet be given at this juncture, the total heat production is 26 per cent. below the actual normal, or 17 per cent. below if recorded on the basis of surface area."

According to an old analysis of Voit, the body of a strong young man contained 18 per cent. by weight of fat. We thus perceive how large is the normal reserve of fuel and that, *under a favourable environment*, viz., at rest in a warm room, the survival period must needs be lengthy. We cannot of course simplify too far. In experimental animals death frequently occurs while there is still an appreciable amount of fat left in the tissues; it would be wrong to look upon 18 per cent. of the body as a species of iron ration for the remaining 82 per cent. But we see that the total store of calories in fat of a 70 kilo man is about 117,000 calories and were only half of these available there is provision for an expenditure of 2,000 calories daily for nearly a month—i.e., sufficient to cover the minimum energy needs for maintenance.

These remarks we hope are sufficient to bring into due prominence the physiological data. The real difficulty of the subject is not so much uncertainty respecting the justice of the physiologists' conclusions as the quantitative application of principles themselves clearly established. In essaying such an application one is step by step forced away from the domain of pure science into the world of confused practice and involuntary, therefore impure, experiment. The first step takes us to:—

THE EFFICIENCY OF WORK PERFORMANCE.

At this stage of the journey one still seems to be within the realm of pure and exact science. Such studies as those of Benedict and Cathcart, of Macdonald, of Atwater, and of many others, seem in no way to fall short in exactitude of the biochemical work which occupied our attention above. Indeed, in one respect they are more accurate. The muscular work performed and the energy utilized by the workers were both measured with nicety. No question could or did arise of approximations dependent upon variations of utilized potential values as when calculations are made from statistics of consumption. So far indeed as one kind of work is concerned—viz., pedalling a stationary bicycle against a known resistance—the relation between work performed and energy used is, for the range of values studied, ascertained. That relation is to a first approximation, $H = aW + b$, where H is the total heat production, W the thermal equivalent of the external work done, a a constant and b a variable parameter (Dunluce and Greenwood). But there is no direct evidence that this relation is applicable to any other kind of work and it is more than doubtful whether it even applies to work on the bicycle for the whole practicable range of work performance. One of us has elsewhere shown grounds for thinking that the linear relation is merely the form assumed by a more general expression *after* a critical value of W has been reached; there is room for much difference of opinion as to how the ultimate approximation to linearity comes about. It is at least certain that the deduction from such observations—viz., that for each unit increment of work three units of heat energy will suffice to maintain the balance of income and expenditure, is not a proposition *quod semper, quod ubique, quod ab omnibus creditum*—should determine the grading of rations for varying severities of work. At best we must regard it as a minimum requirement. Still keeping within the region of pure science, we could solve our problem by directly measuring the energy transformed in different kinds of industrial work, using the invaluable and relatively simple technic of Zuntz, a course actually pursued by Amar (1914) in the study of metal workers, and by Zuntz and Schumburg in their work on marching soldiers. But the amount of information thus accumulated is still small and there is no hope that enough will be done in the immediate future to enable us speedily to reach precise results. It is necessary to face these facts. To calculate the number of footpounds of work performed by an operative, to reduce the result to calories and then to divide by 0·25 proclaiming the quotient to be the energetic need of the workman is unphilosophical. Not only does the speed of the work performance profoundly modify the need (in the linear relation given above the variable parameter b varies with the speed of performance) but the assumption of 25 per cent. efficiency is itself arbitrary, while no account is taken of external conditions, the influence of which it is hard to over-estimate. These considerations suffice, in our opinion, to establish the necessity of evacuating the present ground and travelling a stage further from the laboratory into the debatable land of—

STATISTICAL CONSIDERATIONS.

By way of illustrating the particular difficulties of the statistical method in its present application, we first tabulate some data obtained from Slosse and Waxweiler's inquiry into the nutrition of Belgian handworkers. We take weavers, using mechanical power, the lighter metal workers and the stone quarriers. In each case the energy is net energy, allowance having been made for waste in preparation and loss in absorption, and expressed in terms of kilos of body mass (Table II).

TABLE II.—SLOSSE AND WAXWEILER'S DATA.

Calories per kilo	Weavers (Tisserands au metier mechanique)	Light metals (Petit travail des metaux)	Quarriers
26—30	1·0	1·0	1·0
30—34	5·0	2·0	1·0
34—38	9·0	9·0	2·0
38—42	20·5	15·0	3·0
42—46	15·5	9·0	1·0
46—50	17·5	26·5	1·0
50—54	20·5	13·5	11·0
54—58	18·5	14·0	9·0
58—62	12·5	2·0	6·5
62—66	11·0	9·0	6·0
66—70	3·0	5·0	4·5
70—74	1·0	1·0	1·0
74—78	—	1·0	2·0
78—82	1·0	1·0	1·0
82—86	2·0	2·0	4 0
86—90	1·0	1·0	—
90—94	—	—	1·0
94—98	—		—
98—102	1·0	—	
102—106		1·0	—
106—110	1·5		
110—114	2·5		2·0
114—118	—	1·0	—
118—122			
122—126			1·0
126—130	—	—	
Totals	144	114	58
Mean	52·53 ± 0·85	51·84 ± 0·87	61·79 ± 1·65
Standard deviation	15·15	13·79	18·58
Coefficient of variation	28·84 ± 1·24	26·59 ± 1·27	30·07 ± 2·05

It will be seen that the distributions are irregular and the variation (conveniently measured by the percentage ratio of the standard deviation to the mean, or coefficient of variation) large. It also appears that while the average energy intakes of weavers and light metal workers are practically identical, the quarriers consumed a good deal more. The point to be investigated is whether anything more than this crude fact can be deduced from the statistics, for Slosse and Waxweiler's data are probably the most accurate and homogeneous records of consumption yet compiled.

Here we must revert for a moment to physiological considerations. It is plain that the need for energy, under uniform conditions, will increase with body mass, that intake and mass will be positively correlated. It also follows from the work of Rubner, confirmed by many others, that the relation between surface and energy demand is still closer. Indeed the experiments of Du Bois, of Means and of Benedict (Lusk, p. 127) show that the basal heat production of resting men per unit of surface is very nearly constant. The maximum variation on either side the mean (38·9 calories per square metre) of Benedict's eighty-two subjects was only 10 per cent.

Hence if Slosse and Waxweiler's statistics are *in pari materia* with laboratory observations we should find (*a*) a positive correlation between body mass and energy used, but not necessarily a linear correlation—i.e., a considerable discrepancy between correlation coefficient and correlation ratio—and (*b*) very high linear correlation between body surface and energy use. As Slosse and Waxweiler do not record the heights but only the weights of their subjects, we cannot compute the surfaces accurately; the best approximate formula, that of Du Bois, requires a knowledge of both mass and height; the older method of Meeh, which puts the surface as equal to a constant multiplied by the cube root of the square of the mass, is known to be inaccurate. Nevertheless, the two-thirds power of the mass is a quantity of the same dimensions as the surface; hence if we first correlate mass with energy and then $M^{\frac{2}{3}}$ with energy, we ought to fulfil roughly the requirements of (*a*) and (*b*). This has been done upon the data for weavers. We find that for energy and body mass, the correlation coefficient is $0·16 \pm 0·06$ (the correlation ratio is 0·26), while for "surface" and energy it is $-0·20 \pm 0·05$, the correlation ratio being 0·27.[1] There is a suggestion of nearer approach to linearity in the second case, but in all other respects our expectations are entirely falsified; the correlations are not large and positive but small and negative. The inference is peremptory; either the data are inaccurate, the circumstances not homogeneous from individual to individual of the series, or, lastly, consumption does not vary strictly with energetic needs. These alternatives are not mutually exclusive, and we conceive that each has some share in producing the observed result. On the first point, we remark that the reduction of family budgets to a common "man" standard involves an assumption which may, as Slosse and Waxweiler were careful to point out, lead to quite gross error. We allude to the sort of assumption involved in reckoning a child of a certain age as 0·5 of a man, a woman as 0·8 of a man and so forth. On the second count, we think that, however rigidly one delimits industrial classes, it can never be safe to postulate an equality of conditions whether of work done or exposure to heat loss, or even a sufficient approximation to such equality for it to be possible to deduce any very exact conclusions. As to the third point, any industrial group which is at all well defined, in the sense of our classification, will not probably be living very near the margin of subsistence. Slosse and Waxweiler found—a result which we can confirm—that in none of their industrial classes was there any important correlation between family income and energy value. As, however, the elucidation of this point would involve an extremely close analysis—which, for other reasons we do not think the data adequate to support—we will not press it. Sufficient has been said to make plain the restricted value of statistics of average consumption; they cannot possibly replace experimental data. Within their limits, however, such statistics are of importance. They at any rate establish certain broad demarcations between class and class and incidentally they teach us what alterations of national or class habit will be involved in any method of rationing. The recent report of Dunluce and Greenwood contains a tolerably complete collection of existing material, including the really remarkable mass of data gathered under the auspices of the Welfare and Health Section of the Ministry of Munitions. We shall not repeat the details here but merely provide a short summarizing table (Table III). These figures we hold are congruent with the interpretation placed by most physiologists upon their experimental data—viz., that a purchased diet furnishing 3,500 calories *per diem* is a conservative estimate of the requirements for moderate work. Our next table exhibits the result of a quasi-statistical inquiry which, at the cost naturally of introducing difficulties of its own, evades some which we found insuperable in working upon statistics of consumption.

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[1] Each ratio is for the regression of calories upon the other variable.

In 1910, Amar published a study of work performance by Arab labourers. The kind of work was, unfortunately (for the reasons given above) pedalling a stationary bicycle, but the method differed from that of the laboratories. A good many subjects (thirty-seven) were employed, none of whom was a skilled cyclist, while the energy datum was not the amount transformed per unit of working time, but the twenty-fours hours' ration needed to maintain the body weight constant. Such a datum and, indeed, the whole of the experimental technique (although very creditable to Amar considering the conditions under which he worked) cannot challenge comparison with the procedures of such investigators as Atwater, Benedict and Cathcart; but, from the point of view of an inquiry into rationing, the plan is evidently convenient.

TABLE III.

Source of data	Number of observations	Grammes Daily			Calories daily
		Protein	Fat	Carbohydrate	
English agricultural families	More than 100	90·9	92·4	570·3	3,571
Urban industrial families, earnings 25s. to 30s.	289	91·8	70·6	564·6	3,348
Urban industrial families, earnings 30s. to 35s.	416	99·0	82·4	587·6	3,581
Urban industrial families, wages ranging from less than 25s. to more than 40s.	1,944	98·8	83·7	593·2	3,615
English munition workers, 1917 ...	18,000	115·7	141·3	408·4	3,463

One of us found that Amar's results were quite satisfactorily described by the formula: $H = 6{\cdot}24W + 17{\cdot}78M + 977{\cdot}50$, where H total energy value of the food consumed, W is the thermal equivalent of the work done, and M = the body mass in kilogrammes.

TABLE IV.—ENERGY REQUIREMENTS OF VARIOUS GRADES OF WORK (DEDUCED FROM AMAR'S STATISTICS).

The figures in brackets are the estimated values of purchased food.

Body weight kilos	Sedentary work	Moderate labour 65,000 kilogrammetres *per diem*	Severe labour 130,000 kilogrammetres *per diem*
45	1,778 (1,976)	2,733 (3,037)	3,687 (4,097)
50	1,867 (2,074)	2,822 (3,136)	3,776 (4,196)
55	1,955 (2,172)	2,910 (3,233)	3,864 (4,293)
60	2,044 (2,271)	2,999 (3,332)	3,953 (4,392)
65	2,133 (2,370)	3,088 (3,431)	4,042 (4,491)
70	2,222 (2,469)	3,176 (3,530)	4,131 (4,590)
75	2,311 (2,568)	3,266 (3,629)	4,220 (4,689)

With the help of this formula, Table IV has been constructed. The estimates of the mechanical equivalents of moderate and heavy labour are derived from Amar's later (1914) inquiries into industrial work and are a good deal smaller than those proposed by Lefèvre. From this table we infer that a man doing moderate industrial work and weighing 70 kilos needs 3,177 net calories, or a little more than 3,500 in purchased food; once again a result in accordance with the physiologists' estimates. We do not wish to put more emphasis on this than it will properly bear, but it does seem to us interesting and suggestive.

Finally we call attention to Table V, which gives some of Lefèvre's results on the effects of cold and wind.

TABLE V.—REQUIREMENTS OF SEDENTARY WORKER UNDER DIFFERENT CONDITIONS. LEFÈVRE'S RESULTS. SUBJECT: A CLOTHED MAN WEIGHING 65 KILOS.

The figures in brackets are the estimated values of purchased food.

Temperature	Air current 1 metre per second	Air current 3·5 metres per second
− 1° C.	5,406 (6,007)	6,654 (7,393)
+ 5° C.	4,000 (4,444)	4,704 (5,227)
+10° C.	3,060 (3,400)	3,690 (4,100)
+15° C.	2,317 (2,574)	2,754 (3,060)

We see that a 65 kilo man who at room temperature will, according to our calculations, need 2,133, 3,088 or 4,042 calories according to whether he is sedentary, doing moderate or doing severe muscular labour (Lefèvre's 2,317 calories at 15° C. agree well with our calculation of 2,133), needs as much as 4,000 at 5° C. in an air current of 1 metre per second, 4,704 in a current of 3·5 metres per second. This does not of course mean that the man doing severe work under such conditions would require, say, 4,042 + (4,704 − 2,133) calories; the inconvertible heat of work performance is, in part at least, available to cover the deficit due to the loss by wind and low temperature, but it shows how closely inter-related are the questions of fuel and food provision, or again of food and clothing.[1] We shall see that in each of the special cases of food shortage followed by disease and disaster which we have to describe, exposure to cold weather was involved and hastened the end.

For these reasons, the shielding, especially of sedentary workers, from cold and the proper arrangement of workrooms are essential parts of food economy.

Before leaving this section something must be said on the topic of a protein minimum, statistically considered. Upon this matter McCay has written a valuable treatise. His statistical data are adequate to establish the conclusion that among all races and in every part of the world the successful and predominating classes consume much animal protein. The point is, whether these persons are successful because they consume much protein, or whether, as Chittenden holds, they are high protein feeders because they are successful. McCay strongly supports the former contention. He attaches great importance to a comparison of the physical developments of Bengali and Eurasian students in a large Calcutta college. The conditions of life were identical for the two classes, save the dietaries. The Bengalis consumed 67·11 grm. protein, 548·73 grm. carbohydrates, 71·55 grm. fat; energy value, 3,100 calories, 14 per cent. by weight of the protein of animal origin. The Eurasians took 94·77 grm. protein, 467·00 grm. carbohydrates, 56·20 grm. fat; energy value, 2,830 calories, 40 per cent. of protein of animal origin (McCay, p. 161). Observations extending over many years led to the following results. The Bengalis showed an average increase in weight at the end of three years of only 2 lb., while the Eurasians increased by 14 lb. Only 2 per cent. of the latter but 42·8 per cent. of the former actually lost weight. The average chest measurement of the Bengalis was stationary, that of the Eurasians increased from $33\frac{1}{3}$ in. to $34\frac{3}{4}$ in. In other matters (success in athletic contests) the superiority of the Eurasians was manifest. McCay provides much other evidence of the inferior stamina of the Bengalis, and nobody can dissent from his conclusion that the low protein feeders are on the average inferior living machines. We also think that the destructive criticism to which McCay subjected Chittenden's experiments is cogent. But we are not satisfied that the comparison instituted between Eurasian and Bengali students is sufficient to prove that high protein feeding and good bodily development stand one to another in the relation of cause and effect. After all, something must be allowed to the inherent properties of the bioplasm. A comparison of individuals having different racial origins is not a clean experiment. McCay's statistical method establishes a high correlation between efficiency in its widest sense and a rich protein diet. No physiological experiments which will bear criticism weaken this statistical association. Some, such as the observations of Haecker quoted by Benedict (McCay, p. 144) showing the inferior development of cows fed on a low protein diet, strengthen it. There the matter must rest; we cannot pass from correlation to causation.

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[1] Cf. Hill, 1914.

The practical conclusions we draw from these statistics are the following: To devise an appropriate ration for any class of the community is an extremely difficult undertaking. The general sense of the evidence, whether statistical or experimental, is that a purchased ration of 3,500 calories is, *as an average*, not excessive for adults doing moderately strenuous work. But that such a ration will really satisfy the physiological needs of all the individuals composing the class is highly improbable. This conclusion is enforced by the data of Table II. Rationing is precisely the kind of operation for which averaging is unsatisfactory, here it is directly opposed to the practice of life assurance. The man who lives longer than his statistical expectation and has therefore lost his bet with the insurance company, is not likely to complain; the winner, being dead, cannot complain. But those who need more and those who need less than the average ration of their class are equally (let us hope) alive. He who may consume 500 calories more than he needs cannot be cancelled out against the luckless wight who gets 500 calories too few. From this difficulty no way of collective escape presents itself.

THE CONSEQUENCES OF FOOD SHORTAGE.

Notwithstanding our national boasts of "Merrie England" it would seem that in olden days this country did not have a very good reputation in the matter of rations. There is a mediaeval saw which runs: "tres plagae tribus regionibus appropriari solent, Anglorum fames, Gallorum ignis, Normannorum lepra." Creighton (i, p. 19) suggests that our evil name in this matter was not so much due to habitually short commons (which would be hard to reconcile with the criticism of that fourteenth century precursor of Mr. Kennedy Jones, Higden, who said England was "prae ceteris gulae dedita, in victu et vestitu multum sumptuosa")[1] as to the occurrence of three famines, those of 1194-96, 1257-59, 1315-16, sufficiently terrible to acquire European celebrity. The available particulars respecting the famines of 1194-96 and of 1257-59, do not throw much light upon their epidemiological sequelae. On each occasion there was a great mortality and a pestilence which may or may not have been typhus.

The famine of 1315-16 was likewise associated with various diseases, some of which may have been typhus, while dysentery and a putrid sore throat (pestis gutturuosa) are mentioned. Measures provoked by this famine are of interest. At a parliament held before Easter the prices at which fat oxen, cows, sheep, pigs, geese, fowls, capons, chickens, pigeons and eggs should be sold, were fixed with a penalty of confiscation if sale were refused. In 1316 the price of ale was fixed by statute—ne frumentum ulterius per potum consumeretur—(Creighton, i, p. 50) and an ordinance was issued against extravagant housekeeping. Somewhat similar methods have been heard of in more recent times. But we cannot, of course, say that history repeats itself, because after the fixing of maximum prices in 1316 provisions actually became scarcer than ever and the statute was repealed in the following year. No such dearth as this was seen again in England and of subsequent famines, such as that in Ireland in the nineteenth century, there is little which can usefully be said. In the Irish famine, as in those of towns beleaguered in the Napoleonic wars, particulars respecting which are summarized in Prinzing's book on war pestilences, the story of famine is not separable from that of malignant typhus. This is a natural sequence. Where there is lack of food there will usually be a lack of the other factors of civilized life. The spectre of famine has always been fond of company, but in our days one might reasonably expect to confront him with fewer attendants, and we have endeavoured to find records of famine uncomplicated by epidemic typhus. Two deserve notice. The siege of Paris in 1870 and the events in Kut-el-Amara in 1915.

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[1] See also "Vision of Piers Ploughman," passus vi, lines 1,393, &c.

The mortality of Paris during its siege was the subject of a *thèse de Paris* by Sueur, but, no copy being obtainable in London, we have had to take our information at second hand from Prinzing (pp. 322 *et seq.*). The investment of the city was completed on September 19, 1870. On October 8, the meat ration was fixed at 100 grm. for adults and 50 grm. for children. On November 21 potatoes were requisitioned. During December the quality of the bread deteriorated; white bread was no longer baked, a mixture of bran, wheat, rye, rice, barley and oats being employed. A scarcity of fats was particularly remarked. On December 15 a horse-meat ration of 50 grm. was fixed, and on December 19 the bread ration was reduced to 300 grm. for adults and to 150 grm. for children. The first supplies were brought in on February 4. The actual number of persons within the invested city was not short of the normal population, the exodus of the wealthy being more than compensated by the arrival of refugees from the surrounding country and a reinforcement of the garrison. The age and sex composition was abnormal, males of military age being in excess. The mortality from all causes during the twenty-eight weeks of the siege was 38·6 per 1,000, that of the corresponding weeks in 1867-69, 13·1. Of the recorded deaths, 75,167 (which do not include about 3,000 buried on the battlefields), 8,068 were due to small-pox, which was already epidemic before the siege began, 4,821 to typhoid, 1,042 to dysentery, 2,923 to diarrhœa, 5,623 to pneumonia, and 6,982 to bronchitis. The largest number of deaths from typhoid was in the twentieth week of the siege, 375 compared with sixteen in the corresponding week of the previous year. The weather was cold and fuel (requisitioned in October) as scarce as food. In the twenty-second week of the siege 627 persons died of bronchitis (the corresponding figure for the previous year was 76). Similarly the death-rate from pneumonia was some five times the normal. Scurvy was not widespread, but there were cases, attributed to a shortage of green vegetables. It will be seen that here also we have not a pure famine effect; there is the complication of pre-existing epidemic disease and of deficient fuel during cold weather.

A short account of the events in Kut-el-Amara appears as Appendix III to the Report of the Mesopotamia Commission contributed by Colonel Hehir, I.M.S. Colonel Hehir says that after arrival in Kut (December 4, 1915), the health of the troops improved and remained fair up to the end of February, after which it gradually declined to the end of March. From this time onwards there was a rapid lowering of stamina, vitality, visible condition and health generally to the conclusion of the siege. During the last month (April) men at fatigues, such as trench digging, after ten minutes' work had to rest; men on sentry go would drop down, those carrying loads would rest every few hundred yards; men availed themselves of every opportunity of lolling about or lying down. If we try to correlate these changes with the varying energy value of the diet, we reach the following results. The normal army ration was available until January 20. On January 22 the ration for British troops provided 2,374 calories; 12 oz. of bread and 8 oz. of meat were part of the issue. Horse-flesh was issued early in February and increased to 1¼ lb. on March 1. At that date the energy value was below 2,609 calories, 2,609 being the figure reached on the assumption that horse-flesh and beef are of equal energy value; certainly an over-estimate owing to the sparsity of fat in horse-flesh. By March 11 the energy value was reduced to 2,462 calories and on April 10 a further reduction of bread and flour brought it down to just over 2,000 calories. The garrison surrendered on April 29. The *rapid* decline of vigour and health coincided with the period during which the diet yielded about 2,000 calories. In the early weeks of the siege the weather was cold; during April, "the nights were pleasant, and the days becoming hot, but not uncomfortably so."

The following passages from Colonel Hehir's report are noteworthy:—

> "During the last five weeks of the siege there were 165 deaths from conditions variously diagnosed as gastro-enteritis, enteritis, diarrhœa, &c. The essential cause in these cases was undoubtedly starvation. The men had got into such a state of debility and wasting from shortage of food, that any added strain on the economy, such as diarrhœa, was sufficient to lead to speedy dissolution. In all these cases there was finally a marked subnormal temperature. The condition was clinically identical with cholera: but neither I nor any of the medical officers had any doubt about its not being cholera. This disease continued epidemically after we got to the Prisoners' Camp, Shamran; during the month of May we had over 1,500 cases. . . . Pneumonia (487 admissions) was one of the most serious and fatal diseases, especially in Indian troops, throughout the siege. It was largely predisposed to by the intense cold at night in January and February, exposure in trenches, and in March and April breathing in germ-laden dust, and the more or less lowered stamina. . . . We were fortunate in having no infectious disease."

The condition of Kut approximated more closely to a pure famine effect than did that of Paris, but exposure to extreme cold was, as in Paris, a factor.

We cannot refrain from one further extract from Colonel Hehir's report:—

> "On April 18 I noted: 'There is a vast amount of suffering from hunger amongst the troops, which is being borne with admirable patience and fortitude, and arouses enthusiastic praise at the pluck and grit displayed by both our British and Indian soldiers. As one is amongst the men daily one speaks with intimate knowledge of the conditions; the behaviour of the men in meeting these unfortunate conditions is heroic.'"

A remarkable case in the history of food shortage is that of the Millbank Penitentiary and, as the facts may not be well known, we shall consider it in detail.

The Millbank Penitentiary was a convict prison on Benthamite principles, opened for the reception of prisoners in 1816, and containing at the date of the events we are about to describe nearly 900 prisoners (about 550 males and 300 females) serving long sentences and employed on moderate industrial, or severe labour. Down to July, 1822, the diet was as follows: Each male aged over 18 received 1½ lb. of bread and 1 lb. of boiled potatoes daily. Four days a week 6 oz. of coarse beef (cooked weight without bone) were served. On the other days males received 1 quart of broth derived from the cooking of the meat and thickened with Scotch barley, rice, potatoes or peas, with the addition of cabbages, turnips and other cheap vegetables. For breakfast and supper 1 pint of hot gruel or porridge was supplied (i.e., 1 pint at each meal).

Sometime before July, 1822, the Medical Officer appears to have thought the food excessive, and this opinion was confirmed by Sir James McGrigor, the well-known head of the Army Medical Board. From July onwards, the following changes were made: No potatoes were supplied nor any solid meat. In lieu thereof males received twice daily one pint of soup made from stewing an ox head in the proportion of one ox head to every 100 male prisoners, and thickening the soup with vegetables, peas, and barley. The exact date at which untoward symptoms were detected among the prisoners, whether inability to work or actual illness, is not easy to determine. The task master (Select Committee, p. 103) did not notice any special inability to perform the tasks before the winter, which was a severe one (see, however, Latham, p. 6), but by the middle of February, 1823, there was an alarming amount of illness fully described by Latham, who subsequently published a special treatise embodying his observations. The ordinary signs of scurvy were present in more than half the prisoners and associated therewith, but persisting after scurvy had vanished, was an epidemic which we may, with Mott, quite safely regard as dysentery similar to the minor outbreaks which have been seen more recently in asylums. It was remarked by Latham that of twenty-four prisoners employed in the kitchens all were free from disease, excepting three who had been promoted to the kitchen within four days of the appearance of symptoms. The physicians (Latham and Roget) shortly after being called in ordered a change of diet, 4 oz. of fresh meat daily, 8 oz. of rice, and three oranges taking the place of the ox-head soup, and by the third week of March the scorbutic symptoms had almost disappeared At first it seemed as if the dysentery were likewise abating, but the appearance was delusive; there was a serious recrudescence before the end of April. From now onwards dysentery was the chief disease. Ultimately the whole of the surviving Millbank prisoners were removed to other prisons, but among some of these, particularly the women, such severe relapses and recrudescences occurred that "the only measure which remained was to set at liberty all the female prisoners, without exception, for the sake of preserving their lives" (Latham, p. 193).

This epidemic is a very instructive one. That dysenteric infection was in existence at Millbank *before* the change of diet in July, 1822, a proposition upheld by Latham and vehemently impugned by the spokesman of the Committee, seems to us highly probable. The interesting point is whether the outbreak following the change of diet was provoked merely by the absence of antiscorbutic vitamines or whether deficiency of energy was an adjuvant. That scurvy may have been directly caused by the diet is clear. Potatoes were almost absent and the vegetables given were, as appears from the evidence of the steward (Select Committee, p. 97), celery, carrots, turnips and parsnips only in the proportion of 1 lb. to five persons. These esculents contain antiscurvy vitamines (Chick and Hume), but the stage at which they were added to the soup stock is not described and it is likely that they were subjected to the temperature of boiling water for some time, with resulting destruction of a large proportion of the vitamine content. Turning to the energy value, we can give a rough approximation.

From bread there were derived 1,764 calories in each dietary. This is taking the modern value for army bread; for potatoes, present in the earlier absent from the later diet, 427 calories may be allowed. To gruel we allot Atwater and Bryant's figure of 155 calories to the pound and reckon 1 pint as equivalent to 2 lb. We further allot, to cover the barley and vegetables in the soup ($1\frac{1}{2}$ gills of barley per person and $\frac{1}{5}$ lb. mixed vegetables), 486 calories.

The 24 oz. of cooked meat we represent by Atwater and Bryant's average for the edible portions of neck of ox—viz., 920 calories per pound. For the substituted ox head in the revised diet, we allot 9 lb. of meat at the above value to 100 males, this being, according to the evidence of the cook, the average yield of meat from an ox head (Select Committee, p. 98). We would remark that these allowances are smaller than those of the prison medical officer, who estimated that on the old diet the males received $38\frac{3}{4}$ oz. of meat weekly and 10 oz. on the revised scheme (Select Committee, Hutchison's evidence, p. 73). This is clearly an over-estimate, because it assumes that in the preparation of meat a nutrient value nearly equal to the whole of that contained in the cooked meat served, is retained in the cooking process and rendered up in the soup stock on the meatless days. We have, however, added also the result of using this correction in the table (Tables VIA and VIB). As will be seen, the original diet did not furnish a superabundance of energy and the revision brought it down to some 2,600 calories. In the first place, the prisoners were actively employed. Out of 452 male prisoners in the penitentiary between October 30, 1822, and June 4, 1823, 396 were employed in manufactures, the majority in rough tailoring, but a good deal of heavier labour must have been performed. "From the testimony of the officers of the establishment, and particularly of the matron, it appears that during the last autumn[1] the general health of the prisoners began visibly to decline. They became pale and languid, and thin and feeble. Those employed in tasks requiring much bodily exertion, were unequal to the same quantity of work as formerly. Those at the mill could grind less corn ; those at the pump could raise less water. From time to time some of the laundry women fainted under their work ; and the business of the laundry could only be carried on by continually changing the hands engaged in it." (Latham, p. 4.)

Table VIa.—Millbank Penitentiary, Male Prisoners, One Day.

	Pounds	Grammes			Calories
		Protein	Fat	Carbohydrates	
No. 1 Diet.					
Bread	1·5	54·44	8·10	357·15	1,763
Meat	0·2143	20·12	12·34	—	197
Potatoes	1·0	7·94	0·46	95·26	427
Fresh vegetables	0·2	1·07	0·24	3·71	22
Barley, 1½ gills	—	10·83	1·41	99·26	464
Gruel	4·0	21·76	7·20	114·32	625
Daily total ...	..	116·16	29·75	669·70	3,498
No. 2 Diet.					
Bread	1·5	54·44	8·10	357·15	1,763
Meat	0·09	8·45	5·18	—	82
Fresh vegetables	0·2	1·07	0·24	3·71	22
Barley, 1½ gills	—	10·83	1·41	99·26	464
Gruel	2·0	10·88	3·60	57·16	312
Daily total ...	...	85·67	18·53	517·28	2,644
No. 1 Diet	—	116·16	29·75	669·70	3,498
Additional meat as calculated by Dr. Hutchison	0·1317	12·36	7·59	—	121
Total ...	...	128·52	37·34	669·70	3,619

In the second place, the evidence was clear that many of the cells were insufficiently warmed, a temperature of 46° F. being often unobtainable. Hence, apart from the deficiency of vitamines, we suggest that disaster was inevitable.

Two attempts to justify the diet were made. First, the opinion of Sir James McGrigor, who had approved it beforehand and, even in February, 1823, denied that the diet was responsible for the state of the health in the prison. Sir James did not emerge from this inquiry with flying colours; his investigation of the state of prisoners in February, 1823, seems to have been extremely superficial and he evidently approved the dietary without adequately considering the circumstances. Perhaps he was influenced by the standard dietary for prisoners of war and Botany Bay convicts on ship, which, with other data of historical interest, we collect in Tables VIIa, VIIb, and VIIc. The Navy diet also provided 1 gallon of beer a day; this we have not reckoned in. The second point raised in defence was the experience of Devizes Jail. Here the official ration consisted of 1¼ lb. best wheaten bread, 1 pint of oatmeal gruel, 1 lb. of potatoes daily, with a vegetable soup twice weekly —whether in addition to or substitution for the potatoes is not certain —(*vide* Select Committee, p. 62 and p. 105). This diet provides only

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[1] i.e. before the appearance of scurvy.

Table VIb.—Millbank Penitentiary, Female Prisoners, One Day.

	Pounds	Grammes			Calories
		Protein	Fat	Carbohydrates	
No. 1. Diet.					
Bread	1·0	36·29	5·40	238·10	1,176
Meat	0·2143	20·12	12 34	—	197
Potatoes	1·0	7·94	0·46	95·26	427
Fresh vegetables	0·1	0·54	0·12	1·86	11
Barley, ¾ gill	—	5·42	0·71	49·63	232
Gruel	4·0	21·76	7·20	114·32	625
Daily total ...	...	92·07	26·23	499·17	2,668
Man value ...	...	115·09	32·79	623·96	3,335
No. 2 Diet.					
Bread	1·125	40 83	6·08	267·86	1,322
Meat	0·075	7·04	4·32	—	69
Fresh vegetables	0·15	0 80	0·18	2·78	17
Barley, 1⅛ gills	--	8·09	1·05	74·11	347
Gruel	1·5	8·16	2·70	42 87	234
Daily total ..	...	64 92	14·33	387·62	1,989
Man value ...	...	81·15	17·91	484·53	2,486
No. 1 Diet	—	92·07	26·23	499·17	2,668
Additional meat as calculated by Dr. Hutchison	0·0827	7·76	4·76	—	76
Total	...	99·83	30 99	499·17	2,744
Man value ...	...	124 79	38·74	623·96	3,430

TABLE VIIA. WEEKLY ALLOWANCE FOR PRISONERS OF WAR, 1797.

	Pounds	Grammes			Calories
		Protein	Fat	Carbohydrate	
Bread	7·0	254·03	37·80	1666·70	8,227
Beef	3·5	238·00	285·78	-	3,634
Peas, 1¾ pints . .		95·20	5·95	238·18	1,422
Total	.	587·23	329·53	1904·88	13,283
Daily		83·89	47·08	272·13	1,898

TABLE VIIB.—DIETS OF CONVICTS ON PASSAGE TO BOTANY BAY, 1802.

Six men for one week	Pounds	Grammes			Calories
		Protein	Fat	Carbohydrate	
Bread	20·0	725·80	108·00	4762·00	23,504
Flour	12·0	626·04	54·72	4082·40	19,813
Beef	16·0	1088·00	1306·40	—	16,612
Pork	6·0	272·16	1088·76	·	11,241
Butter	1·5	6·75	576·75	—	5,391
Peas	6·0	625·80	40·80	1633·20	9,752
Rice	2·0	72·58	2·72	716·60	3,261
Suet	1·5	31·95	556·50	—	5,306
Raisins	3·0	31·20	40·80	932·10	4,329
Total ...	. .	3507·28	3775·45	12126·30	99,209
Daily, per head ...	...	83·51	89·89	288·72	2,362

TABLE VIIC.—WEEKLY RATION OF SEAMEN AND MARINES, 1823.

	Pounds	Grammes			Calories
		Protein	Fat	Carbohydrate	
Biscuit	7·0	317·80	31·50	2697·80	12,657
Beef	4·0	272·00	326·60		4,153
Pork	2 0	90·72	362·92		3,747
Butter	0 375	1·69	144·19		1,348
Cheese	0·75	85·05	102·00	8·48	1,332
Peas	1·0	108·80	6·80	272·20	1,625
Oatmeal	2·0	146 00	65·40	612·40	3,718
Total	...	1022·06	1039·41	3590·88	28,580
Daily, per man ..	...	146·01	148·49	512·98	4,083

2,207 calories. Statistics of weights of prisoners at admission and after at least nine months' confinement were tendered in evidence. Fifty-eight gained and twenty-one lost weight; nine neither lost nor gained. The average weight on admission was 145·2 lb., on discharge 148·8 lb. These data are hopelessly incomplete. Although 178 names appear in the return, only 88 were weighed both on admission and at discharge. The prisoners were petty offenders committed from Quarter Sessions, the maximum sentence to be served in the jail being only two years. There is nothing to show that the prisoners were put to regular labour, nor even that the Millbank cellular system was in force. Therefore, all the evidence amounts to is to show that bare maintenance is possible on a diet of 2,200 calories, which is in accordance with physiological expectation. It will be noted that the diet differed for the better from that of Millbank (revised diet) in providing a large amount of potatoes. A review of the whole evidence disposes us to think that the following verdict of a coroner's jury upon the death of one of the Millbank prisoners is substantially correct: "That the said Charles Thompson, being a prisoner in the said General Penitentiary, on the 26th June, 1823, then departed this life from the effects of solitary confinement and the want of the common necessities of life during his residence in the said prison, since 29th June last." (Select Committee, p. 313.)

This long and rather gruesome experiment furnishes additional evidence in favour of the conclusion reached above—viz., that for industrially employed persons, about 3,500 calories are needed. So long as the diet provided about that amount, notwithstanding the probable presence of dysenteric infection and the normally depressing circumstances of prison life, the state of health was good. The sequel to reduction, a gradual loss of power culminating in epidemic disease so soon as cold was added to want, is instructive. We do not suggest that had the reduction of energy been unattended by a loss of antiscorbutic vitamines, the disaster would have been as crushing as it was. But we surmise that the energy deficit alone must have led to serious results even without the complication of scurvy.[1]

We think the conclusions to be drawn from the evidence just marshalled are the following: Semi-starvation lowers the bodily resistance to *all* infections; the type of a famine sickness will depend upon the prevailing epidemic constitution.[1] From the standpoint of symptomatology, gastro-intestinal disturbances—whether genuine dysentery as in Millbank, and, perhaps, in some mediaeval famine fevers, or superficially like those of cholera, as in Kut and often in India—seem to be the most frequent. In all cases, an inability to react with success against mild toxins, characterized by a prevalence of furunculosis and skin affections (observed by Latham at Millbank, and also mentioned by Moore in his account of Indian famine, where the complication of scurvy was not in question) is noticed. As we should expect on physiological grounds, exposure to cold, physical work, or both, notably hasten the decline of health. Moore's observations upon the consequences of undertaking relief works in famine-stricken districts are to be remembered.

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[1] During this period of Latham's attendance thirty prisoners died. (Latham, p. 155.) He attributed twenty-two to *this disease*, meaning dysentery alone or in combination. The number of deaths, if any, due to scurvy alone cannot be ascertained. As late as May 15, 1823, 225 persons were suffering from diarrhœa.

It follows that in predicting the epidemiological consequences of a general shortage in any type of population, we should pay attention to the prevalent epidemic constitution. This leads up to the question whether tuberculosis, which Niven has termed the most convenient mirror of poverty, stands in any specially close relation to famine. It would be stretching that elastic term "epidemic constitution" far to say that the existing constitution is phthisical, although it is a statistical fact that tubercular infections are responsible for more deaths than any other homogeneous cause-group. Many writers again—for instance, Newsholme—are disposed to under-rate the importance of malnutrition as a predisposing cause in comparison with other probable or possible factors. It is also pertinent to remark that, since the outbreak of war, the death-rate from tuberculosis among women in this country (civilian statistics of males are, for obvious reasons, not fairly comparable with peace-time returns) has risen, although during the period over which statistics have been collected, there has certainly been no decrease and in all likelihood there has been an increase in the consumption of food by the labouring classes. It would be an evident *non sequitur* to argue from the last-named observation that this rise would not be accelerated by any lowering of the average state of nutrition; but the data we have analysed do not display any close relationship between famine and tuberculosis.

In conclusion, we may add a word or two upon the psychological aspect of food shortage. In an excellent essay on the morbid effects of deficiency of food, published nearly eighty years ago, Howard has the following passage:—

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[1] Cf. Hirsch I, p. 579, and III, p. 353. Haeser's remarks on the mortality of the years 1770-75 are also of interest (Haeser III, pp. 506-7).

"It is a popular saying, that hunger will break through stone walls, by which it is implied, that its calls are not to be resisted and that the most difficult obstacles will be overcome to satisfy it. This may be true of hunger induced by sudden fasting occurring to persons in good health, but is very far otherwise when an individual is suffering gradually and becoming exhausted by slow degrees, from a long continued deficiency of food or the use of a very impoverished diet. In such cases, when the health begins to decline, mental obtuseness and diminished sensibility follow: all the sensations are blunted, and the feeling of hunger being feebly experienced, no very strenuous efforts are made to allay it." (Howard, p. 28.)

There is nothing to be added to this description, which applies to all the instances we have studied. Its bearing on the course of events in Europe and elsewhere is likewise evident.

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Certain Aspects of the Theory of Epidemiology in Special Relation to Plague.[1]

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DURING recent years many lines of research have been initiated with a view to discover the conditions under which epidemics of plague develop. This research has resulted in the accumulation of much information of considerable value from the point of view of quantitative epidemiology. Though the data are defective in many particulars (for instance the biology of fleas has been investigated in Poonah and that of rats in Bombay) in certain respects they are yet the most complete at the present moment in regard to a disease that is mediately spread and on that account merit careful criticism. The data are contained in a large number of reports published in recent years. As it has been found impossible to make the necessary mathematical calculations with regard to the whole data published, the data considered in this communication are chiefly derived from the series of ten reports issued under the authority of the Advisory Committee appointed by the Secretary of State for India, by the Royal Society, and by the Lister Institute.

As I wish specially to treat the data on my own lines, I omit in this place the historical reference to the work which has been done as I think it will be better to discuss the different conclusions arrived at by other investigators in relation to the evidence obtained from the calculations made with regard to the many epidemics of plague discussed in this communication. Up to the present moment, in spite of the immense literature relating to zymotic disease in general, far too little attention has been paid to the study of the quantitative relationships which hold in epidemics. Without an extensive preliminary survey of these relationships no progress can be made.

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[1] At a meeting of the Section, held February 8, 1918.

What kind of preliminary survey then is required? Two main lines of investigation are open: one direct or observational, the other indirect or deductive. In the former case observations widely collected must be submitted to some kind of mathematical treatment, not necessarily in the first instance very recondite. From these observations the forms of epidemics which are common will appear, and the differences which may be found to occur in epidemics of the same and different diseases may become apparent. It may also be determined from the same data whether epidemics occur periodically or not and also the influence which such agents as temperature, humidity, &c., exercise on the progress of epidemics. In the second case different laws of infectivity may be assumed, and by calculation based on these and on the number of individuals available for infection, epidemic forms may be derived. In this way the consequences following on any hypothesis may be investigated and compared with the facts. In both these fields there is room for many workers, as the spade work necessary for safe induction has hardly yet passed the stage of a first approximation.

A special caution is here necessary. It might be thought that when the form or forms of the epidemic wave had been discovered by actual observation that these forms would necessarily throw some light upon epidemic laws in general. Unfortunately this hope has proved vain. Forms of epidemic wave indistinguishable statistically from those actually found arise on at least four different hypotheses. The fact that some of these hypotheses, though manifestly inapplicable to the particular cases considered, nevertheless graduate the data most successfully must "give us pause."

In the succeeding discussion I do not wish to introduce any unnecessary mathematics but there are one or two quantities which I must use and must therefore explain. One of these is the well known standard deviation which is fundamental in the theory of all distributions and which describes to a certain extent the relative form of such distributions. It is usually denoted by the Greek letter σ.

If two curves are being compared that curve possessing the higher value of σ is flatter and more extended. The standard deviation, as will be seen, furnishes an aid in differentiating between the epidemiological theories. Two further constants are required; they are both denoted by the Greek letter β and are distinguished by β_1 and β_2. This is the notation originally given by Professor Pearson, a notation which has come into constant use. By these two constants certain properties of curves can be conveniently described in a manner suitable for comparison.

The first constant β_1 describes the degree of symmetry or asymmetry possessed by a curve. If β_1 is small in value, the curve is very nearly symmetrical. As the value of β_1 increases the curve becomes more and more asymmetrical. A series of values of β_1 referring to a series of epidemics thus describes roughly the degree of symmetry or asymmetry present in each case. For the normal curve of error, and for any perfectly symmetrical curve $\beta_1 = 0$.

The constant β_2 fulfils a different function and describes the form of the curve. In the case of the normal curve of error $\beta_2 = 3$. As β_2 increases in value the curve becomes narrower and with a higher curvature at the apex than the normal curve, while if β_2 falls below 3 the curve becomes broader and flatter at the apex. In plague the curve which describes the epidemic is in almost every case the curve which has been called by Professor Pearson Type IV. The equation of the curve in its most general form is—

$$y = \frac{M}{\left(1 + \frac{x^2}{a^2}\right)^m} e^{-\nu \tan^{-1} \frac{x}{a}}$$

but in most cases the curve assumes a symmetrical form and the equation then reduces to—

$$y = \frac{M}{\left(1 + \frac{x^2}{a^2}\right)^m}$$

This curve has at present no name, but provisionally I propose to give it the name of the "epidemic curve" as this curve may arise on almost any epidemic theory which has hitherto been advanced.

In this curve β_2 is regularly greater than 3, and if β_1 is greater than zero, $2\beta_2 - 3\beta_1 - 6$ is also greater than zero.

Theories of Epidemics.

Of the hypotheses on which a theory of epidemics can be based, it is only necessary for the present purpose to mention two.

(1) Let it be assumed that the infecting power at any particular moment is in proportion to the number of acute cases of the disease and that the infectivity of the organism remains constant. According to the value of the constant describing the infecting power, a series of epidemic waves can be calculated which in the first instance resemble closely the normal curve of error and change as the infectivity increases to the asymmetrical form of the "epidemic curve." In all cases these curves exhibit a more rapid fall than rise. On this hypothesis an epidemic comes to an end because with each extension of the disease the susceptible individuals become so much diluted by the insusceptible, that is, by those who have succumbed to the infection, that the former no longer constitute a sufficiently large proportion of the total population to permit the spread of the infection. After this point has been reached only a few sporadic cases of the disease can occur. On this theory, the form of the epidemic curve changes, as the power of infecting on the part of the organism increases, in such a way that the value of β_2 also increases. Thus, taking the whole number of susceptible persons as 1,000, and assuming that on an average each infected unit infects 1·4 units, the value of β_2 is "3·3." This value rises continuously as the infectivity increases, being equal to "4," when each infected unit is assumed capable of infecting two others. In the first instance the curve is nearly symmetrical; in the second instance β_1 0·4, a figure indicating considerable asymmetry, the asymmetry in this instance being of the nature that the decline of the epidemic is more rapid than the rise. This asymmetry increases further as the infectivity increases.[1]

(2) On the second theory the hypothesis by which an epidemic is explained is fundamentally different. It is assumed that in general an epidemic ends because the organism loses infective power. In the particular form of the theory developed by myself the infective power is described at any instant by the terms of a geometrical progression. In other words, the organism producing the epidemic loses infecting power according to the law of a monomolecular reaction. In this case, no matter what the power of infecting may be initially, β_2 is always

[1] To be modified by the results of Theorem VI, Appendix (p. 120), a theorem not discovered when this was written.

equal to 3. Increase in the value of β_2 is explained as due to the concentration of the infecting agent. Many cases of the disease on this theory tend to infect to a greater extent than the mere actual number of infecting agents would require. That such a view is tenable is shown by the manner in which the death-rate varies with the density of the population. Applying the same law which has been found to hold for the relationship between the density and the death-rate the form of the epidemic curves observed can be easily obtained. Large values of β_2 can thus be explained as due to the increasing importance of the concentration of the infecting agent.

(3) Both these theories may be combined, but when combined the importance of the second is so much the greater that the influence of the first may be practically ignored.

Plague, a Test of Epidemiological Theory.

As a test of these theories plague offers some special advantages. The epidemiology is very complex. The disease depends firstly on the plague bacillus, an organism subject to biological variations, as are all other organisms. In many cases these variations must be of a very profound character, though they may be far from easily observable under the artificial conditions of laboratory experiments. Secondly, there is the rat or the marmot, the natural hosts of the plague bacillus. Thirdly, there is the flea, which conveys the parasite from animal to animal, and as a secondary phenomenon from animal to man, man being capable of infection by the plague bacillus mediately through the flea equally with the rat.

The biology of a plague epidemic thus involves many issues, but that is not necessarily a drawback. Though the general epidemiological problem must be assumed essentially similar whether a disease attacks animal or man, yet in this instance there is a great difference between the epizoötic and the epidemic, for while the flea can transmit plague from rat to rat, or from rat to man, it is generally agreed that the communication of plague from man to man hardly occurs in this way. The form of the curve of the epidemic in man being thus largely independent of man himself may be found to throw some light on the ætiological interpretations.

In my view epidemics are best studied in great cities. In such cities there is a very large number of susceptible persons, so that an epidemic may reasonably be considered as running independently of the exhaustion in the number of such persons. The basis on which the theoretical form of the epidemic may be developed is thus much simplified. Further, in large cities the progress of the epidemic cannot be altered to any very great extent by the removal of a large proportion of the total population to an uninfected site, as is frequently done in villages. Even in the great epidemic of plague in London in 1665, in which possibly one-tenth of the population perished, the emigration, though very considerable, was not on a sufficiently large scale to alter fundamentally the progress of the epidemic. Further, in large towns the statistics are in general more trustworthy, and even when the figures regarding the number of deaths from plague are not quite trustworthy the fact that the returns of the deaths from all causes are also published affords a certain check on the figures which refer to the deaths from plague. Holding this view it is natural that the phenomena observed in great towns or cities should in the first place be considered.

The earliest epidemics of which sufficient records exist regarding their progress from start to finish are those of the City of London. These records begin in the year 1563, and continue more or less intermittently till the latest outbreak of plague in the year 1665. The chief epidemics during the period occurred in the years 1563, 1593, 1603, 1625, 1635 and 1665. Of the records included in this series those referring to the year 1593 are unfortunately quite untrustworthy. The number of deaths ascribed to plague varies in an extremely arbitrary manner, and when the number of deaths due to plague is compared with the number of deaths due to all other causes no correspondence can be made out. In addition, the figures relating to the deaths from all causes, apart from plague, suggest that some other epidemic besides plague was present in the later part of the year. The figures for the year 1635 also have been rejected. This epidemic is small in comparison with the rest, while during the decline of the epidemic a recrudescence occurred. It must therefore be considered as consisting of a mixture of two epidemics. And such a mixture is at the present moment incapable of analysis. In this case, however, it may be remarked there is no indication that the figures are not trustworthy, as the deaths ascribed to all other causes and the deaths ascribed to plague vary in a manner which is quite within the limits of random error. Four epidemics are thus left for consideration. As I have shown before, the curve which describes the type of epidemic in many diseases most accurately is the symmetrical form of the curve described by Professor Pearson under the heading of Type IV and which I have just proposed to call the "epidemic curve." In the present case this may be said to be true, the four solitary epidemics considered being in two cases described with great accuracy, and in the other two with very fair accuracy. All these curves are very nearly symmetrical. In the last instance this opinion is contrary to that implied in my first paper on this subject. In the calculations made in that paper the constants of the epidemic of plague in London in the year 1665 definitely show the epidemic to be asymmetrical, but on a re-examination of the figures it appears that the asymmetry formerly calculated is due to the defective attribution of the deaths during the later portion of the epidemic to their proper causes. The number ascribed to plague as given in the statistics is a greater proportion of the total number of deaths from all causes than is justified by the statistics of other years. When due allowance is made for this error the asymmetry disappears. As a correction such as this should not be rashly made, I do not insist upon its acceptance. When, however, it is noted that during the latter months of the year 1664 and the earlier months of the year 1665 the weekly number of deaths before the epidemic of plague began is in no instance less than 300, which is a minimum, and rises as high as 470; that from the end of October to the middle of November of the year 1665, the same excess of the deaths from all causes over those from plague exists; and that in the latter end of November and the beginning of December the excess is only about half, or 200 a week, it is obvious that some correction must be made. To assume that the last figure should be somewhere between 350 and 400 is, I think, legitimate.

It is a far cry from the year 1665 to the nineteenth century, but I have not come across any statistics of the number of deaths from plague described serially in periods of time in any large city between that date and 1835, when a series of outbreaks occurred in Alexandria. Of these outbreaks three were of considerable size, and of these three one is definitely symmetrical.

With the outbreak of plague in the East at the end of last century the data become very numerous. Calculations of the value of β_1 and β_2 have been made for more than one hundred epidemics occurring during the last twenty-five years in the cities of Hong-Kong, Calcutta, Bombay, Poonah, Belgaum, Allahabad, and various other towns.

The values of these constants are given in the accompanying table for each individual epidemic (Table I). These values as thus given cannot be considered as the sole criterion. In an epidemic of any disease,

TABLE I.—SHOWING THE VALUES OF β_1 AND β_2 FOR EPIDEMICS OF PLAGUE IN MAN IN CERTAIN LARGE TOWNS.

	Date	β_1	β_2
London	1563	0·119	3·222
,,	1603	0·032	3·268
,,	1625	0·022	3·727
,,	1665	0·013	3·488
Alexandria	1834	0·385	5·197
,,	1840	0·212	3·418
,,	1841	0·062	3·407
Hong-Kong	1896	0·001	3·632
,,	1898	0·334	4·387
,,	1899	0·074	3·864
,,	1900	0·153	3·855
,,	1901	0·001	5·290
,,	1902	0·016	4·051
,,	1903	0·057	4·632
,,	1904	0·001	3·515
Sydney	1900	0·001	3·356
Lahore	1903	0·212	4·419
,,	1904	0·004	4·888
,,	1905	0·005	4·456
,,	1906	0·694	6·008
Calcutta	1901	0·004	5·180
,,	1902	0·014	4·507
,,	1903	0·179	5·020
,,	1904	0·017	5·401
,,	1905	0·043	5·310
,,	1906	0·620	4·097
,,	1907	0·337	5·171
,,	1908	0·323	3·454
,,	1909	0·002	3·385
Jhansi	1911	0·337	3·999
Bombay	1898	0·014	3·433
,,	1899	0·000	3·545
,,	1900	0·006	2·723
,,	1901	0·008	3·558
,,	1902	0·221	2·910
,,	1903	0·018	3·512
,,	1904	0·112	3·808
,,	1905	0·055	2·853
,,	1906	0·010	4·402
,,	1907	0·117	5·167
,,	1908	0·296	4·926
,,	1909	0·358	4·720
,,	1910	0·043	3·059
,,	1911	0·063	3·702
,,	1912	0·016	4·102
,,	1913	0·024	4·241
Poonah	1897-8	0·007	3·691
,,	1899	0·395	4·709
,,	1900-1	0·256	4·026
,,	1901-2	0·561	3·190
,,	1902-3	0·307	4·941
,,	1903-4	0·001	2·539
,,	1904-5	0·027	3·381
,,	1906	0·077	4·373
,,	1907-8	0·118	2·597
,,	1908-9	0·243	3·153
Belgaum	1897	0·453	3·859
,,	1898	0·070	3·420
,,	1899	0·032	4·194
,,	1900	0·084	6·044
,,	1901	0·017	4·227
,,	1902	0·567	4·563
,,	1903	0·001	4·017
,,	1904	0·097	3·827

even if the typical epidemics were symmetrical, symmetry could not be expected as a regular phenomenon. Owing to action of chance asymmetry must occur more or less frequently. Thus the rise of an epidemic might be more prolonged than the decline, and, on the other hand, the decline of the epidemic might be more prolonged than the rise if, in the first instance, the extension of the epidemic from densely crowded districts to less crowded districts occurred during the decline of the epidemic, or in the second instance if a densely crowded area were invaded at a late stage. The test of asymmetry will thus not rest on the form of any individual epidemic, but if the data in regard to epidemics of similar size in the same place are combined it should be possible to observe whether the constants of the combined epidemics exhibited more or less approach to symmetry. The easiest way in which this can be effected is to compound the various moments round the mean of each epidemic for a series of years in a definite place. This has been done, and the results of the various combinations are given in the table (Table II). It is found by this means that epidemics are as often asymmetrical on the one side as on the other, and it is further found that the compound epidemics are practically in every case almost absolutely symmetrical, the value of β_1 not exceeding 0·02. On the whole the decline of the epidemic is, if anything, slightly more rapid than its rise. From these examples I think it therefore may be taken that in large cities, at least as exemplified by the cities referred to, epidemics of plague among man run a course described fairly accurately by the "epidemic curve."

TABLE II.—SHOWING THE VALUES OF β_1 AND β_2 FOR CERTAIN TOWNS WHEN THE EPIDEMICS ARE GROUPED AS DESCRIBED IN THE TEXT.

	β_1	β_2
Poonah, large epidemics— 1897-8, 1898-9 1900-1, 1902-3 1904-5, 1906	− 0·016	4·519
Poonah, small epidemics— 1901-2, 1903-4 1907, 1908	+ 0·000	3·168
Poonah, all epidemics	− 0·010	4·247
Hong-Kong—		
1895-1903	+ 0·009	4·422
Bombay—		
1897-1906	− 0·032	3·377
1906-1913	+ 0·065	4·338
1897-1913	− 0·011	3·537
Calcutta—		
1901-1909	+ 0·023	4·967
Lahore—		
1903-1906	− 0·077	5·191
Belgaum—		
1897-1904	+ 0·006	4·351

Note.—The sign prefixed to β_1 signifies when − that the decline of the epidemic was more rapid than the rise, and vice versa when +.

Plague in Rats.

The problem of the epizoötic in rats has aspects of importance of a quite different character. The most extensive data relate to the epizoötic in rats in the city of Bombay, and the first part of the present discussion is based on these figures. The rat population in Bombay is formed of two types of rat, the black and the brown, the former as estimated by the relative frequency with which the two species are caught in traps being in proportion of about seven to three of the latter.

The black rat is essentially a house rat and inhabits all houses, even the better class bungalows, from floor to roof. The brown rat, on the other hand, frequents the basements and the sewers and in no case has it been caught in traps placed in positions above the third floor. Both rats, but to some extent specially the black rat, are thus in direct relationship with man. In spite of the preponderance of black rats, the brown is much more frequently affected with plague than the black. This opinion is based not merely on the much greater frequency with which brown rats collected dead are found to have suffered from plague, a fact open to possibly different interpretation on account of the different habits of the species, but specially upon the fact that among those trapped alive more brown rats are found to be affected with acute plague, while the lesions considered to be sufficient to demonstrate the existence of resolving and resolved plague are much more common among the brown than among the black rat. The different observations indicate that the proportion of brown rats affected by plague is at least two and a half times that of the black rats. Extreme accuracy for the present purpose is not necessary in this matter, the differences recorded being sufficiently large to demand very special differences in the epizoölogy of plague in the two species affected.

The season of the year in which plague chiefly affects rats in Bombay extends from February to May, during which the main mortality occurs. A slight recrudescence of rat plague sometimes takes place in the latter part of the year, but this is not sufficiently frequent nor sufficiently great to entitle it to rank as a regular phenomenon. Further the evidence is nearly complete that plague is constantly present in Bombay as an endemic disease among rats. The epidemic may therefore be regarded as produced locally and not as the result of the introduction of new infection. A curious point falls to be noted that though the epizoötic first develops itself in the brown rat the black rat seems to be the chief means of carrying the disease during the inter-epizoötic stage.

One other very important factor remains to be noted. Though the mortality from plague during the height of the epizoötic is very considerable only one-fourth of the rats found dead at this period are considered to have died of plague. The epoch of the plague epizoötic is quite evidently also the epoch of other epizoötics of a combined importance much greater than that due to plague. This fact, if it be a fact, must be remembered when the problem of the epizoölogy of plague among rats is statistically treated. It brings a large possibility of error into the statistical differentiation of epidemiological theory but the error may be successfully estimated.

This view of the presence of other epizoötics is confirmed in another way. When plague attacks a town the number of rats caught per trap gradually falls. In the case of Belgaum the fall in an instance given is from thirty-three rats per 100 traps to about fifteen. This phenomenon has been ascribed to mortality from plague but it also occurs in the absence of plague and has been observed in Madras to nearly the same extent. If the fall in numbers trapped is really the result of mortality among rats then the mortality must be largely independent of plague.

The data relating to plague among rats in Bombay are of two kinds. During the year 1905-06 the epizoötics in both rats were recorded for twelve different districts of the city. For the succeeding five years the published figures relate only to the city as a whole and these figures give the number of plague-infected rats of both species in each half month. Considering first the range of date of the epizoötic it is found that in Bombay for these years in all cases the epizoötic for black and brown rats alike is essentially confined between the limits extending from the middle of December to the end of June. To determine the maximum the mean of the epizoötic in the two species of rat has been calculated by the usual methods. As will appear the mean and the maximum in these cases nearly coincide.

In the year 1906 the mean of the epizoötic of plague in the black rat was nine days later than in the brown, in 1909, 4·5 days later, in the year following—namely, 1910—2·6 days earlier, and in the year 1911, the last for which statistics exist, the mean of the epizoötic among the black rat was again three days later than that among the brown. The average difference for the four years is thus only three and a half days. The difference found is much less than that published referring to the year 1906 which amounted to seventeen days. It is unfortunate that this result as established earlier has been taken as expressing general experience. This point is referred to later.

The values of β_1 and β_2, as fundamental constants of the epidemic, give some further information and are best studied first in the data referring to the year 1906 (Table III). For this year the data referring to twelve different districts in Bombay have been published. It is found that for both species of rat two-thirds of the epizoötic curves found in the different districts of the city are symmetrical and further that for both types of rat the epizoötic curves for the whole city are essentially symmetrical. In this regard no difference exists between the epizoötics.

TABLE III.—SHOWING THE VALUES OF β_1 AND β_2 FOR THE DISTRICT EPIDEMICS OF PLAGUE IN BOMBAY IN *Mus rattus* AND *Mus decumanus* AND IN MAN, 1905-06.

District	*Mus rattus*					*Mus decumanus*					MAN				
	Number of rats	β_1	β_2	Mean of epidemic	σ	Number of rats	β_1	β_2	Mean* of epidemic	σ	Number of deaths	β_1	β_2	Mean* of epidemic	σ
Fort North and South	342	−0·182	2·749	83	2·61	1,346	+0·005	2·848	68	2·50	332	−0·002	2·923	96	2·60
Mandvi	1,230	+0·102	2·726	85	2·90	2,058	+0·125	2·753	66	3·13	498	−0·094	3·762	97	2·38
Chakla	448	−0·003	2·858	81	2·21	1,407	+0·007	3·170	67	2·37	303	−0·181	4·898	100	2·04
Market	347	−0·051	5·168	87	1·56	1,117	−0·310	4·401	72	1·85	429	−0.051	4·872	101	2·12
Oomarkhadi ...	456	+0·702	3·867	77	2·67	1,720	+0·425	4·171	54	2·24	549	−0·001	3·051	94	2·17
Dongri	374	+0·883	4·103	92	2·34	835	+0·375	4·316	68	2·35	512	+0·041	3·259	99	2·51
Dhobi Talao ..	237	−0·014	3·170	83	2·19	1,473	+0·068	3·282	67	2·26	506	−0·011	3·163	97	2·18
Bhuleshwar ...	519	+0·020	3·501	80	2·27	1,766	−0·058	3·577	58	2·18	631	−0·002	3·511	98	2·11
Fanaswadi ...	995	+0·157	2·587	83	2·16	348	−0·003	2·310	72	2·27	434	−0·004	3·355	101	2·23
Khara Talao ...	243	+0·551	3·725	76	2·78	1,072	+0·739	4·471	55	2·61	316	+0·048	3·175	100	2·21
Khumbharwadi ...	242	−0·149	3·292	85	2·05	1,018	−0·001	4·054	71	2·30	356	−0·470	5·017	100	1·81
Khetwadi ...	54	−0·695	3·313	98	1·99	217	−0·220	3·297	84	1·83	263	+0·244	4·241	109	1·89
Whole City ...	—	0·070	3·159	—	2·53	—	0·038	3·499	—	2·45	—	0·010	4·402	—	—

* The mean of the epidemic is expressed in days from the beginning of the year.

Correlation of figures in this table :—

	β_2	Date of mean
Mus rattus and *Mus decumanus*	0·760 ...	0·837
Man and *Mus decumanus*	0·203 ...	0·722
Man and *Mus rattus*	0·058 ...	0·691

In the values of β_2 however a distinct difference is seen. For the city as a whole the value of β_2 for the brown rat is 3·50, as against 3·16 for the black rat, figures expressing some profound difference in the method or means of spread of the epizoötics. When the data for the different districts are considered it is found that though considerable variations in the value of β_2 occur, there is very high correlation (0·76) between the values of β_2 as obtained by calculation for the epizoötics of the brown and black rats.

The same close correlation is found for the years 1908-11 between the epizoötics among the brown and black rats when the whole city of Bombay is considered (Table IV). The values of β_2 are however not strictly comparable with those given above, as in the present case many rats are recorded as having died of plague during the inter-epizoötic period. The base line thus never approaches zero. If no allowance is made for this, and in the present instance none has been made, it follows that the values of β_2 calculated are too small when compared with those given above. The correlation between the value of β_2 in the epizoötics of the brown and black rat is in this case 0·9, being greater than that previously given. Further the constant β_1 is nearly equal to zero in all cases, showing that both epizoötics are symmetrical, or in other words, the maximum and the mean are identical.

TABLE IV.—SHOWING THE VALUES OF β_1 AND β_2 AND THE DATE OF THE MEAN OF THE EPIDEMIC, BOMBAY, 1908-11.

Year	*Mus rattus*					*Mus decumanus*				Man	
	Number of rats	β_1	β_2	Date of mean	σ	Number of rats	β_1	β_2	Date of mean	σ	Date of mean
1908	2,236	0·083	2·697	89	2·68	12,308	0·018	3·095	80	2·50	104
1909	1,809	0·045	2·919	86	2·43	8,192	0·062	2·980	81	2·50	104
1910	2,067	0·020	2·157	78	3·10	9,030	0·001	2·329	80	2·95	112
1911	1,951	0·029	2·360	85	2·80	10,415	0·000	2·622	82	2·74	103
Mean	—	—	2·533	—	2·75	—	—	2·757	—	2·67	—

Correlation β_2 *Mus rattus* and *Mus decumanus* = 0·915.

So far it has been shown that the form of the epizoötic in the black and brown rats are essentially different; that the black rat in Bombay is more than twice as common as the brown rat; that the percentage of the black rat developing plague is only about half that of the brown rat and that the main mortality among rats which causes the fall in the number of rats in the latter part of the year is not due to plague but is independent of plague.

One further feature requires question, and that is how far the rat population is rendered temporarily immune to plague after the epizoötic. The evidence regarding this is slight, but a small number of experiments referring to twenty-nine animals in the first instance, and to forty-six in the second, have been made. The former experiments were performed during the epizoötic and the latter during the post-epizoötic period. The respective number of deaths were in the former case 55 per cent., and in the latter case 28 per cent. of the infected animals. If the results of this experiment hold generally it may be concluded that only about half of the susceptible rats develop the disease in any epidemic. Such later experiments as have been made give the same range of values, so that these proportions may be taken as a basis of discussion.

Three theories of the cessation of the epizoötic of plague in the rat are possible: Firstly, that it dies out from lack of sufficient numbers of susceptible rats; secondly, that the infectivity of the plague organism decreases; and, thirdly, that some disadvantageous circumstance, such as unsuitable weather conditions, so far reduces the number of fleas as to determine the end of the epizoötic. The third theory presents great difficulties for the reason that the epidemic in man has its maximum about seventeen days later than the epizoötic in rats, and the epidemic in man is admittedly spread by fleas which are derived from the rat. The absence of a sufficient number of vermin can hardly be given as the reason for the decline of the epizoötic in rats without some attempt to explain why the same conditions affect rats and man differently.

The other two theories remain to be discussed. On the theory that the epizoötic dies out from want of a sufficient number of susceptible rats, it must be shown that statistically this is compatible with the facts. The data are simple: On the one hand, after the epidemic of plague is over, about a third to a half of the rats at least are found to be still susceptible to plague; secondly, the proportion of black rats affected is only about one-half that of the brown rats. As the evidence is in favour of equal susceptibility to plague in both species under like conditions of infection, this difference must be due to the facilities for infection being smaller in the case of the black rat than of the brown rat, and has been explained on the ground of the observed fact that the number of fleas on the brown rat is twice as great as that on the black rat. Either of the sets of facts regarding the black or brown rats might easily be explained on the theory that the epizoötic ends because the population of susceptible rats is not sufficient to maintain it. As far as I can see, however, at the present moment, the data regarding the two epizoötics are not easily adjusted so as to be mutually reconcilable. In the appendix I give a diagram (Diagram A) with a table of constants of the forms of epidemics which may occur on this hypothesis. The constants include the infectivity assumed, the values of β_1 and β_2, the number of susceptible units out of each initial thousand units remaining uninfected at the end of the epizoötic and the standard deviation. The form of the epidemic among the brown rats closely corresponds to that shown in the diagram as being due to an infectivity per infected unit of 1·6, the value of β_1 being in this case small, the value of β_2 3·58, approximately that given by the observations, namely, 3·50, and the proportion of susceptible units left uninfected, 30 per cent. of the original number, a number sufficiently close to the facts for all practical purposes. If it is now assumed that owing to inferior power of transmission of the disease a lower degree of infectivity exists among the black rats, and that the proportion of black rats affected is half that of the brown rats, the infectivity among the black rats must be in the neighbourhood of 1·2, which would satisfy the conditions—namely, β_1 0, β_2 3·16, and the percentage left 60. The epidemic in the black rat would however require to have a standard deviation in proportion to that of the brown rat of 8·4 to 3·2, whereas the actual values are respectively 2·53 and 2·45. Further, if the theory held, the epidemic in the black rat would culminate very much later than that in the brown, unless some other circumstance interfered to suddenly terminate the epizoötic. But such a sudden termination of the epizoötic implies great asymmetry, and the data are in direct opposition to the possibility of this great asymmetry. Closer approximations to the maxima of the two epidemics in time might be found by taking the infectivity among the brown rats as initially higher, but in this case β_1 and β_2 assume higher values, values quite out of the range of those found from the statistics.

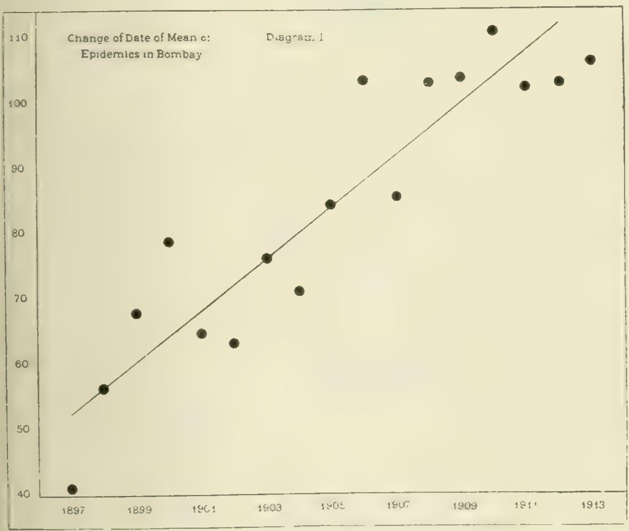
On the other hand, on the theory of the loss of infectivity on the part of the infecting organism the whole facts are easily explicable. It furnishes an explanation of the symmetry and also of the near synchronism of the two epizoötics, while the difference in the number of fleas provides a basis for expecting that the concentration factor already alluded to should be different in the epizoötics of the brown and black rat.

Periodicity in Plague Epidemics.

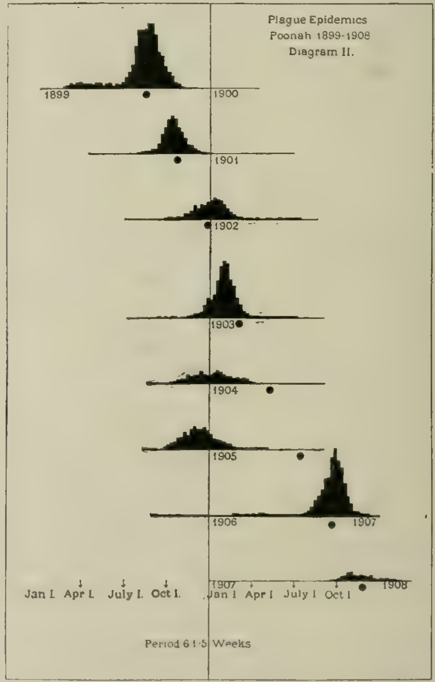
The form of the plague epidemics and epizoötics as existing in a number of large towns have thus been determined. The question as to whether these epidemics are in any way periodic must now be investigated before any estimate of the effect of elements such as the weather can be discussed. Such periodicities undoubtedly exist in many other diseases. Sometimes these periodicities conform exactly to the solar year, in which case no conclusions can be drawn. In other cases, however, the average interval between the epidemics is quite distinct from the solar year. Thus in Liverpool the two chief periodicities in the epidemics of measles are of lengths respectively ninety-two and one hundred weeks. In other places periods even closer to the solar year exist without there being any evidence that the periods are dependent on the year itself. This phenomenon is exceedingly common in plague, and is too obvious not to have received discussion, but it does not seem hitherto to have been regarded as possible that the change of the season of the epidemic was possibly due to some property in the life-history of the organism. Probably one of the reasons why more attention has not been paid to this subject is that in only a small number of cases are the data available for more than a short series of years, the present epidemic prevalence of plague dating back not much more than twenty-five years. In such places as Hong-Kong, in which the statistics exist since the year 1895, the periodicity seems almost exactly a solar year.

One of the first things, however, that strikes the observer with regard to the Bombay epidemics is that from the commencement of the epidemics in 1896 to the present day the date of the mean of the epidemics, though oscillating backwards and forwards, has been steadily becoming later and later in the year, though the difference in date of the mean during the seventeen years observed is little more than fifty days. This phenomenon is shown in the diagram (Diagram I). In this diagram the ordinates give the number of days from the commencement of February, at which the mean occurs, corresponding to the years given by the abscissæ. A straight line has been drawn through the observations to indicate their general trend.

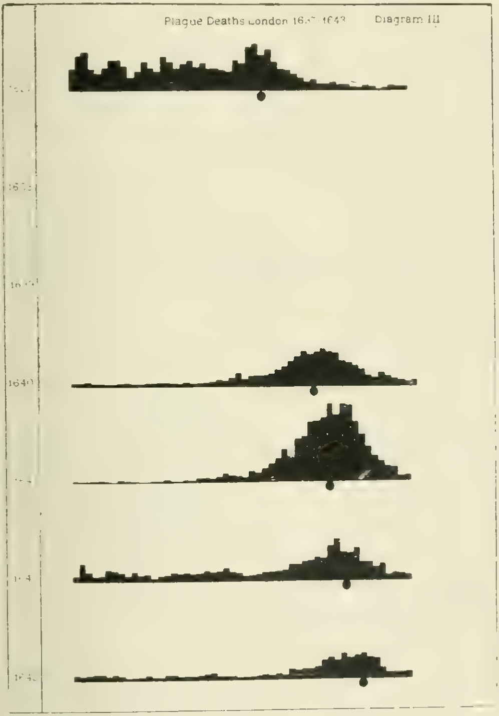
A further important instance refers to Poonah. In this case the period between the different epidemics is on an average somewhat more than two months greater than a year. The diagram (Diagram II) in this case shows the course of the plague epidemic in Poonah from 1899-1909. The point in each epidemic at which the mean of the epidemic might be expected to occur is indicated by a black circle. It would seem that the weather conditions in Poonah are such as to make an epidemic in the early months of summer nearly impossible. A break might be expected to occur at this point, and if the unfavourable circumstance present at this time did not result in the destruction of the special type of organism to which the series of outbreaks was due. it might also be expected that when the period of the life-history again coincided with a suitable season the epidemic would reappear. I think



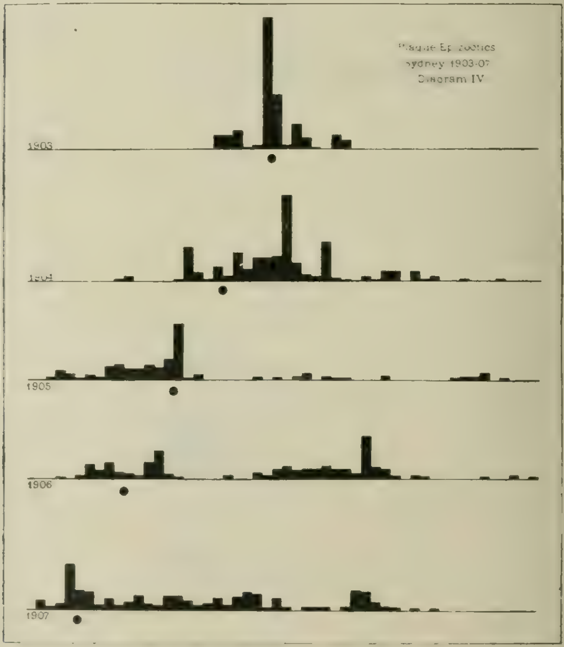
that the facts exhibited in this diagram confirm this view, and that the coincidence of fact with theory is too striking to be due to chance. The period in this case is almost exactly 61·5 weeks. The next example of the same phenomenon is chosen from older epidemiology. In the accompanying diagram (Diagram III) a similar phenomenon is shown for London for the years 1637-46. The statistics for two years are absent in this case, but the phenomenon for the years for which statistics exist are quite definitely explainable on this hypothesis. The same remarks apply also with regard to Cawnpore and Calcutta. The periodicity is not necessarily more than a year, but may be less, and this phenomenon is shown in the accompanying diagram (Diagram IV) which illustrates the progress of the epizoötic in rats in the town of Sydney, in which the epizoötic tended to occur earlier and earlier every year, while in this case it would appear as though there were a strain of the organism which tended to produce an epizoötic later and later.



The statistics in no case are sufficient to determine whether any of these periodicities are permanent or temporary. This requires much longer series of statistics. The most extensive investigations on this point I have been able to make relate to measles. With regard to this disease it is found that a period may be very regularly present for a series of eight or ten years, may then apparently disappear to reappear sometime later. Thus the epidemics expected to occur in London associated with the chief periodicity in that city—namely, that of ninety-seven weeks—have at different times been absent for periods of seven or eight years to reappear again in the place expected. In London there seem to be several strains of the organism associated with different periodicities and during the last seventy years epidemics due to one or other of these strains seem to take up the place of the chief epidemic for a time. The same kind of remarks apply to the

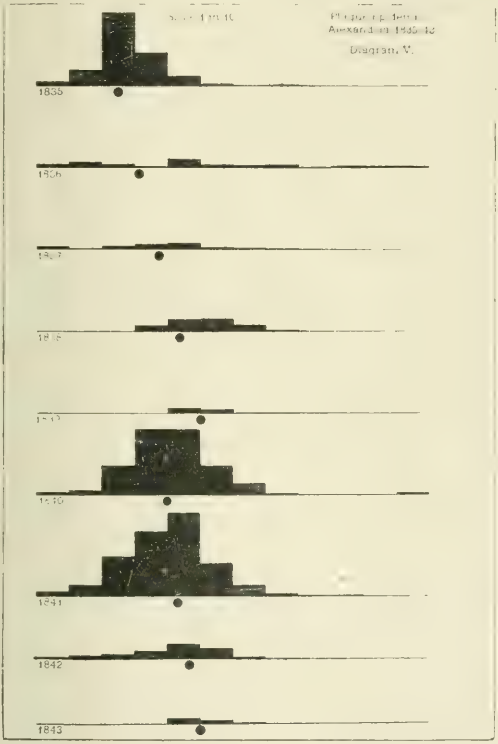


different towns in England with regard to measles. In consequence it is obvious that the permanent existence of such phenomena in relation to plague epidemics will only be established when the data for a much greater series of years than at present exist have been obtained.



There is another phenomenon of great importance which may be termed pseudo-periodicity. In this case an epidemic gets later and later in the year till a point in the year is reached at which the possibility of the disease breaking out becomes almost zero. At this epoch a throwback may occur and a second succession of epidemics follows only to arrive at a like *impasse*. An example of this from Alexandria for the year following 1835 is shown in the diagram

(Diagram V). In the second epoch the plague organism failed to throw back and the disease disappeared. Many such examples are found in the plague statistics of India.



Weather and Plague.

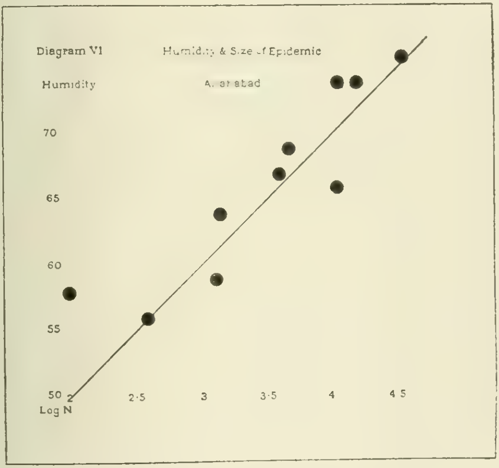
The relation of weather to plague must now be briefly touched upon, but anything like a complete discussion is not possible. There is no doubt at all that epidemics of plague may occur in almost any kind of weather. Severe epidemics have broken out in Europe during the coldest winters and, in certain parts of India, during the hottest and driest seasons, but such extremes must be considered the exception and not the rule. Both hot and cold weather undoubtedly limit the potentiality of the epidemic prevalence of plague. Within, however, a wide range of temperature, outbreaks of plague may occur. The limits cannot be accurately defined, but probably great epidemics of plague are possible in all zones between which the mean summer temperature ranges from 50° F. to 80° F.

More important than temperature seems to be humidity. Of the effectiveness of this there is no doubt. It is agreed that the reason for this is to be found in the bionomics of the flea. For the satisfactory development of the flea in all stages from the egg to the perfect insect a certain degree of humidity is necessary. In addition, fleas deserting a dying or dead host are able to live without food in humid weather for considerably longer than in dry weather. It is obvious therefore that, except under special conditions, it is unlikely that epidemics will occur at seasons of the year when the flea population is at a minimum. All this is sufficiently well recognized through the work of the Plague Commission and need not be further discussed.

Quantitative estimations of the relationship of humidity to temperature have not hitherto been made. In some districts, however, a very close connexion between plague and humidity can easily be shown. For this purpose the data given in the last Plague Supplement regarding towns and districts in the United Provinces have been used. Unfortunately weather data are only available for ten of the towns in these provinces and some of these towns are not of special size.

Contrary to the experience of this country, where there is practically no correlation between the humidity and rainfall of months which are not contiguous (for example the temperature and rainfall in the month of March are almost independent of those in January), in the United Provinces this want of correlation is not present. Thus the humidity of November is correlated very highly with that of the succeeding months and even at a distance of five months the correlation between the humidity of November and that of March is about 0·7. As epidemics occur in the United Provinces largely in the earlier months of the year it follows that this condition opens up a means of determining if there is any quantitative relationship between the humidity and amount of plague in the following spring. The humidity of November has been taken as the standard on account of the close association of this month with the humidity in the following months.

It is found that the logarithm of the size of an epidemic is in almost direct linear relationship with the degree of humidity. This is shown in the diagram (Diagram VI) for the town of Allahabad. This illustration is given for two reasons: Firstly, the town is of considerable size and during the period in question has never been free from plague; secondly, the disease has occurred essentially at the same season of the year during the period under review whether the epidemic was large or small. The difficulty due to variation of seasonal incidence does not therefore arise. The correlation between the logarithm of the amount of plague and humidity is in these cases very high, being in the neighbourhood of 0·9, so that for these towns the relationship is too close to be questioned. A like high correlation also exists for Cawnpore, the correlation coefficient being again 0·91. This instance, however, is not so good as that of Allahabad, as the interval between the different epidemics is not that of the solar year but rather greater. The correspondence found must therefore be questioned as partly, though not wholly, accidental. For Benares and Bahraich the correlation is nearly as high, while for the other four towns investigated, Agra, Lucknow, Bareilly and Manipur, the correlation varies from



0·45 to 0·60. In some of these cases a statistical difficulty arises which lowers the correlation. Working with a logarithm as one of the variables the occurrence of a year with no plague, or with almost no plague, results in one of the observations, when spotted in a diagram, lying so far distant from the other observations that the correlation is very seriously affected, though there is no other reason for believing that the regression is not linear. This occurs in Benares and in Bahraich especially.

The exact meaning of this high correlation is not quite clear. The correlation is nearly as high when any month up to March is chosen instead of November, but as already stated the humidity of November seems to determine the humidity for the next five or six months. It is probable in this case that the continued deficiency of humidity acts as a consistent force depressing the vitality of the flea.

TABLE V.—SHOWING THE VALUES OF THE CONSTANTS FOR FOUR PLAGUE EPIDEMICS IN ALLAHABAD.

	Number of deaths	μ_2	β_1	β_2	Date of mean
1902-3	14,411	2·03	0·144	2·87	52
1903-4	10,914	1·72	1·061	4·36	67
1904-5	33,374	2·46	0·190	3·35	53
1906-7	10,754	1·65	3·355	5·32	86

When the effect of a change of the weather on the course of an epidemic of plague is sought the difficulty must be faced that there is no means at present of determining the form the epidemic would take were the conditions favourable throughout. The effect has, however, been measured to some extent in one case. In four years the epidemics in Allahabad were of sufficient size to allow the constants to be determined with some certainty. These epidemics occurred in the years 1903, 1905, 1906 and 1907. In all four like conditions of humidity were present during the end of the preceding and the beginning of the epidemic year. Two differences, however, were observed. The fall in the humidity as the year went on, the normal phenomenon in Allahabad, was in some of the years more rapid than in others, and in two instances the mean of the epidemic was much earlier than that usually found. It must be noted that this variation of the date of the mean cannot at present be explained. The constants β_1 and β_2 were calculated for each of these epidemics. It is found on examination of the values of β_1 that there is an almost direct linear relationship between the date of the mean of the epidemic and the value of β_1. The earlier the epidemic occurs, the more its course lies in the period where the conditions of humidity are not distinctly different, the more symmetrical the epidemic is. If the epidemic occurs later in the year when the differences of humidity rapidly change in a manner adverse to the vitality of the flea the epidemic becomes more and more asymmetrical. The evidence from Allahabad therefore supports the idea that even in this district the typical epidemic would be symmetrical if it occurred a little earlier in the year and that the asymmetry is in this case due to the action of a specially inhibitory force acting to modify normal form. This view is rendered probable when it is considered that the symmetrical epidemics have occurred in neighbouring towns, such as Cawnpore, at seasons of the year when such epidemics might be reasonably expected.

In strong support of the view just advanced the statistics of Bombay may be taken. In this case, as has been shown before, the mean of the epidemic has been steadily advancing into the year since the epidemics first broke out. Associated with this change in the season of the year the size of the epidemics has been consistently decreasing though, as is only natural in a case like this, there has been oscillation in the size of the epidemic from year to year. Considering the question of the influence of fleas and considering also that with the passage of the epidemics to a later part of the year a more prolonged period of dryness and high temperature exists before the epidemic begins, it is natural to expect that the number of cases in the epidemic will also consistently decrease. It may, perhaps, also be said that though the size of the epidemics becomes consistently smaller even in the more recent years a more humid year is associated with a somewhat larger epidemic than a year when the air is drier ; the correlations however are very difficult to evaluate and any attempt to place this statement on a footing of rigid mathematics has been unsuccessful.

Relation between the Form and the Size of the Epidemic.

On the hypothesis that the decline of an epidemic is due to a diminution in the number of susceptible persons, the form of the epidemic is highly correlated with the number of cases. Reference to Table A in the appendix shows that as the percentage of those left uninfected at the end of the epidemic decreases the value of β continually increases and that a quite definite relation exists between the two quantities. Even if the method of calculation used be modified it is difficult to imagine that any other result would obtain.

On the second theory that the epidemic ends because of loss of infective power on the part of the infecting organisms as modified by the existence of a concentration factor it is also to be expected that a positive correlation will exist between β_2 and the total number of cases in the epidemic. There is however no essential connexion in this case as there seems to be no *a priori* reason why the concentration factor should not vary independently of the power of infection. The point must be settled by observation and not by theory. Reference to the table (Table VI) shows that out of the five instances examined, in three high positive correlation coefficients are found, and in the other two negative coefficients. Of the latter only one is of considerable size, but the fact that negative coefficients arise is of great interest inasmuch as it proves the absence of essential connexion between the form of the epidemic and the amount. The same result might possibly be obtained on the first theory if we imagined the epidemic hurried to a close by some other factor, but in that case the increase in the value of β_2 would be associated with a much higher degree of asymmetry than any found in the observations.

TABLE VI.—SHOWING THE CORRELATION BETWEEN β_2 AND THE TOTAL NUMBER OF DEATHS IN THE EPIDEMIC.

	Years	Correlation coefficients.
Poonah	1897-1906	0·805
Bombay	1898-1913	− 0·471
Belgaum	1897-1904	− 0·145
Hong-Kong	1893-1904	+ 0·636
Calcutta	1901-1909	+ 0·703

The Relation between Plague in Man and Plague in the Rat.

The relation of plague in man to that in the rat was investigated by Mr. Greenwood for the year 1905-06 in the city of Bombay by the method of the correlation coefficient, and the result he arrived at was that plague in man was more closely associated with plague in the black than with plague in the brown rat. While this result is undoubtedly correct for the year in question, its generality has not been borne out by observation in the subsequent years. Correlation coefficients have been calculated for each of the years 1907-11 between the number of deaths from plague in man and the number of deaths among the black and brown rats for the fortnight preceding. This interval has been chosen, as the date at which the maximum of the plague epidemic in man occurs is about seventeen days later than that at which the plague epidemic in the rat occurs. These coefficients are shown in the table (Table VII). In addition certain partial correlation coefficients have been evaluated. It will be noted that the correlation between plague in man and plague in the black rat is, on the whole, slightly lower than that between plague in man and plague in the brown rat; but there is little difference between either of these correlations and the correlation between the amount of plague in the two species of rat. Partial correlation coefficients have been calculated in the usual way by assuming in turn that the size of each epidemic or epizoötic remains constant, while those of the two others vary. These partial correlation coefficients vary in a sufficiently surprising manner. If anything can be deduced from them, it is that plague in man is related more to plague in the brown than in the black rat. I must confess, however, that the use of the method of partial correlation in this manner seems to me quite unjustifiable. As plague in man is due directly to infection from the rat in the great bulk of the cases, and as plague in man has no corresponding influence on plague in rats, such a mathematical application does not seem to have any root in fact. Examining specially the partial correlation coefficients between plague in man and the black rat, calculated on the hypothesis that the size of the epizoötic in the brown rat is constant, the coefficients are observed to vary through such a wide range as to lead to the conclusion that there is little real ætiological relationship between the epidemic in man and the epizoötic in the black rat. In this matter, as I am not disposed to admit the conclusion, the error must lie in the application of the method to a case in which it cannot properly be used.

TABLE VII.—SHOWING THE CORRELATIONS BETWEEN THE NUMBER OF DEATHS FROM PLAGUE IN MAN, AND THE NUMBER OF DEATHS AMONG BLACK AND BROWN RATS IN THE FORTNIGHT PRECEDING, BOMBAY, 1907-1911.

	Correlation Coefficients			Partial Correlation Coefficients		
	$r_{H.R.}$	$r_{H.D.}$	$r_{R.D.}$	${}_{D}r_{H.R.}$	${}_{R}r_{H.D.}$	${}_{H}r_{R.D.}$
1907	0·937	0·902	0·951	0·594	0·103	0·700
1908	0·890	0·968	0·873	0·363	0·859	0·106
1909	0·895	0·925	0·966	0·022	0·519	0·814
1910	0·600	0·855	0·877	−0·602	0·857	0·877
1911	0·851	0·972	0·855	0·164	0·897	0·225

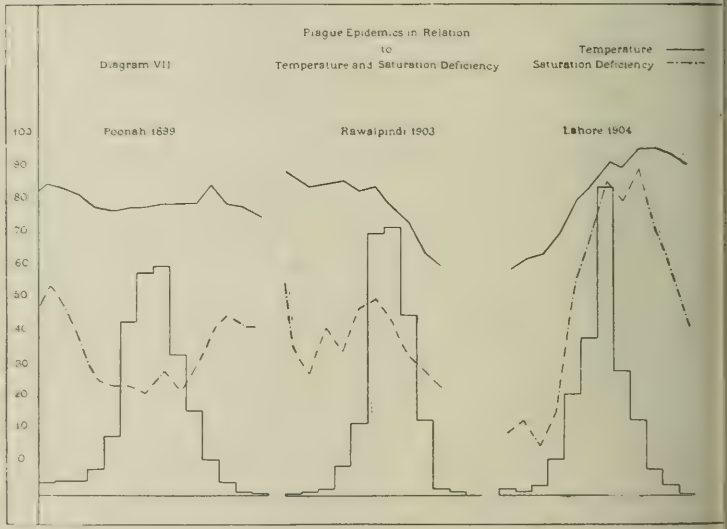
Apart from the reasons given, the discrepancies found may also have part of their origin in certain definite differences between the phenomena of the disease, as seen in man and as seen in the rat. Referring again to Table III, where the constants describing the epidemic in man and in the rat in the different districts of Bombay are given for the year 1905-06, it is found that the value of β_2 in man is consistently higher than in the rat, the values for man, the brown rat, and the black rat being respectively 4·40, 3·50, and 3·16. It is difficult to account for this by any mere direct relationship connecting the number of fleas harboured by the rat with the mode of spread. A higher value of β_2 in the epidemic in man than in the epizoötic in the rat might possibly arise if the fleas, on leaving the rat, had a progressively shorter life as the season advanced. This would, however, produce asymmetry in the curve describing the progress of the epidemic in man, a curve which is much more symmetrical than that in either species of rat. On the second theory the difference is quite easily explained, on the ground that the properties of the organism which permit the disease to spread in man are of a different quality from those which permit its spread in the rat, and that the concentration factor in relation to man is of more importance than in relation to the rat. This, again, is a point for observation, and not for theory. Referring again to Table III, in which, as before stated, it is shown that the correlation between the value of β_2 for the epizoötic in the two species of rat is 0·76, it is seen that the correlation of the same quantity with regard to the epidemics in man and in the black rat is only 0·20, and with regard to epidemics in man and in the brown rat is practically zero. It is difficult to see how such relationships can arise from purely statistical considerations, but there is no difficulty in imagining that local circumstances as regards housing, overcrowding, suitability for the preservation and transmission of fleas, &c., should have power to determine that the form of the epidemic in man should be very nearly independent of the form of that in the rat. In any case the higher value of β_2 suggests that man is more easily infected by plague than the brown rat, and still more easily infected than the black rat.

Brief Review of the Statistical Literature regarding Plague.

It is now possible to review briefly the statistical conclusions deduced by previous workers on plague. The most important work from this point of view is that of Mr. Greenwood, issued in a series of reports by the Plague Commission. Mr. Greenwood comes to a considerable number of conclusions which are of epidemiological importance. He finds that endemic plague is chiefly associated with large towns, and that the smaller the town the more probable it is that outbreaks of plague will only occur at intervals. Further, he finds that there is a considerable degree of association between the size of a town into which plague has been introduced and the number of deaths which occur, a greater number of deaths occurring in small towns than in large towns in proportion to the number of inhabitants in each case. It may be stated that I think this is a necessary conclusion from my theory of the epidemic, but I have not at present worked out the mathematics, and hesitate to posit this definitely. Mr. Greenwood also finds that certain localities seem to be in some way protected against plague, while others seem more susceptible to infection by the disease than might be expected from the theory of chance. He also discusses the method of entry, and shows that frequency of infection is associated with lines of commerce, railways or other, and comments on the mode in which the disease establishes a footing, questions which are of fundamental importance, but on which I have not touched. Though Mr. Greenwood comes to the conclusion that in any individual region the size of the epidemic cannot be correlated in any one year with any definite weather conditions, he is also quite convinced that certain conditions of humidity and temperature are almost essential to the spread of the disease. This conclusion is apparently received by all his fellow-workers, and is probably true with few exceptions. Mr. Greenwood does not, however, touch on the form of the epidemic except in individual instances, chiefly relating to epidemics in the districts of Amritsar and Gujrat, but he comes to the conclusion that the factors which cut an epidemic short are more determinate than those responsible for its commencement, an inference with which I take leave to differ. I am doubtful if district curves can be advantageously used in the first place to determine the forms of epidemics. The conditions are not sufficiently simple, but that is a question which must be left for future discussion.

The last Plague Supplement, the tenth, issued in May, 1917, contains an important paper by Major Gloster and Major White, in which some statistical investigations, mostly of a graphical kind, are made into the conditions under which plague spreads in the United Provinces in India. The conclusion arrived at by the authors is that humidity has a fundamental relation to the epidemic prevalence of plague; specially they state that the humidity during the winter months is so regularly associated with severe epidemics of plague that they are fully justified in concluding that one stands to the other as cause and effect; further, they come to the conclusion that this effect is exercised mainly through the influence of humidity on the length of the life of the flea when separated from its natural host.

This supplement also contains a paper by Dr. Brookes on the influence of humidity and temperature on plague. Dr. Brookes introduces a new physical constant which he terms the saturation deficiency, a constant which expresses the drying power of the air rather than the humidity, and which may prove of great importance in future work. He advances the view that when the saturation deficiency increases to 0·3 and the temperature to 80° F. the epidemic tends to cease. He completely fails, however, to examine the course epidemics take in what he might consider ideal conditions for the spread of plague, a necessary preliminary to any such investigation as that which he has undertaken. His work is thus greatly invalidated.



I have calculated the saturation coefficient for a number of instances, and of these three are illustrated in the accompanying diagram

(Diagram VII). It thus seems that epidemics of plague similar not only to appearance but also in the value of their mathematical constants, can occur under three completely different conditions of temperature and humidity. It is specially to be noted that in the case of the epidemic in Poonah the epidemic rises to its maximum and thereafter falls during a period when both the temperature and humidity are almost constant.

In conclusion, it may be stated that I do not think the requisite data exist at present to establish any but the roughest association between outbreaks of plague and weather conditions. To determine fine associations will require not merely more data but also mathematical treatment on lines not hitherto suggested.

APPENDIX.

In the previous pages the results obtained on various theories of epidemiology by Sir Ronald Ross and myself have been referred to and compared with the series of statistics relating to the epidemics of plague during the last three centuries. For the convenience of those readers who desire to test the conclusions arrived at without special search through the literature I have tried to compress into a short note the present state of the mathematical theory. The methods are at present very incomplete and the solutions of the equations have hitherto only been obtained in a few simple cases. Two methods of approach to the subject have been used. In one of these the ordinate of the epidemic curve, which represents the number of new cases occurring per unit of time, is taken as the dependent variable, time being the independent variable: in the other the sum of all the cases which have occurred up to the instant examined is taken as the independent variable. The two methods are complementary.

For convenience the same terminology will be applied to both methods throughout the following notes. Let the epidemic curve be denoted by the equation $y = f(t)$ the number of new cases at any instant is thus ydt or $f(t)dt$. If x be the sum of all the cases that have taken place up to time t then x is equal to $\int_0^t ydt$ or $\int_0^t f(t)dt$ while $\frac{dx}{dt}$ is obviously equal to y. Consider first the solutions given in terms of the ordinate y. The infectivity at any moment may be defined as the ratio of two ordinates of the epidemic curve taken at such intervals of time as seem convenient (practically this is the incubation period). This ratio may then be assumed to be some function of the time and of the ordinate. An equation of the following form thus arises:—

$$\frac{y_2}{y} = \phi(t, y)$$

Using the notation of finite differences this becomes :—

$$1 + \frac{\Delta y}{y} = \phi\,(t,\, y)^{(\Delta t = 1)}$$

This law may be assumed to hold for the interval of time denoted by dt so that in the notation of the differential calculus :—

$$1 + \frac{dy}{y} = \phi\,(t,\, y)^{dt}$$

Expanding the function on the right-hand side by the ordinary method,

$$1 + \frac{dy}{y} = 1 + dt \log \phi\,(t,\, y) + \frac{dt^2}{1 \cdot 2} \left\{ \log \phi\,(t,\, y) \right\}^2 + \ldots$$

Proceeding to the limit this gives finally :—

$$\frac{1}{y}\,\frac{dy}{dt} = \log \phi\,(t,\, y) \;\ldots\; (1)$$

The interpretation of this equation is comparatively simple. It means that ydt measures the whole amount of infecting material at the moment, and that the number of persons newly infected bears a relation to this amount defined by the function $\phi(t,y)$. The method as developed apparently requires that the incubation period of the disease should be infinitesimal, which, of course, never occurs in practice. This, however, is not necessary in all cases. The problems which can be most easily solved by this method are those in which the infecting organism is assumed to have its infectivity described according to some law, generally in terms of the time but not necessarily so.

Theorem I.—In the first place let it be assumed that the number of persons infected during an epidemic is so small in relation to the total population that the supply of susceptible units never becomes appreciably less, such a case, for instance, as in the epidemic of small-pox in London in 1902, when out of 6,000,000 persons only 6,000 contracted the disease. The simplest case on this hypothesis is that in which the infecting power of the organism decreases with time so as to be described by the terms of a geometrical progression In other words the organism loses power of infecting according to the law of the uni-molecular reaction.

The infectivity is thus denoted by $e^{p - qt}$.

From equation (1)

$$\frac{1}{y}\,\frac{dy}{dt} = \log e^{p-qt}$$

$$= p - qt \qquad \ldots\ldots (a)$$

which when integrated gives

$$y = ce^{pt - \frac{1}{2}qt^2} \qquad \ldots\ldots (b)$$

or the normal curve of error.

Corollary I.– The method by which the above result has been obtained requires that the rate at which the cases increase is directly proportional to the number of new cases occurring at that moment. This, however, need not be true. It may be assumed as possible that the greater the concentration of new cases the greater the action. Thus the relationship which is found to exist between density of population and the death-rate is such that the death rate varies as the tenth root of the density. If the same law be assumed to hold in this case a second factor in the form y^n must be introduced. Making this change according to the analogies of physical chemistry the infectivity at any moment is of the form $cy^n(p-qt)$

giving an equation $$\frac{1}{y}\frac{dy}{dt} = cy^n(p-qt) \quad \ldots\ldots \text{(c)}$$

which gives the epidemic form as

$$y = \frac{1}{\{n(c-pt+\tfrac{1}{2}qt^2)\}^{\frac{1}{n}}}$$

which, as is easily seen by changing the origin, is equivalent to Pearson's Type IV.

$$y = \frac{M}{\left(1-\frac{t^2}{a^2}\right)^m} \quad \ldots\ldots \text{(d)}$$

Corollary II.—On the other hand it may be assumed that in the presence of an epidemic disease a certain amount of partial immunization takes place owing to slight exposures to the disease. In this case a factor of the same nature y^n may also be assumed, n however, being a small negative quantity since increase in the concentration will tend on this hypothesis to make the population insusceptible. Taking n negative the equation is in the same form as that above but the solution of the epidemic form is now,

$$y = M\left(1-\frac{t^2}{a^2}\right)^m \quad \ldots\ldots \text{(e)}$$

Theorem II.—In the preceding theorem and its two corollaries it has been assumed that the total number of cases of a disease bears a sufficiently small proportion to the total population to afford a practically uniform supply of susceptible units. This factor must, however, be taken into account: for simplicity the infectivity is assumed constant. Taking P as the total population of susceptible persons and x as the total number of cases up to any instant then it is obvious that, if κ be the number of persons infected by one case at the beginning of the epidemic, at any subsequent time one case will affect on the average $\kappa\left(1-\frac{x}{P}\right)$ since the possibility of infection has decreased in the ratio $P-x$ to P.*

If the infectivity of the organism κ remain constant we have from (1)

$$\frac{1}{y}\frac{dy}{dt} = \log \kappa\left(1-\frac{x}{P}\right) \quad \ldots\ldots \text{(g)}$$

This equation cannot be integrated so as to express y in terms of t. It is, however, easily integrated and the solution is given later (Theorem VII).

Method II.—Turning now to the second method, that introduced by Sir Ronald Ross, it may be noted that this method is essentially the same as that used in many equations in physical chemistry and that the reasoning on which the equations are formed in the two cases is essentially the same. In this case the total number of cases occurring up to any instant is denoted by x and the rate at which new cases occur by $\frac{dx}{dt}$. Relationships between these two quantities are then assumed. This method is specially suitable in examining the manner in which the limitation of the number of susceptible persons may act in determining the course of an epidemic. Thus if the number of susceptible persons at the commencement of an epidemic be denoted by P at any subsequent time the number will be $P-x$. Any law of infecting may be assumed introducing y, x, or t, so that the equation finally takes the form.

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* Sir Ronald Ross arrives at the same result by considering that the "happenings" fall alike on the uninfected and on those already infected. In the latter case the "happening" is wasted.

$$\frac{dx}{dt} = \phi\,(y, x, t)\,(P-x) \qquad \ldots\ldots (2)$$

Theorem III.—The first case to which this hypothesis is applied by Sir Ronald Ross is that in which the infecting agent is distributed uniformly. The probability of infection at any instant thus remains in a constant ratio to the number of susceptible persons. The equation (2) thus takes the form,

$$\frac{dx}{dt} = \kappa\,(P-x) \qquad \ldots\ldots (h)$$

which gives,

$$x = P\,(1-e^{-\kappa t}) \qquad \ldots\ldots (i)$$

This equation closely describes the process of disinfection as has been shown by Miss Chick. It probably also represents such a phenomenon as the rate at which numbers of a battalion at the front, exposed to a continual atmosphere of gas by the enemy, are sent to the rear.

Corollary.—This case can easily be extended to that in which the population is increasing uniformly at a definite rate. Such an instance may be considered given by the Royal Society, to which every year fifteen members are added. If the rate of death and resignation combined be assumed constant and equal to κ the equation assumes the form,

$$\frac{dx}{dt} = \kappa\,(a+pt-x) \qquad \ldots\ldots (j)$$

where $(a+pt-x)$ represents the number of members of the Society at any time, and where p represents the number added per annum. The solution of this equation,

$$a+pt-x = \frac{p}{\kappa} - \left(\frac{p}{\kappa}-a\right)e^{-\kappa t} \qquad \ldots\ldots (k)$$

shows that within a few years of the establishment of the society the number of members becomes constant and equal to $\frac{p}{\kappa}$ a result immediately obvious without calculation.

Theorem IV.—In the second case discussed Sir Ronald Ross assumes that all those who have taken the disease up to any point remain equally infectious during the whole epidemic. This leads to the equation,

$$\frac{dx}{dt} = \frac{\kappa x}{P}\,(P-x) \qquad \ldots\ldots (l)$$

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[*] The equations of Sir Ronald Ross are given in this paper in a different form from that in which they were originally expressed. By a very ingenious process Sir Ronald Ross arrived at an equation identical in form with that given in Theorem IV. His x, however, is the ratio of the number of infected cases to a population which is assumed to vary. I think it better not to work with this ratio but to use x as the actual number of cases that have occurred up to any moment. This allows changes in the different conditions, such as the number in the population, to be introduced with less chance of error in the mathematics.

It is better, however, to write the infecting factor as $(a+x)$, a being the amount of introduced infection at the beginning of the epidemic. Thus we have,

$$\frac{dx}{dt} = \frac{\kappa(a+x)(P-x)}{P}$$

From this, $$dx\left(\frac{1}{a+x}+\frac{1}{P-x}\right) = \kappa\frac{a+P}{P}dt$$

or, $$\log\frac{a+x}{P-x} = \kappa\frac{a+P}{P}t+C$$

As $x = o$ when $t = o$, $C = \log\frac{a}{P}$, which gives the form

$$\log\frac{P(a+x)}{a(P-x)} = \kappa t\frac{a+P}{P} = \kappa t \text{ since } \frac{a}{P} \text{ is small,}$$

whence it is found, $$x = \frac{aP(e^{\kappa t}-l)}{P+ae^{\kappa t}} \qquad \ldots\ldots \text{(m}$$

so that $$y = \frac{dx}{dt} = \frac{a\kappa P(P+a)e^{\kappa t}}{\left(P+ae^{\kappa t}\right)^2} \qquad \ldots\ldots \text{(n)}$$

the equation of the curve of the epidemic. This curve is symmetrical and has the moment relationships of Pearson's Type IV.

Theorem V.—Sir Ronald Ross considers next the case in which the individuals who have taken the disease cease to be infective and become again susceptible, the rate at which this change takes place being proportional to the number of infected persons at any time. If the number of reversals be denoted by ϕ and the rate at which they are formed be denoted by λ, the following equations can at once be written down,

$$\frac{dx}{dt} = \kappa(a+x-\phi)(P+\phi-x) \qquad \ldots\ldots \text{(o)}$$

$$\frac{d\phi}{dt} = \lambda(a+x-\phi) \qquad \ldots\ldots \text{(p)}$$

Subtracting the second from the first an equation in the same form as that given in the preceding paragraph arises, of which the solution is,

$$a+x-\phi = \frac{a(\kappa P+a-\lambda)e^{(\kappa P+a-\lambda)t}}{\kappa ae^{(\kappa P+a-\lambda)t}+\kappa P-\lambda} \qquad \ldots\ldots \text{(q)}$$

Differentiating this relation and replacing $\frac{d\phi}{dt}$ by the expression obtained by substituting the value just found for $a+x-\phi$ in equation (p) we have,

$$\frac{dx}{dt} = y = \frac{le^{ct}}{(n+e^{ct})^2} + \frac{me^{ct}}{n+e^{ct}} \qquad \ldots\ldots \text{(r)}$$

where
$$c = \kappa(P+a)-\lambda$$
$$l = \frac{c^2}{\kappa}\left(\frac{c}{\kappa a} - 1\right)$$
$$m = \frac{\lambda c}{\kappa}$$
$$n = \frac{c}{\kappa a} - 1$$

This expression for y is made up of the symmetrical epidemic curve $\frac{le^{ct}}{(n-e^{ct})^2}$ with an added portion. For certain ranges of values the epidemic curve may, as Sir Ronald Ross remarks, be dwarfed by the second part. The value of y approaches a constant limit with time as is obvious from general considerations.

Theorem VI.—In this case it is assumed that a person once infected remains immune during the continuance of the epidemic. It is further assumed that the units infected lose power of infecting according to the law of the uni-molecular reaction. Denoting the total loss of infecting power by ϕ and the rate at which the infected units lose infectivity by λ, the equations can at once be written down—

$$\frac{dx}{dt} = \kappa(x-\phi)(P-x) \qquad \ldots. \quad (s)$$

$$\frac{d\phi}{dt} = \lambda(x-\phi) \qquad \ldots. \quad (t)$$

The matter may, however, be looked at a little differently. As the number of persons infected at any time is y and as these lose their infecting power at the rate described by λ the infecting power of those developing the disease at any time T will be at time t

$$y_T e^{-\lambda(t-T)} dT$$

so that at time t the total amount of infecting power will be,

$$\int_o^t y_T e^{-\lambda(t-T)} dT$$

this gives at once the equation,

$$\frac{dx}{dt} = \kappa \int_o^t y_T e^{-\lambda(t-T)} dT\,(P-x) \qquad \ldots. \quad (u)$$

The two equations (s, t) and (u) lead directly to the same equation. Dividing the first equation (s) by $P-x$ and differentiating, we have,

$$\begin{aligned} \frac{d}{dt}\left\{\frac{1}{P-x}\frac{dx}{dt}\right\} &= \kappa\frac{dx}{dt} - \kappa\frac{d\phi}{dt} \\ &= \kappa\frac{dx}{dt} - \kappa\lambda(x-\phi) \\ &= \kappa\frac{dx}{dt} - \frac{\lambda}{P-x}\frac{dx}{dt} \qquad \ldots. \quad (v) \end{aligned}$$

Whence we have integrating,

$$\frac{1}{P-x}\frac{dx}{dt} = C + \kappa x + \lambda \log(P-x)$$

If when, $x = o$, $\frac{dx}{dt} = o$, $C = -\lambda \log P$

whence, $t + c = \int \frac{dx}{(P-x)\left\{\kappa x + \lambda \log\left(1-\frac{x}{P}\right)\right\}} \qquad \ldots. \quad (w)$

This integral cannot be used directly on account of its form. Choosing, however, suitable values of λ and κ the form of the epidemic can be easily though laboriously calculated. If an integraph is at hand, however, the process is not difficult. The necessary calculations have been made and it is found that curves of Type IV result, not however symmetrical, but with the ascent more rapid than the descent, a curve of a form somewhat different to that which might have been expected from a consideration of the result in Theorem IV. When κ approaches λ in value the curve becomes symmetrical and closely approximates to the normal curve of error.

Theorem VII.—Let it be assumed that the period of infecting of an individual who develops the disease is very short in comparison with the length of the epidemic. Such a condition as this is seen in measles where children who have taken the disease are excessively infective during the first three days of illness, but as they are usually confined to their homes after the rash has appeared do little further damage. In this case writing the equation as in the previous paragraphs we have,

$$\frac{dx}{dt} = \kappa y(\mathrm{P} - x) \qquad \ldots\ldots \text{(x)}$$

It is obvious since $\frac{dx}{dt} = y$ that a limiting case has been reached to which the method no longer applies. It is necessary to go back to Theorem II. Taking the equation there given, namely—

$$\frac{1}{y}\frac{dy}{dt} = \log \kappa \left(1 - \frac{x}{\mathrm{P}}\right) \qquad \ldots\ldots \text{(g bis)}$$

and noting that $y = \frac{dx}{dt}$ the solution can be written at once and is as follows,

$$\frac{d^2x}{dt^2} = \log \kappa \left(1 - \frac{x}{\mathrm{P}}\right)\frac{dx}{dt}$$

which gives, $\frac{dx}{dt} = \mathrm{C} + x(\log \kappa - 1) - (\mathrm{P} - x)\log\left(1 - \frac{x}{\mathrm{P}}\right)$

if when $x = 0 \quad \frac{dx}{dt} = 0 \quad \text{then} \quad \mathrm{C} = 0$

therefore, $t + c = \int \frac{dx}{x(\log \kappa - 1) - (\mathrm{P} - x)\log\left(1 - \frac{x}{\mathrm{P}}\right)} \quad \ldots\ldots \text{(y)}$

In this case again t is determined as an integral of x. Using the integraph as already described it is found that in this case the descent of the epidemic is more rapid than the rise in contrast to what has been seen in the previous theorem.

A series of curves of this form have been calculated arithmetically by finite differences and are illustrated in the accompanying diagram (Diagram A). The values of the chief constants of these curves are given in the accompanying table (Table A). These values have been discussed in the body of the paper and do not require any further remark. The forms of the curve calculated by arithmetic very closely resemble those obtained from the differential equation.

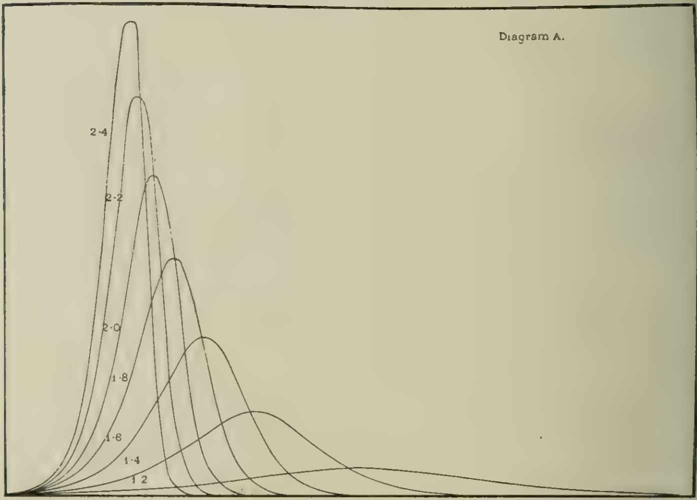


Table A.—Showing the Values of the Infectivity of β_1 and β_2, and also of the Standard Deviation σ, and of the Percentage Number of Units left uninfective on the Hypothesis that an Epidemic has a Constant Infectivity, and ceases because the Number of Susceptible Persons is too few to permit the Further Extension of the Disease.

Infectivity per unit of time	β_1*	β_2	σ	Percentage uninfected	Mean of epidemic from beginning
1·2	− 0·011	3·15	8·38	67·0	22·14
1·4	− 0·030	3·33	4·58	46·3	15·28
1·6	− 0·090	3·58	3·22	31·3	12·14
1·8	− 0·183	3·74	2·51	20·7	10·32
2·0	− 0·383	3·93	2·13	13·0	9·06
2·2	− 0·518	4·21	1·71	7·2	8·22
2·4	− 0·806	4·46	1·48	3·7	7·57
2·6	− 1·140	4·73	1·31	0·9	7·05

* The minus sign signifies that the decline of the epidemic is more rapid than the rise.

The values given in this table, on account of the method of approximation, must only be taken as correct to the first two figures.

Note.—When the results of the last two theorems are compared it seems very probable that with some law of loss of infection lying between those considered symmetrical curves may be obtained.

Theorem VIII.—The effect of modifying factors now can be discussed. Returning first to Theorem I in which the infectivity of the organism disappears according to the law of the uni-molecular reaction, let us suppose that some condition tending to the progressive increase in the chances of infectivity is introduced. Such a factor, for instance, as $a + bt$ may be placed against the formula which describes the infectivity at any moment of time. The resulting equation is,

$$\frac{1}{y}\frac{dy}{dt} = \log (a + bt) e^{p - qt}$$

$$= \log (a + bt) + p - qt \qquad \ldots . \quad (z)$$

giving on integration the form of the epidemic as,

$$y = C(a + bt)^{\frac{a}{b} - t} e^{(p-1)t - \frac{1}{2}qt^2} \qquad \ldots . \quad (A)$$

This curve is asymmetrical and the descent of the epidemic is more rapid than the rise. This means that the maximum of the epidemic occurs later and as the infectivity is largely lost by this time the descent is rapid. Similarly it may be found by applying a factor $\frac{1}{a + bt}$ that when some progressively inhibiting influence is at work the form of the epidemic is so changed that the decline of the epidemic is less rapid than the rise. Such factors must be considered where the influence of such a condition as varying temperature on the progress of an epidemic is examined, a subject as yet hardly worked at. It may be noted that in the case of the normal curve an exponential increasing or inhibiting factor alters the position of the maximum without changing the form of the epidemic.

Theorem IX.—So far the population has been considered constant. Let, however, the number of susceptible persons per unit of time uniformly increase and equation (2) may be written,

$$\frac{dx}{dt} = \kappa f(x) (P + pt - x) \qquad \ldots . \quad (B)$$

In Sir Ronald Ross's case (Theorem IV) $f(x) = x$: this equation being a modification of Riccati's equation is immediately integrable, so that we have,

$$y = \frac{(P + pt)T}{B + \int T dt} - \frac{T^2}{\kappa \{B + \int T dt\}^2}$$

where $T = e^{\kappa Pt + \frac{\kappa pt^2}{2}}$ and B is a constant.

The form of this equation indicates that the fall of the epidemic is more rapid than its rise.

Theorem X.—Returning again to Theorem II and introducing an element in the form of an increasing population we have the equation,

$$\frac{1}{y}\frac{dy}{dt} = \log \kappa (P + pt - x) \qquad \ldots . \quad (C_1)$$

or say, $$= \log \kappa z \qquad \ldots . \quad (C_2)$$

With a few simple transformations:—

$$-\frac{\frac{d^2z}{dt^2}}{p-\frac{dz}{dt}} = \log \kappa z$$

Denoting $\frac{dz}{dt}$ by R this takes the form,

$$-\frac{R\frac{dR}{dz}}{(p-R)} = \log \kappa z$$

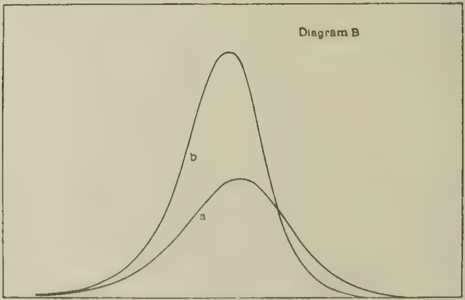
In this form the variables are separable and the first integral is obtained in the following form,

$$\frac{dz}{dt} + p\log\left(p-\frac{dz}{dt}\right) = C + z\log z + z(\log\kappa - 1)$$

$$\text{or} \quad (p-y) + p\log y = C + z\log z + z(\log\kappa - 1) \qquad \ldots . \text{(D)}$$

From (D), values of y can be obtained for each value of z. For each corresponding value of y and z, $\frac{dy}{dt}$ can be obtained from equation (C_2). Plotting the corresponding values of y and $\frac{dt}{dy}$ on squared paper and using the integraph we find that the form of the epidemic is again one in which the fall is more rapid than the rise and this to a greater extent than that found in Theorem VII.

For the value $\kappa = 1·5$ and p equal respectively to zero (Diagram B, *a*) and to 0·0015 per unit of the population (Diagram B, *b*) the forms of the curves of the two epidemics are shown. It is to be noted that in this case also, an adjuvant factor in the form of an increasing number of susceptible individuals leads to greater asymmetry, the epidemic ending more rapidly than in the case where no increase in the numbers of susceptible persons takes place.



Theorem XI.—All the previous equations in which the left hand of the equation is $\frac{dx}{dt}$ or $\frac{1}{y}\frac{dy}{dt}$ can be easily modified to permit of the infection varying as some function of the time, $f(t)$, if t does not occur explicitly on the right hand of the equation. Thus Theorem IV becomes,

$$\frac{dx}{dt} = f(t)\, x \left(1 - \frac{x}{P}\right)$$

It is evident that the division of both sides by this function $f(t)$ leads to dt being replaced by $f(t)dt$. All that is required to obtain the new solution is to substitute the $\int f(t)dt$ for t in equation (n). In most cases the formula will be so complicated that the determination of the constants becomes nearly impossible, but there is nothing difficult in the mathematical treatment.

Theorem XII.—Within small ranges of variation of temperature it is found in physical chemistry that if T be the absolute temperature the rate at which a reaction takes place at different temperatures varies approximately as e^{nT}.* As T in the case of the average temperature at any place is approximately a sine function this can be written in the form of

$$e^{a+b\cos(t-\phi)}$$

As the temperature varies therefore the rate of growth of an organism might be assumed to be expressed by,

$$e^{a+b\cos(t-\phi)}$$

where ϕ is a constant which determines the epoch of minimum or maximum temperature.

If the number of new cases thus depend upon this rate we can write

$$\frac{dx}{dt} = y = e^{a+b\cos(t+\phi)}$$

This expression is easily enough fitted to any series of statistics. All that is necessary is to take the logarithm of the number of cases at each interval of time and fit to a harmonic series. If the expression describes the data the coefficients of all the harmonics except the first should be nearly zero.

A very typical example of the correspondence between the formula and the facts is afforded by the number of epidemics of sheep scab in Ireland, 1900-1905: the figures and the formula are given in the accompanying table (Table B):—

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* As has been seen in the paper a similar formula expresses the relationship of some plague epidemics to humidity.

TABLE B.—EPIDEMICS OF SHEEP SCAB IN IRELAND, 1900-1905.

	Actual number	Theoretical number
January	745	709
February	572	628
March	363	390
April	207	194
May	104	93
June	55	52
July	37	40
August	39	45
September	75	73
October	160	147
November	331	308
December	505	544

Formula.

$$\begin{aligned}\log_{10} N = 2{\cdot}228 &- {\cdot}509 \cos x + {\cdot}012 \cos 2x + {\cdot}015 \cos 3x \\ &- {\cdot}364 \sin x - {\cdot}034 \sin 2x - {\cdot}019 \sin 3x\end{aligned}$$

where N is the number of epidemics in each month and the origin of x is in the middle of the month of June.

There are, however, great difficulties in interpretation: it is found, for instance, that the expression describes closely the number of notifications of enteric fever in London; when, however, the data for Australia are examined it is found that a simple sine curve fits the form much better. Again with regard to pneumonia in Australia the logarithmic fitting is exceedingly good, yet the simple sine curve applies nearly exactly to the phenomena observed in Glasgow.

The method by which Sir Ronald Ross tackles this part of the subject is to assume periodicity in the ordinate and to work back to the laws under which this may be produced. This is not a very satisfactory method. It is, in addition, difficult to interpret the results. Those who desire fuller information must refer to Sir Ronald Ross's own memoirs

CONCLUSIONS.

Various conclusions may be deduced from the remarks in the preceding pages. In the first place it is to be noted that many completely different hypotheses relating to the infectivity of the organism, to the numbers of susceptible persons, and to the relation of those who take the disease to the number infected at any previous time, lead to equations which though not mathematically identical represent curves indistinguishable statistically. Thus it may be noted that the equation found in Theorem IV describes an epidemic form which is nearly identical with that found by Theorem I, Corollary I. Even supposing an epidemic ran rigidly according to the laws of infection used to develop the respective theories, a requirement which no epidemic can be expected to fulfil, it would be impossible to distinguish between the curves referred to unless the number of cases in the epidemic were at least 12,000 : such numbers are very unusual.

In addition, it is to be noted, that the hypothesis underlying Theorem IV, namely that every infected person remains a source of infection from the date of infection to the end of the epidemic has no counterpart in fact, yet gives a form of curve which closely describes many epidemics. After working at this subject for a number of years I am quite convinced that nothing can be deduced as to the reasons for the definite course which epidemics pursue from a consideration of the curves of the epidemics themselves.

To distinguish true from false theory such extra details as have been discussed in this paper with reference to plague in the brown and black rat will have to be carefully considered. In this case we find that the amount of the disease is distinctly different in the two species, and yet the form of the epidemic in both cases is essentially the same. On the theory that an epidemic dies out from want of susceptible units this can only be explained so far as I can see if the incubation period of the disease in the black rat is about half that in the brown rat, whereas such evidence as is available points to the conclusion that it is rather longer. The method of spread of disease, when sufficient data have been collected, should throw light upon the subject, but in this case also the difficulty of disentangling the different threads will probably be very great.

DISCUSSION.

Mr. M. GREENWOOD: In my report of 1911[1] I think I was able to prove that although small villages were less likely to be attacked by plague than large ones, yet, when attacked, they tended to suffer with especial severity. I also showed that the form of the relation between size and rate of mortality could be mathematically described on the assumptions, (*a*), that the number of initial foci, perhaps colonies of infected rats, increased more slowly than the absolute population; and (*b*), that the actual density of population increased with size to a critical value. These suppositions and the resultant mathematical form are congruent with, and partly explained by, Dr. Brownlee's hypothesis. With respect to climatic conditions, I am in general agreement with Dr. Brownlee. I had then only data of rainfall and temperature, and concluded that *intra-seasonal* variations of these were either not factors or were but trivial factors in comparison with more general determining agencies of mortality. If one takes in any district a series of, say, Aprils, and uses as variables, deaths from plague and rainfall or temperature, these variables are not found to be significantly correlated. What little correlation there is, could, if desired, be explained upon Dr. Brownlee's lines as due to cross association. On the other hand, if one considers the progress of an epidemic *through any one season*, the following results are suggestive. I assumed that the beginning of the epidemic might be regarded as the tail of a normal distribution and deduced therefrom the constants of the whole symmetrical distribution of which it, by hypothesis, formed a part. With this artificial frequency I compared the actual epidemic distribution. The result was that the ratio of observed to actual cases within each time unit (the unit was three weeks, the data those of Amritsar district 1906-7) fluctuated round unity until one reached the epoch at which the mean temperature exceeded the annual mean by 10 or 12 : then, for a brief period, the ratio exceeded unity, thereafter diminishing very rapidly. The suggestion—I do not put it higher—is that the epidemic law may be ultimately reducible to two main components: (1) a symmetrical function, perhaps a normal function, dependent upon the life history of the organism; (2) another symmetrical function, not a normal function, descriptive of the meteorological sequence and perhaps a mirror of the changes in the insect carrier. Here, again, is nothing incompatible with Dr. Brownlee's main proposition. With respect to the choice of data for testing epidemiological laws, I am unconvinced by Dr. Brownlee's arguments that urban statistics are, at least so far as India is concerned, to be preferred to those of rural districts (not individual villages). On the contrary, I suggest that the general returns from a wide yet self-contained rural district are the least unsatisfactory material we have. I even think there is something to be said for the expedient adopted in my Congress Report[1] viz., to use as the criterion of plague prevalence not deaths at all, but the numbers of villages first returning cases in successive months. With certain limitations and after the application of certain corrections, such data are of value. The statistical method adopted in Table II is open to criticism, while the interpretation of the correlations of Table III, like that of my rat-man correlation in 1908, may need revision when further particulars are available.

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[1] ***Journal of Hygiene*, xi, Plague Supplement, pp. 62-156.**

Mr. A. W. BACOT: The action of saturation deficiency in controlling the spread of plague by fleas is far from simple. In India, where there are at least two species of rats and two genera of fleas concerned, the normal flea population is influenced chiefly in the active larval state, and again during adult life, by periods of low humidity. Of the two fleas mainly concerned, *Ceratophyllus fasciatus* is, owing to the sewer haunting, underground habits of the brown rat (its correct host) and its own preference, when adult, to live in the rat's nest, less exposed to the dangers of drought during its larval stage, and during its adult stage as much exposed as, if not more exposed than, *Xenopsylla cheopis*. On the other hand, this last named flea, in its adult stage, spends more of its time travelling on the body of its host, the females dropping their eggs indifferently in the lairs or runs of the rat, where the larvæ are more apt to suffer from saturation deficiency than those of *Ceratophyllus fasciatus*, while the adults have the advantage of frequent meals, and live in the humid atmosphere engendered by their host's skin. Under the conditions prevailing during an epizoötic, the adults of both species are adversely affected by the death of their host, *Xenopsylla cheopis* probably to an even greater extent than *Ceratophyllus fasciatus*, while, if the fleas have reached that stage of infection which makes them active agents in transmitting the disease by their bite, their position on the death of their host is extremely critical. An element of "lagging" in the onset of bubonic plague in its relation to saturation deficiency is to be expected, because it will need some days of drought before the flea population is seriously reduced; but, once this has occurred, it is not surprising that a rapid decrease in plague mortality should occur in spite of falling temperature and increasing humidity. A theoretic point of some interest is that in temperate climates the effect of oncoming cold should produce a somewhat similar modification of the mortality curve to heat and drought, owing to the fleas being rendered dormant by cold on the death of their host.

Dr. REGINALD FARRAR: The Indian plague outbreaks on which Dr. Brownlee's curves were constructed were, for practical purposes, outbreaks running a natural course uncontrolled by effective administrative measures, for the chief incidence of the Indian plague epidemic occurred in years prior to the full development of the theory of rat-flea infection, and sanitary measures applied during this period were either ineffective or even worse than useless, for as long as the population remained in association with rats in their dwellings plague continued to spread in spite of isolation of patients, disinfection, or other administrative action. So-called disinfection or even demolition of infected areas tended even to propagate the disease, for the rats fled before the advance of the disinfecting parties and invaded other areas which had previously been free of disease. The only measures which proved effective were evacuation of infected areas and so-called "health camping," the effects of which were reported to be "almost magical." When the population of an infected area could be deported in bulk into the country, leaving the rats behind them, plague returns fell with striking rapidity to zero. Such measures were practicable and were carried out in the case of small towns and villages, but were impracticable in large towns such as Bombay with an industrial and practically stationary population. The curves of outbreaks treated by measures of evacuation and "health camping" should be compared with those of the Bombay outbreaks and would probably show types very different from those shown by Dr. Brownlee. An outbreak which blazed up fiercely and burnt itself out rapidly, yet in which both the virulence and the infectivity were as active at the end as at the beginning, was the Manchurian plague of 1911. This was an outbreak purely of pneumonic plague; no bubonic cases occurred and the outbreak had no association with a rat epizoötic, not a single infected rat having been found in the invaded areas, though careful search had been made for such. The outbreak had a fatality of practically 100 per cent., for about 43,000 cases occurred, and only three recoveries were claimed, and even as to these three there was some doubt. Starting in Manchu-li, the frontier town between Siberia and Mongolia, the disease was carried by direct case to case infection to Harbin, Mukden, down the Liao-tung peninsula to Dairen, and across the Gulf of Pechili to Shantung, where it burnt itself out. The disease burnt itself out, not owing to any natural biological abatement in the virulence of the infecting organism, but owing to the vigorous system of isolation enforced. Official sanitary measures of isolation, house-to-house inspection, disinfection, &c., were carried out in such towns as Harbin, Mukden, and Dairen, but even in other towns and in villages where there was practically no efficient official sanitary organization, when once the general populace realized the terrible danger of the disease, unorganized but efficient isolation was enforced by general consent, a cordon being drawn round infected houses or even whole villages, food being deposited outside, and no one being allowed to enter or to leave the infected house or area, in which the inhabitants were left to die unaided. Many whole households or even whole villages were exterminated by this measure, but the epidemic progress of the plague was effectively limited. Similar measures have been taken in earlier plague outbreaks, and notably in the Derbyshire village of Eyam in 1666, where out of a village of some 500 inhabitants all but about thirty perished of plague.

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[1] Report to Section XVIII, International Congress of Medicine, London, 1913.

Mr. W. D. BETENSON: The value of the curves will be increased by correction. Being a record of deaths they give no information of the number of cases nor of the proportion of recoveries. Administrative measures are not taken into account, which include (1) treatment, (2) early destruction of rats, (3) prophylactic inoculation, (4) evacuation. Evacuation, either voluntary or compulsory, temporary or permanent, has been the method of dealing with large epidemics in India in all ages. During the 1899 Poonah epidemic the greatest number of deaths per week was recorded when the population of 120,000 had been reduced by voluntary evacuation to 30,000. The actual deaths at this period were 1,000 to 1,200 per week. My experience in epidemics at Poonah and Johannesburg has been that the early cases of an epidemic are pneumonic, then numerous bubonic cases follow, and should pneumonic cases again become prevalent they herald an exacerbation of the epidemic.

Dr. BROWNLEE (in reply) stated that he was in general agreement with what Mr. Greenwood had said. At the present moment one of the chief difficulties in epidemiology was to settle what data were the best. His own preference was for urban statistics but he did not, on that account, deny that very valuable conclusions might be drawn from the study of the statistics of the country districts, in fact, it might be found that the number of villages attacked in a district would furnish a good basis for epidemiological induction.

In reply to Mr. Bacot, Dr. Brownlee said that the point raised that once the flea population was reduced it could not be expected, even in spite of falling temperature and increasing humidity, to again increase rapidly, was certainly most important and might explain part of the phenomenon observed in the epidemic in Rawalpindi in 1903, but he could not accept it as the whole explanation.

Dr. Farrar's references to plague in Manchuria were most interesting and Dr. Brownlee said that he had no doubt that the measures taken undoubtedly aided in preventing the spread of the disease. He could not admit, however, that the evidence regarding the continued virulence of the organism had any bearing on its infectivity. The bacteriologist has yet to learn the difference between power of infection and power of causing death. The word virulence should be restricted to the latter, but was at present used indiscriminately to include both. He had been able to find no record of any experiments designed to test infectivity carried out under rigorous conditions. Here and there there was a glimmering that infectivity and virulence were conditions requiring to be distinguished, but nothing more.

With regard to the remarks of Mr. Betenson, Dr. Brownlee said that they contained one point of great importance which required consideration. Mr. Betenson had stated that in the Poonah epidemic of 1899 the greatest number of deaths per week was recorded when the population of 120,000 had been reduced by voluntary emigration to 30,000. This would seem to be a phenomenon which should distinctly modify the course of the epidemic. The theoretical curve of the epidemic in 1899 does not fit the data with any special accuracy: still the epidemic must be held to have the typical form. In this particular case, however, though the emigration must probably have produced a considerable effect on the total number of deaths it must be remembered that as the population decreased in number the means of infection increased, and that if the number of people exposed to infection fell to one-fourth, the proportionate number of fleas open to infect the remainder correspondingly rose. The form of a plague epidemic might, therefore, not be greatly altered even by a great reduction in the size of the population.

Section of Epidemiology and State Medicine.

President—Dr. G. S. BUCHANAN, C.B.

DISCUSSION ON THE INCIDENCE OF INDUSTRIAL TUBERCULOSIS.[1]

Dr. E. L. COLLIS.

THE present time, when industry is so profoundly affected, chiefly by the removal of men and the introduction of women, may not be inappropriate for considering the influence which occupation exerts upon the prevalence of tuberculosis. This influence is important, but it is one which has not received close attention. I propose in introducing this question for discussion to limit the subject to pulmonary tuberculosis, and to the occurrence of the disease among those aged 15 years and upwards. Industry supplies, for the investigation of the causation of disease, great opportunities which are to-day hardly sufficiently appreciated, or if appreciated, are not taken advantage of. Here are groups of people living under fairly similar housing and climatic conditions, but spending many hours of each day exposed to an environment entirely different according to the occupation they follow. Compare for example the working environment of a coal-miner, a weaver, a cement-maker, a shoemaker, a blast-furnace worker, and an agricultural labourer. The diversity is extraordinary, and far in excess of any alterations in environment presented by such things as housing, sub-soil and climatic conditions which affect different towns and localities. Yet while the incidence of disease and especially of tuberculosis has been studied in relation to these latter conditions, it has not been closely investigated and correlated in relation to the more diverse conditions of industry.

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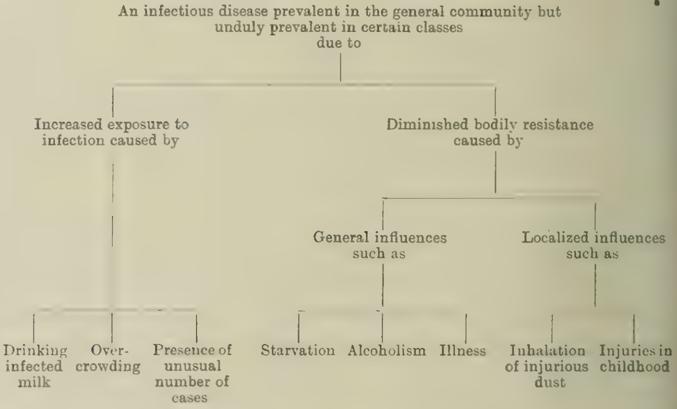
[1] At a meeting of the Section, held May 24, 1918.

INFLUENCES AFFECTING THE PREVALENCE OF TUBERCULOSIS.

I have ventured to prepare the following scheme embodying the influences which are generally agreed to have an influence in promoting the development of tuberculosis:—

TABLE I.—TUBERCULOSIS.


An infectious disease prevalent in the general community but unduly prevalent in certain classes due to
Increased exposure to infection caused by
Diminished bodily resistance caused by
General influences such as
Localized influences such as
Drinking infected milk
Over-crowding
Presence of unusual number of cases
Starvation
Alcoholism
Illness
Inhalation of injurious dust
Injuries in childhood

The influences named in this scheme which are more or less intimately associated with industrial environment are:—

(1) *Overcrowding.*—I mean by overcrowding, the aggregation in unusually close proximity, such proximity as might allow of spray infection due to coughing, of persons among whom tuberculosis is not more than normally prevalent.

(2) *Presence of Unusual Number of Cases.*—By this I mean the presence in an otherwise normal environment of an unusual number of cases of tuberculosis. Clearly this condition will often be associated with and follow upon overcrowding.

(3) *Alcoholism.*—The association of this influence with industrial employment is due to "industrial drinking," either established by the custom of the trade or conduced to by conditions of heat or dust.

(4) *Illness.*—By which I mean lowered general resistance brought about through fatigue, the result of long hours, imperfect ventilation, bad feeding, lack of rest and healthy recreation.

(5) *Inhalation of Injurious Dust.*—This influence may be summed up as exposure to the inhalation of dust of silica. I have dealt with this influence at length elsewhere [1]. It is commonly considered to be the main industrial influence concerned with the incidence of tuberculosis; and I have no desire to minimize its importance; but having regard to the undue prevalence of tuberculosis in industries in which there is no exposure to this special dust, and to the vastly greater numbers of persons employed in these industries, I do not consider the inhalation of silica dust the main pre-disposing cause in industry of tuberculosis.

THE ASSOCIATION OF TUBERCULAR MORTALITY WITH THAT FOR OTHER DISEASES.

The following table gives the comparative mortality for some important causes of death for four typical groups of males:—

TABLE II.—COMPARATIVE MORTALITY OF CERTAIN CLASSES AGED 15 AND UPWARDS, 1900-1902.

Cause of Death	General infection (occupied and retired males)	Increased infection (shoemaker)	Alcoholism (publican, inn servant)	Dust (tin miner)
Influenza	100	67	*171*	104
Alcoholism and liver diseases ...	100	77	*670*	28
Cancer	100	103	110	101
Phthisis	100	*145*	*173*	*436*
Other lung diseases	100	84	*148*	*419*
Nervous diseases	100	101	*178*	84
Circulatory diseases	100	100	*144*	105
Bright's disease	100	86	*243*	*143*
Accident	100	38	88	92
Suicide	100	100	*216*	32

The first, occupied and retired males, is taken as the standard. The second (shoemaker) is chosen as an example of an industry wherein, except for the possibility of infection, no adverse influences such as fatigue, low wages and malnutrition, alcoholism, bad housing, bad ventilation, or imperfect lighting, are more prevalent than in other industries in which tuberculosis is less prevalent [2], and the figures show that the mortality from tuberculosis is the only one calling fcr serious comment. I should mention that the industries of printing and of tailoring show very similar comparative mortality data to those here stated for shoemaking. The third group, not an industrial group, represents the peculiar influence of alcohol; and the data show how this drug, which lowers the general resistance of the body, causes an excessive mortality from every cause; and, probably, if a group could be isolated in which starvation was prevalent throughout life, a similar excessive mortality from every cause would be established. The fourth group is inserted to show the influence of silica dust, which exerts its influence upon the incidence of tuberculosis by damaging the lung tissue; and as might be expected, therefore, other diseases of this damaged tissue are also found in excess. Special investigations have established that this group might have been replaced, among others, by sandstone masons, or grinders of articles upon sandstone wheels; but such workers are not grouped separately in the Registrar-General's tables, from which these comparative mortality figures are taken.

I have drawn attention to these figures to demonstrate the importance, when investigating what possible adverse influences may be determining the presence of an undue prevalence of tuberculosis, of considering whether that prevalence is or is not associated with an unduly high mortality from any other disease. If there is such an association, then, by noting it, attention may be directed to some influence, otherwise unsuspected, which is affecting the point at issue; while if there is no such association, as in the case of the shoemakers, then the issue is narrowed down and the inquiry simplified.

THE INCIDENCE OF TUBERCULOSIS MORTALITY AT VARIOUS AGE-PERIODS.

A further point, which is also of interest, may be deduced from the appended diagram (*see* p. 137). Here the curve for the shoemaker, though maintaining a higher level, is closely similar to that adopted as the standard; so close as to suggest that the predominating influence determining it is, though more accentuated, the same as that determining the curve for "occupied and retired males." The curves for

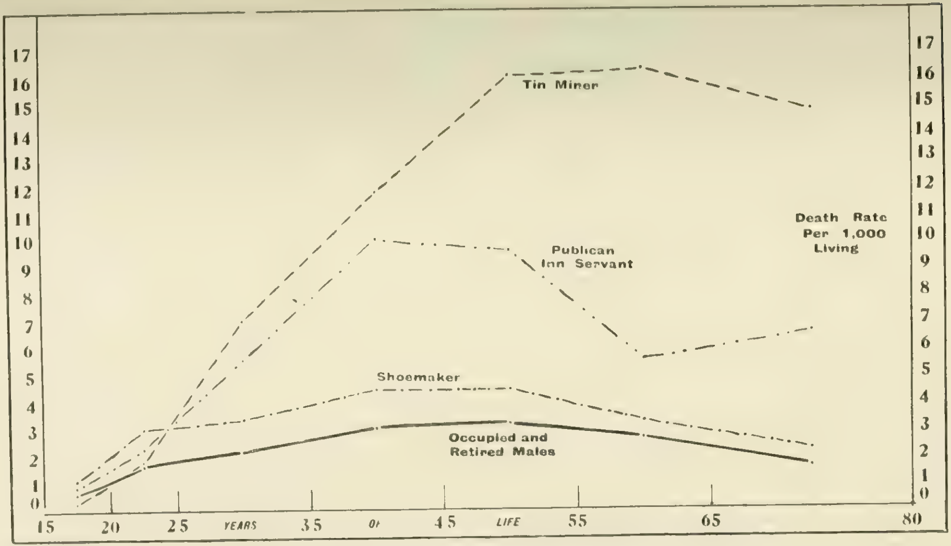


TABLE III. MORTALITY FROM TUBERCULOSIS.

the publican and the tin miner, on the other hand, differ entirely from that of the standard, and from one another; and such differences suggest that different determining influences are at work—as indeed we know to be the case.

THE VALUE OF "PROPORTIONATE MORTALITY."

The statistics so far dealt with concern the death-rate per 1,000 living, but in investigations into industrial mortality such death-rates are particularly difficult to obtain for two reasons:—

(1) Information is nearly impossible to gather as to every death which should be ascribed to some scattered industry or to some particular process common to several industries under investigation. Take, for instance, the grinding of metal on sandstone wheels. This forms part of the cutlery industry of Sheffield, but men are also to be found throughout the engineering trade, a few in this factory and a few in the next, grinding every kind of metal article from fenders and grates to needles. The difficulty moreover is accentuated for such a chronic disease as tuberculosis, which often compels its victim to change his occupation before death, when no indication of the offending occupation appears on the death certificate.

(2) Difficult though accurate information as to total deaths may be to obtain, it is simple compared with obtaining an accurate census of those exposed to risk. At its best a census only gives accurate information of the population at a given time, but the fluctuations of labour are often so considerable as to render such information of doubtful utility a short period from that time; while in order to obtain for some special industry sufficient deaths for statistical purposes, the inquiry has usually to be spread over a considerable period.

These difficulties have induced me to suggest the use of the method of "proportionate mortality" for investigating occupational mortality-data, and particularly data due to tuberculosis. This method consists in examining the causes of death of a sufficient number of persons whose exact occupation is known, without considering the number among whom these deaths occur. If such deaths be classified according to age period, the percentage of all deaths due to any special disease, such as tuberculosis, at each age-period can be stated. I have carefully compared death-rates for tuberculosis, and also for other diseases, at certain age-periods for certain groups for which both the mortality per 1,000 living and the percentage of all deaths are known, and have shown that when the death-rate per 1,000 living exceeds the standard, the percentage of all deaths as a rule exceeds the standard. Reversing the process I think the deduction fair that when the percentage mortality is, say, 20 per cent. in excess of the standard, the mortality per 1,000 living, could it be obtained, would also be found to be in excess. If, for example, the percentage of deaths due to tuberculosis among masons who work on sandstone is taken and compared with that for tin miners, a similarity is present which suggests that the mortality per 1,000 living, could it be obtained for sandstone masons, would, like the mortality per 1,000 living for tin miners, be found to be in excess.

Should for any reason the mortality from all causes, except the one under consideration, be abnormally low, this method gives an exaggerated idea of the probable mortality-rate per 1,000 living, but it still indicates the relative prevalence of any one cause of death. This method is thus far defective, but I desire to dwell on its value, because, from the records of trade societies, and from searches through local registers carried out over a series of years to obtain a reasonable number of deaths, information as to causes of death among individuals whose exact occupation is known, can frequently be obtained, and can be examined by this method, even when no estimate of any value can be made of the size of the population among whom these deaths occurred. The investigation I made [3] into the mortality of members of the Operative Stonemasons' Society of England and Wales, and of the United Operative Masons' Association of Scotland, is an instance of the value of this method. Hoffman has called [4] this statistical method "the proportionate mortality," and he has laid stress upon its value, "particularly in relation to the broad questions involved in the modern problems of industrial hygiene," because "it emphasizes the true incidence of preventable causes of death by divisional periods of life. . . . Granting the limitations of this method, it remains for practical purposes, and particularly with regard to the needs of industrial hygiene, the most useful one available."

NECESSITY FOR A STUDY OF MORBIDITY RECORDS.

I have dealt so far with mortality data and with statistical methods which may be applied to determine the prevalence of tuberculosis in industry, and its probable determining influences; but I have pointed out that this disease, after its inception, often leads to a change of occupation. This fact in itself tends to obscure the issue, and indicates that we require information as to the occupation when invalidity commenced; but the necessity for studying morbidity rather than mortality when investigating industrial health questions rests on surer grounds than this. As long ago as 1853 Finlaison, actuary of the National Debt, when reporting to the Registrar of Friendly Societies, after an elaborate statistical inquiry, writes [5] :—

> In the first place the locality in which a friendly society is established has something but not much to do with the average amount of demand that is made for sickness allowance on the funds of the institution. . . . In the second place the density of aggregation described under the terms of city, town, or rural district, seems to exercise little or no real influence; but in the third place the real practical difference in the distribution of sickness seems to turn upon the amount of the expenditure of physical force. . . . It has been nowhere previously established upon recorded experience, that the quantum of sickness annually falling to the lot of man is in direct proportion to the demands on his muscular power.

This conclusion seems to have lain buried until just fifty years later, when Watson, whose actuarial investigation of the records of the Manchester Unity Independent Order of Odd Fellows was used as the basis of the National Insurance Act, re-stated it in even more definite terms, and after establishing [6] "that the proportion of members sick during any year varies with the occupation," concluded that his investigation afforded [7] "a broad indication of the differences of 'risk' attributable in the case of sickness to occupation causes, and in the case of mortality to geographical situation"; and further that [8] "the element of occupation is of greater importance in determining the contribution for a sickness benefit than that of locality."

Accepting Watson's conclusions and mindful that occupation, as I have indicated, nevertheless makes its mark on the mortality rate of tuberculosis, the inference is clear that if morbidity were carefully investigated the effect of occupation would be obvious. No such investigation, however, has yet been undertaken in this country. It awaits the advent of the Ministry of Health. When it is embarked upon, the difficulty of estimating the population concerned will probably be even greater than in the case of mortality, and I hope that a method of "proportionate" morbidity will be employed, especially in relation to tuberculosis.

CONCLUSIONS.

(1) The diversity of industrial environment presents unique opportunities for studying influences which determine mortality, especially that due to pulmonary tuberculosis.

(2) Most of the influences affecting tuberculosis are present in a pronounced form in one or other industry.

(3) The association of a high mortality from tuberculosis with the presence or absence of a high mortality from other diseases may direct attention to the influences determining the former.

(4) The incidence of tubercular mortality at different age-periods varies with the presence of this or that predominating influence.

(5) Death-rates per 1,000 are so difficult to establish with accuracy for particular industrial groups, that the method of "proportionate mortality" probably gives for any special disease, such as tuberculosis, a better indication of its incidence.

(6) This "proportionate" method should be applied not only to mortality data, but also to morbidity records, which latter are a better index of the effects of occupation than are mortality data.

LIST OF REFERENCES.

[1] Collis, E. L. "Industrial Pneumoconioses," Milroy Lectures (1915), *Public Health*, 1915, xxviii, pp. 252 *et seq*.

[2] The Boot and Shoe Industry (Medical Research Committee), 1915.

[3] Minutes of Evidence, Royal Commission on Metalliferous Mines and Quarries, iii, 1914, pp. 181-189 and Appendix P. (Cd. 7478).

[4] Hoffman, F. L. "Industrial Accidents and Trade Diseases in the United States," i, part ii, pp. 764-769, 1913, Washington.

[5] Finlaison, A. G. Report on Sickness and Mortality, Friendly Societies' Return, 1853, xxix.

[6] Watson. "Sickness and Mortality Experience of the I.O.O.F. Manchester Unity during the Five Years 1893-1897," 1903, p. 66.

[7] *Ibid.*, p. 67.

[8] *Ibid.*, p. 117.

Mr. M. Greenwood.

(Of the Welfare and Health Section, Ministry of Munitions.)

I propose to summarize certain statistical evidence which leads me to think (1) that the recent increase of mortality from tubercular diseases among women in this country is due to industrialization, and (2) that the excessive mortality from this cause in certain occupations not subject to specific risks—such as those involving contact with silicious particles—is not explicable by an appeal to industrial selection.

In Dr. Stevenson's review of the vital statistics for 1916, it is suggested that the slight but real increase in the rate of mortality from tuberculosis which has been experienced by women, an increase of about 6 per cent. in 1916 over the 1914 figure, may well be due to the introduction of many thousands of women to industrial life. In support of this hypothesis, Dr. Stevenson calls attention to the fact that the rise does not affect women of over 45 years of age. The following investigation tends to confirm Dr. Stevenson's hypothesis. The death-rates in occupations of women in this country are unknown, but the German Imperial Statistical Department published in 1910 an analysis of the occupational mortality and morbidity returns as to all insured persons within the Leipzig district for the years 1887-1901. This analysis covers 288,131 years of life (women) with 1,830 deaths and is of course a small sample in comparison with our national data for occupied males. I find, however, that the English and Leipzig males rates for the different occupations are highly correlated, hence it seemed not unlikely that the rates for women would be reasonably applicable to those of our compatriots who followed similar occupations.

The occupational distribution of women in this country in 1914 and again in January, 1917, has been estimated by the Board of Trade. Applying the appropriate rates to each occupational group, we should have found 6,124 deaths from tuberculosis among English industrially employed women in 1914 and 7,691 in 1916. To reach gross deaths we must assume some rate for the large balance of women not industrially employed. I adopted for this purpose the lowest of the Leipzig occupational rates—viz., 990 per million (the rate for commercial employees). I chose this, although the rate for women employed as domestic servants, cooks, &c., was slightly lower (970 per million), because the large residual group includes many women, such as the wives of ill-paid operatives and general labourers, whose mortality experience may well be worse than that of domestic servants. In this way one reaches an expected total of 22,466 deaths for 1914. The actually recorded deaths numbered 22,214, so that the estimate is only in error to the extent of a little more than 1 per cent. Applying the same process to the population as distributed, from the occupational standpoint, in January, 1917, we compute 23,986 deaths, a result within 0·7 per cent. of the actual number, 24,131. The computed totals give rates of 1,177 and 1,221 per million, while the Registrar-General's determinations are 1,164 and 1,234. My calculation on the Leipzig basis gives an increase of 4 per cent. between 1914 and 1916; this is less than the observed increase, but the agreement between the gross totals seems to me to lend decided support to the opinion that industrial reorganization has been a principal factor of the change in the tuberculosis position.

Passing next to the general question of industrial tuberculosis, I wish to deal with the usual criticism of statistics tendered as evidence of environmental factors at work in causing tuberculosis. The criticism is that those occupations subject to a high tuberculosis rate are (excluding the silica trades) precisely the industries which make little demands upon physique and can therefore be pursued by those whose bodily development is below the average and who consequently are presumably less resistant to the ordinary dangers of infection from which no industrial worker is immune. A fisherman, it is urged, does not experience a low tuberculosis rate because he is a fisherman, nor a shoemaker suffer greatly from tuberculosis because he works in a boot factory, but the people who are likely to put up a good fight against tuberculosis become fishermen and those less able to struggle successfully become shoe hands.

No scientific man is, I apprehend, likely to deny that this argument is a very cogent one and I am far from supposing that I have been able to isolate the part played by selection from that due to environment. The following considerations are, however, of some interest. They depend upon the assumption that the death-rate from all causes *other than tuberculosis* is some measure of industrial selection and occupational status. We know that the robust die at a greater rate than weaklings, the poor at a greater rate than the rich. These propositions are not only true for tuberculosis, they are applicable to the great majority of diseases having statistical importance (cancer is *not*, as has been stated by some writers, any exception). If then we determine the average relation between the death-rate from all causes other than tuberculosis and the death-rate from tuberculosis, we shall reach a prediction formula which will give us an estimate of the tuberculosis rate we should expect to find in any occupation on the basis of its mortality from other causes. If the rate actually observed is greatly in excess of this predicted amount and if the particular occupations which show an excess in one set of data occupy the same position in other sets of statistics, it is reasonable to conclude that there is something in the occupation which has a determining influence. The data utilized were the occupational mortalities of the decennial abstracts for 1890-92, 1900-02 and 1910-12 (the latter kindly supplied in MS. form by Dr. Stevenson), and the special report on the experience of 1908-11 recently issued by the Central Bureau of Statistics at The Hague. The regression equation connecting deaths from phthisis (in the 1910-12 statistics deaths from all forms of tuberculosis were used) with deaths from all other causes, except accidents and suicides, was formed—the regression proved to be linear—and the predicted rates were compared with the observed rates. Taking an excess of 25 per cent., or more, beyond the predicted value as a fair limit, it is found that in every set of data, both Dutch and English, four occupations are differentiated—viz., bookbinders, printers, tailors and cabinet-makers. Shoemakers, hatters, hosiery makers and hairdressers also exhibit an excess in each of the English sets of data but not in the Dutch records.

As a further test, the following process was devised. The mortality from phthisis at ages 35 to 45 among the employed persons of 1900-02 was correlated with the death-rate from all causes except phthisis in that decennium, and also with the death-rate from all causes including phthisis of the same occupations at ages 25 to 35 in the previous decennium. Here the predicted value is based not only upon the general death-rate prevailing among these persons but also upon the previous mortality of such of them as had worked continuously at the same trades in earlier life. The introduction of this measure of previous health made but little difference. Thus we have:—

Rates of Mortality from Phthisis, ages 35 to 45: experience of 1900 to 1902 (per 1,000 living).

Occupation	Observed rate	Rate predicted from other mortality	Rate predicted from other mortality and mortality at ages [illegible] of previous decennium
No. 39 (Bookbinders)	4·70	2·48	2·91
,, 40 (Printers)	4·85	2·48	2·9
,, 48 (Shoemakers)	4·24	2·23	2·66
,, 47 (Tailors)	4·00	2·46	2·55
,, 65 (Cabinet-makers)	3·79	2·43	2·55

With these occupations may be contrasted others, such as No. 25 (brewers) and No. 62 (slaters and tilers), the phthisis rates of which are high, in each case 4·06 per 1,000 at ages 35 to 45, but can be predicted with reasonable accuracy from the "other causes" mortality (for No. 62 the prediction is 3·60, and 4·2 for No. 25).

I think, therefore, that this test provides tolerably distinct evidence that the incidence of phthisis upon the occupations I have named is an industrial effect in the stricter sense of the words. The method is open to criticism; many such criticisms occur to me and will be discussed in a forthcoming paper by Dr. Tebb and myself.

Dr. BENJAMIN MOORE, F.R.S.

I submit a classification of all the occupations, 104 in number, dealt with by the reports of the Registrar-General. These are arranged in the descending order of magnitude of the incidence of pulmonary tuberculosis. This list illustrates the great variation in the incidence of phthisis arising from occupation, and shows the paramount importance of environmental conditions. Thus, the severity varies over tenfold from 3·73 times the normal in tin-miners to only 0·37 of the normal amongst clergymen, and there are many manual trades with nearly double the normal incidence and others with an incidence little more than half the normal. Some of the latter are dusty occupations indicating that certain dusts are harmless. These variations may be traced to several operating causes, such as injurious nature for the lungs of substances handled, casual conditions of work and payment, varying degree of individual infection at the work, the nature of the work and of surroundings whilst at work.

The general list of classification gives certain interesting juxtapositions of employment such as that commercial clerks are no more heavily hit by consumption than are open-air transport workers, for the incidence figures in both lies only 8 per cent. above the average for the whole country, that the whole class of factory operatives spinning and weaving cotton indoors who are popularly believed to be riddled by consumption possesses an incidence figure lying only 3 per cent. above the average, and practically the same as over one million males living outdoor lives in all the building trades. The large class of the coal-miners, on the other hand, possesses only half the normal incidence, and ranks on terms of equality with "all agriculturists." Again, "farm labourers," who are commonly believed to suffer much from consumption and are poorly housed, are every whit as healthy, so far as this disease is concerned, as their masters, the "farmers, farmers' sons, and graziers," each class showing only half the normal mortality.

The general table of classification may be split up for more detailed consideration into groups of occupations with common conditions or handling the same or similar materials. When this is done certain of these groups are found to show a great preponderance of occupations with an incidence higher than normal, while others just as clearly show a preponderance with incidences lying below normal level. The following groups show an enhanced incidence: A, casual and badly paid occupations; B, occupations handling the heavy metals (tin, copper, lead, zinc), or their compounds; C, occupations handling hard silica (flint, quartz, ganister) or glass; D, occupations handling skin products and their derivatives—viz., hair, felt, leather, wool; E, occupations exposed to cellulose dusts in various forms, such as wood, paper and cotton. The healthy occupational groups are: F, all occupations handling food; G, occupations handling coal, clay, ironstone, iron and steel; H, agricultural occupations and those largely upon out-door work; I, certain sedentary occupations, or occupations with light manual labour.

In certain occupations in the list, such as miners in heavy metals and silica workers, other types of respiratory diseases rise *pari passu* with phthisis, demonstrating that the primary cause is a chronic inflammatory irritation by the material worked upon; in another class of occupations heavily hit by the disease, such as printers and shoemakers, phthisis alone of the respiratory diseases mounts high above normal, showing that the responsible factors are probably abnormal degree of infection in the workshop, and predisposing general conditions in the occupation.

It may be suggested that free silica probably owes its pre-eminent evil properties as a pre-disposant to phthisis, not to any chemical properties, but to three important physical properties—viz. (1) its heaviness, (2) its sharp vitreous fracture, (3) its great hardness and insolubility.

The statistics derived from a study of phthisis and its predisposing factors in the allied printing trades, made by Moore and Flack from the obituary lists of all the Trades Unions, show that a steady improvement has been going on in the printing trade, which is especially marked between 1900 and 1914 (when the outbreak of the war disorganized all statistics). Another interesting point arising here is that all types of printing labour are found to suffer as much as compositors, and some even more severely.

Improved conditions of labour, and more particularly reduction of hours and regularization of times of work, have contributed largely to this beneficial result.

[Conditions conducive to infection amongst printers were described and remedies suggested.]

Dr. JOHN BROWNLEE.

One aspect of the subject has not hitherto been sufficiently considered. It is commonly assumed that phthisis affects persons in different parts of the country at the same age-periods of life, but this is so far from true that the differences in the age-distribution of the deaths from this disease in different parts of the country are so great that they can only be explained on the assumption that phthisis is not one disease due to one organism but a group of diseases due to a group of organisms, just as enteric fever and dysentery have proved to be. Thus in Shetland the whole number of deaths among males practically takes place between the ages of 15 and 30, few deaths taking place later. In London, on the other hand, the commonest age at death from phthisis is between the years of 40 and 50, the frequency of death from phthisis being distributed somewhat symmetrically about this maximum. In addition it seems to be a fact that among workers in trades which increase the susceptibility to phthisis the age at which phthisis is most common is that at which it is most frequent in the locality in which the occupation is carried on. Thus in Cornwall it is found that the age-distribution of phthisis is such that there are two maximum frequencies in the death-rates, one in young adult life, the other between the ages of 50 and 60. In the tin-mining districts of Cornwall the frequency with which males die of phthisis is at the earlier maximum, not clearly different from that at which males die in the non-tin-mining districts. In the later ages of life, though the number of deaths among males in the tin-mining districts is in immense excess over that in the non-tin-mining districts, the age associated with the maximum frequency of death is the same for both. Similar phenomena are noted in North Wales. In this district there is a great deal of phthisis in the later ages of life, which is in accordance with the observations made by Dr. Collis that among the slate quarriers, the great bulk of whom work in North Wales, the age at death from phthisis is much higher than among those who quarry in sandstone.

This aspect of the question has hitherto not been sufficiently considered. A great amount of spade work must first be carried out before the different factors which determine these phenomena can be fully understood.

Dr. E. L. Collis (in reply).

Agreement has been expressed as to the value of investigating the prevalence of tuberculosis in industry, with a view to throwing light on the environmental conditions which affect the disease. Mr. Greenwood has pointed out how the present industrialization of women has synchronized with a rise in tubercular mortality, a rise which his skilful analysis suggests is definitely due to industrialization. Dr. Moore has emphasized the value of the Registrar-General's mortality data referring to occupations; and, with special reference to the printing trade, has indicated that each industry requires to be investigated intensively. I do not find myself in agreement with his physical explanation of fibroid phthisis, but the fact that we do differ points to the desirability of further research. Dr. Brownlee has uttered a warning that investigations must allow for the possibility of tuberculosis being not one disease but a group of diseases due to a group of organisms, a group the members of which attack different age-periods of human life. This possibility is one of great interest, and the warning he gives is salutary, especially when the "proportionate mortality" method is used, which depends so much upon age-distribution.

I have reason to believe that the object of this meeting will be fulfilled, and that the whole matter will be more closely studied by those members of the Section who are working on it acting in collaboration.

Section of Epidemiology and State Medicine.

President—Dr. G. S. BUCHANAN.

The Epidemiology of Tri-nitro-toluene Poisoning.[1]

By W. J. O'DONOVAN, M.D.

(Ministry of Munitions.)

WHEN the problem of the ill-effects of T.N.T. was discussed before the Royal Society of Medicine on January 23, 1917, the gravity of the occasion and the importance of the discussion was widely recognized. Both in England, and abroad in many countries, the effects of T.N.T. absorption were coming for the first time before all kinds of practitioners, and the need for an authoritative epitome of present knowledge was wisely met by a special issue of the *Proceedings* of the Society, which has been freely drawn upon by all workers in this subject since its issue.

Dr. Legge, of the Home Office, in opening the discussion, stated that "T.N.T. shows itself a splendid antagonist in the resourcefulness of its attack and the variety of the symptoms produced, and will be overcome, not by one precaution alone, but by the combination of several, of which alternation of employment, frequent periodical medical examination, ventilation, and clean working are the chief."

These precautions, formulated into a code of rules by a committee under the able chairmanship of Sir Walter Fletcher, have been applied to every factory handling T.N.T. in this country, and to place before you the results of this special effort in preventive medicine is the object of this short communication.

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[1] At a meeting of the Section, held April 12, 1918.

In August of 1916 there were thirty-seven cases of toxic jaundice and eight deaths published by the Home Office, and in September following twenty cases and eight deaths; in October forty-one cases and thirteen deaths: twenty-nine deaths in three months. This does not include the large number of sufferers from the minor but none the less distressing effects of T.N.T. such as rash and gastritis, and the rare but interesting condition of aplastic anæmia.

Dr. Benjamin Moore voiced the apprehension that all felt when he said that the subject was of enormous importance not merely from the number of people who die, because they are a comparatively small fraction of those who work on the material—but on account of the large loss of labour at a period of stress in the country.

In my first communication to the Society I referred to the fact that non-occupational acute yellow atrophy of the liver was a recognized and constant feature in the mortality returns, and by the courtesy of Dr. Stevenson of Somerset House I can present the reported deaths from this cause up to the end of 1917 :—

Deaths from Acute Yellow Atrophy of Liver, 1911 *to* 1917.

	Male	Female	Total
1911	17	26	43
1912	16	40	56
1913	16	33	49
1914	30	47	79
1915	22	30	52
1916	27	38	65
1917	33	36	69

The quarterly figures of toxic jaundice here set out give the case incidence of T.N.T. jaundice and T.N.T. deaths in a rough but instructive fashion.

Quarterly cases of Toxic Jaundice, 1916 *and* 1917.

	1916	1917
January-March	6^{4}	83^{12}
April-June	16^{5}	56^{20}
July-September	73^{21}	21^{8}
October-December	86^{22}	29^{4}

The raised figures are deaths.

In attempting to work out curves of the incidence of toxic jaundice I had, however, to depart from published returns. It has to be remembered that notifications of industrial disease are often not made at the date of their occurrence, indeed a death and inquest may be the first official intimation of the occurrence of a severe T.N.T. case; further, cases are not published by the Home Office until the report has been verified by the local certifying surgeon. It follows that on occasion cases appearing in, say, a January return, may have occurred in the previous December or November. In these curves every case has been allocated to its proper month of occurrence. When an industrial illness is a constant feature of a trade such delays tend naturally to fall into a constant feature, making one period fairly comparable with another, but, in dealing with this T.N.T. outbreak of a limited duration, individual correction of dates can be applied in practice owing to the intimate relations of the Factory Medical Officers with the Ministry of Munitions.

Information as to the prevalence of T.N.T. sickness is received weekly at the Ministry on a form setting out the names of separate factory processes and the cases are subdivided under headings of: Toxic jaundice, rash, other cases (gastritis). The number working is asked for and the percentage incidence of sickness. Cases of jaundice or aplastic anæmia are individually the subject of a special report by the Factory Medical Officer.

Previous History.—The innocuous nature of T.N.T. has in the past been dwelt upon by all authorities, and the same faith was held up to July, 1917, by F. Koelsch,[1] who said that tri-nitro-toulene had no specially poisonous property, but that impurities present owing to crude war manufacture caused slight symptoms resembling those of nitrobenzol, but is in all respects milder in type.

First Case.—It is of interest therefore to put on record a few facts concerning the first case among shell fillers that attracted the attention of those ever on the watch for new forms of industrial disease. On July 9, 1915, Dr. Edgar Collis, after visiting a factory in Kent, travelled on the back of a motor bicycle to Whitstable to see a patient Rn. under the care of Dr. Rupert Smith, who had connected his patient's illness with the "inhalation" of tri-nitro-toluene, and who gave the following history: Age 19; previously an oyster dredger; employed filling shells and mines with tri-nitro-toluene for twelve weeks. Left work a month previously suffering from severe vomiting and inability to take food. He said his bowels only acted after taking salts, and that the motions were clay colour, also that the urine was deep brown. On examination the conjunctivæ and whole surface of the body were deeply jaundiced, but his general condition was not serious. Except for some depression his aspect was normal, pulse 72, lips a good colour. There was no tenderness over the liver, but Dr. Collis thought the liver was rather reduced in size. The patient recovered.

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[1] *Zentralbl. f. Gewerbehygiene*, July, 1917.

It is of course only too certain that cases had occurred before, and that the link between occupation and disease had not been supplied by the worker or his medical attendant.

The first death certainly due to T.N.T. was a man, A. W., aged 44, who worked on shell filling at Woolwich from May, 1915, to July 30, 1915, and died on August 17, 1915, in Guy's Hospital, under Dr. Herbert French. The post-mortem showed those changes due to T.N.T., with which we since became so familiar.

T.N.T. Sickness.—I would illustrate by the sickness figures of an important filling factory the remarkable decline that has occurred since the present system of medical oversight was instituted at a very critical time. The number employed varied from week to week, but a total of 6,000 T.N.T. operatives may be taken as a working figure. The amount of sickness in percentages on the different processes was as follows :—

WEEK ENDING 1916	MELT FILLING		PRESS FILLING		INCORPORATING MILLS
	Male	Female	Male	Female	Males only
October 7	4·0	5·9	3·2	2·5	5·3
October 14	7·7	10·0	4·3	5·1	5·7
October 21	11·4	8·7	3·8	7·2	7·4
October 28	4·1	3·1	4·5	5·6	3·0

These figures were furnished from the returns of the Factory Medical Officers. It can be seen how arduous must have been their daily task, including as it did the verification of a large amount of external doctors certification, the patrolling of the workshops and the supervision of the absent sick. The above figures can be compared with those of the same period of 1917.

WEEK ENDING 1917	MELT FILLING		PRESS FILLING		INCORPORATING MILLS
	Male	Female	Male	Female	Males only
October 6	0·7		1·4	1·1	1·0
October 13	0·3	—	1·0	2·5	0·7
October 20	0·7	—	1·2	2·3	3·0
October 27	0·6		1·1	1·0	1·4

Males only are employed in the mills on account of the high temperature constantly maintained for the drying, grinding, and mixing of T.N.T. and ammonium nitrate.

The diminution in sickness can be paralleled at the other factories, and may be attributed to the combination of several factors.

Causes of Diminution of Sickness.

The introduction of whole-time Factory Medical Officers has been of immeasurable benefit. Useful as sporadic medical inspection is for occasional problems, its benefits are overwhelmingly multiplied when an able medical officer is added to the managerial staff. As the medical officers are to the regiments so the T.N.T. doctors are to the workers in their factories whose livers and bone-marrow are in truth always in the "danger zone." The presence of a medical officer in the factory is a continual reminder to the manager of the ever present health problem, and the doctor is always at hand to offer advice and to criticize proposals as to hours, ventilation, feeding and sanitation that affect even indirectly the health of the T.N.T. worker.

The preliminary medical examination has come in for much criticism, yet its value is indubitable. It brings the worker and doctor into contact from the first day of employment, it enables the doctor to know the health of the body of labour under his control and to weed out those whose poor physique or actual illness make an easy breakdown probable. Whether fatigue or disease be the cause of illness it is a moral certainty that in the absence of, and indeed often in the presence of, physical signs malaise will be attributed by the worker or his doctor to T.N.T. absorption.

By intercourse with neighbouring practitioners, attendance at the local hospitals, and consultation over dubious cases, T.N.T. can be made to assume its proper proportion in local medical certification, and preventive measures should do the rest.

The cardinal rule for T.N.T., as for other explosive work, is clean working. To take the simplest example, shells were once filled everywhere by a process of hand stemming. T.N.T. was weighed out by hand and emptied into a funnel put into the mouth of the shell. The powder was distributed in the shell and rammed into place with a wooden rod and mallet. An intelligent and skilled worker could do this with ten or fewer well directed blows, but the majority of workers would give over fifty ill-directed blows, spilling the powder with every effort and raising puffs of dust with each impact. Tens of thousands of shells were filled in this manner, and to a lesser degree the same held true of the filling of the small linen exploder bags, only 3 in. long, which process has had its own fatalities out of all proportion to the total weight of T.N.T. handled.

Screw filling has been the ultimate solution of the clean working problem. Much experimental work was needed owing to the explosive and engineering problems involved, but to-night I am able to show photographs of machines that fill both shells and exploder bags with the very minimum of human contact. As an economic proposition automatic filling made its own appeal but reinforced by the health menace and the recommendations of the T.N.T. Committee set up by Mr. Montague, the most earnest attention was directed to this engineering aspect, and with such results, considering the inherent explosive risks and engineering difficulties, not the least of which was the supply of material and labour, that those concerned deserve every encomium that has been expressed by all conversant with the problems of machinery in explosive factories. In his communication read at this Society in January, 1917, Viscount Chetwynd did not refer to the experiments he was even then carrying on, and which, by the March following, enabled it to be said that his method of automatic filling with a vertical Archimedean screw was extremely satisfactory.

I am able to show photographs illustrating shell filling by vertical screws and by the more recent horizontal method. Speaking generally, a 6-in. shell can be filled in forty-five seconds and the saving in labour and contact that this means needs no labouring on my part.

Alternation of employment has been another safeguard to which great importance has been rightly attached. By removing the worker from contact it lengthens the period during which the factory medical officer can pick out the incipient stages of grave poisoning, it gives time for the elimination of T.N.T. absorbed and as shown by the table below comparing the experiences of three large and similar national factories, its existence has been a great factor in preventing a large amount of serious disease during the early period while adequate precautionary measures were being installed.

1916.

	Alternated.				Not Alternated.		
	Workers	Cases	Deaths		Workers	Cases	Deaths
H.	2,011			C.	3,001		
G.	1,308			L.	2,907		
A.	2,989			M.	1,593		
	6,308	9	2		7,501	85	25

Ventilation.—The rôle that improved ventilation has played in the reduction of T.N.T. illness is hard to assess. The oppression that is felt in any enclosed room where T.N.T. is manipulated is a very real thing and prolonged physical labour under such conditions is enervating, and the difficulties of working under such conditions has forced the matter into the foreground : any improvement in ventilation under such circumstances has had immediate and gratifying results. Every T.N.T. worker is an absorber of T.N.T. and to microscopic or visible extent has a certain amount of met-hæmoglobin and NO-hæmoglobin in circulation. Adequate ventilation is a remedial measure tending to preserve the balance or chemical compensation of the hæmoglobin katabolism, an undue strain of which, Dr. Moore suggested, is the underlying cause of both toxic jaundice and aplastic anæmia. The occasionally noxious nature of some forms of T.N.T. supplied to factories has been a matter of complaint since it was first used in large quantities. Irritative and respiratory symptoms, such as lachrymation, cough and sneezing are common when such material is being handled. I have smelt impure samples a hundred yards away from the gate of the factory, and adequate ventilation is an essential for healthy and comfortable working with such material. A volatile and fairly stable impurity has been isolated from crude T.N.T.—tetra-nitro-methane. It is an oily yellow fluid, rather volatile, which clings to crystals of T.N.T. Its acrid characteristic nitrous odour is well-known in manufacturing plants, where the most constant effect of its inhalation is a transient asthmatic-like cough, but which, in susceptible persons, gives rise to headache, precordial depression and vertigo, fatigue on slight exertion, and sometimes dyspnœa. Owing to the presence of this volatile irritating impurity, adequate ventilation is essential where heated T.N.T. is manipulated. Doors and windows and louvre walls provide natural ventilation sufficient for cold processes, but some form of plenum is essential wherever a hot process is in operation. Exhaust ventilation presents its own problems in a T.N.T. factory. It is introduced to suck away fume and dust from the point of origin, and as regards dust powerful local exhaust draughts have proved of great service in keeping the exploder bag filling process in a dairy-like state of cleanliness. A minute quantity of fine dust may fall from the spigot of a bag filling machine but none falls on the worker's hand or the working bench when the draught is in operation, and loose empty bags that should not lie in the working area are whisked away. An electric fan operating in a T.N.T. dust current has explosive and fire risks of its own and hence an induced draught is in general use. A powerful current of air is blown from a lateral opening along the main duct and this pulls along with it the stationary air in the hinder parts of the duct and a recovery chamber is the only obstacle between the working orifice of the duct and the open air.

Factory Dentist.—I have referred to the work of the factory medical officer in keeping down T.N.T. sickness and mention must be made of the factory dentists, first appointed on the suggestion of Sir Thomas Barlow at a visit to the largest T.N.T. shell filling factory. Large numbers of workpeople that would be refused employment by the factory doctor for dental reasons can be made available without much delay. Gastritis is greatly lessened among the workers with healthy mouths, and as it is difficult to distinguish between a mild T.N.T. gastritis and a dental dyspepsia, the possession of a wholesome mouth greatly lessens the difficulty of a differential diagnosis. Scalings and extractions are done on the factory premises in the factory time. One dentist has informed me that his average of extractions was 1,200 a month. We are more than anxious that T.N.T. workers should be well fed and to this end excellent canteens are everywhere established, but without an appetite and a healthy mouth combined this canteen provision becomes largely ineffective.

Gloves have played no part in our protective devices. From the first it was appreciated that workpeople would never keep the interior of their gloves free from contamination and that the duration of the war would not be long enough to instruct thousands of workpeople in an aseptic ritual in putting on and off their gloves and overalls, hence except for rough or hot work, gloves, of cotton or rubber or leather, are definitely discouraged.

> For historical continuity, reference must here be made to a paper by Simeon Snell on "Amblyopia due to Di-nitro-benzol," in the *British Medical Journal*, March 3, 1894, which sets out a series of precautionary measures with practical cautions that might be applied to-day almost without change to T.N.T. factories.

So far, then, I have dealt with minor T.N.T. sickness and rash, and shown, I think, that a combination of clean mechanical working, selected workers, medical supervision, and ventilation, have brought T.N.T. sickness down to a negligible quantity.

Aplastic Anæmia.

In all, fourteen cases of aplastic anæmia are known to have occurred; twelve have died, one, a man aged 72, is still ill, four had previous toxic jaundice. Aplastic anæmia may show itself as a gradually increasing debility and breathlessness as in pernicious anæmia or t may have a fairly sudden hæmorrhagic onset. I cannot lay too much stress upon the necessity of a skilfully conducted post-mortem in these cases, inasmuch as fatal uterine hæmorrhage may, and has been, wrongly ascribed to abortion in certainly two cases and the cutaneous hæmorrhages of both septicæmia and leukæmia may be and have been wrongly ascribed to tri-nitro-toluene when occurring in T.N.T. workers.

It is worthy of note that the factory with the severest incidence of toxic jaundice and the heaviest incidence of minor sickness (C.) has only had one hæmorrhagic case whereas another large factory with only a moderate incidence of toxic jaundice (H.) has had four cases of fatal anæmia.

Varying Incidence of Jaundice and Minor Illness.

The variability of the incidence of the several forms of attack by T.N.T. has been referred to by the Medical Research Committee in the introduction to the report of Dr. Moore's published research on T.N.T. poisoning: "Those who hold this view lay stress upon the independent variability in the degree of cyanosis on the one hand and of jaundice on the other" (Special Report, Series No. 11), and I can best illustrate this wide variation between the amount of jaundice and minor illness by a short note on the sickness at a factory on the West Coast open to the Atlantic breezes. The working conditions were bad, the jaundice cases numerous over a short period, but the minor sickness throughout was small in amount. There were four sheds with five or six melting coppers in each and splashing of molten T.N.T. was common. The spigots of the coppers in which T.N.T. was melted dripped continuously, lumps of T.N.T. lay on the floor and shell carriages, alternation was unreliable and there were other faults. Up to February 14, 1917, there were thirteen cases of toxic jaundice with seven deaths. Dirty working may have been the cause of the toxic jaundice cases, but one would have expected also a large amount of T.N.T. sickness.

SUMMARY OF FOURTEEN CASES OF APLASTIC ANÆMIA DUE TO T.N.T.

Name	Age	Sex	Jaundice	Result	Notes
M. S.	22	F.	No	Died February 20, 1917	During life certified as anæmia and T.N.T. poisoning : purpura before death : no tubercle bacilli in portion of lung ; no atrophy in liver, examined microscopically
M. R.	21	F.	Yes ; September, 1917	Died March 26, 1918	Recovered from jaundice ; petechial hæmorrhages a fortnight before death. Post-mortem : Pale marrow, scarred liver
M. T.	19	F.	No	Died September 3, 1917	Nine days' work in T.N.T : purpura August 21, 1917
F. P.	36	F.	No	Died November 25, 1917	Six months' work on T.N.T. ; vomiting May 8, 1917 ; very anæmic June, 1917 ; red blood corpuscles, 900,000, September 22, 1917. Pregnant, delivered 5½ lb. live baby day of death
M. B.	26 about	F.	No	Died April 20, 1917	Left work for holiday March 31, 1917 ; blood in mouth in mornings one month before leaving work ; menorrhagia April 13, 1917. Diagnosis, abortion
H. G.	20	F.	No	Died August 3, 1917	Ill July, 1917 ; epistaxis, hæmatemesis, menorrhagia one week previous to death
W. H.	72	M.	No	Alive	August, 1916, T.N.T., gastritis ; hæmatemesis January, 1917 ; 1,300,000 red blood corpuscles September, 1917 ; condition stationary, April, 1917 ; no albumin in urine
A. K.	44	M.	Yes ; June, 1916	Died February 17, 1917	Pallor and epistaxis six weeks before death. Post-mortem described in *Proceedings of the Royal Society of Medicine*, 1917, x, (Discussion on Toxic Jaundice). p. 76
A. E. P.	?	F.	Yes ; February, 1917	Died June 17, 1917	Purpura noted May 8, 1917
E. J.	31	F.	No	Died August 27, 1917	July 6, sore throat ; purpura July 30. Post-mortem showed no other disease present
F. P.	20	F.	No	Died January 4, 1918	Amenorrhœa ten weeks ; flooding December 21, 1917 ; gums bled before death ; no skin hæmorrhages
K. C.	21	F.	No	Died December, 1916	Hæmatemesis day of death. Post-mortem : No gastric ulcer, petechiæ in skin, large degenerated areas in liver
S. S.	18	F.	Yes ; December 28, 1916	Died April 18, 1917	Two and a half weeks' work on T.N.T. ; jaundice disappeared in hospital, but increasing anæmia supervened
M. N.	19	F.	No	Died April 19, 1918	Profuse mucous hæmorrhages ; red blood corpuscles, 840,000. Post-mortem : General petechiæ, no change in liver, aplastic bone marrow

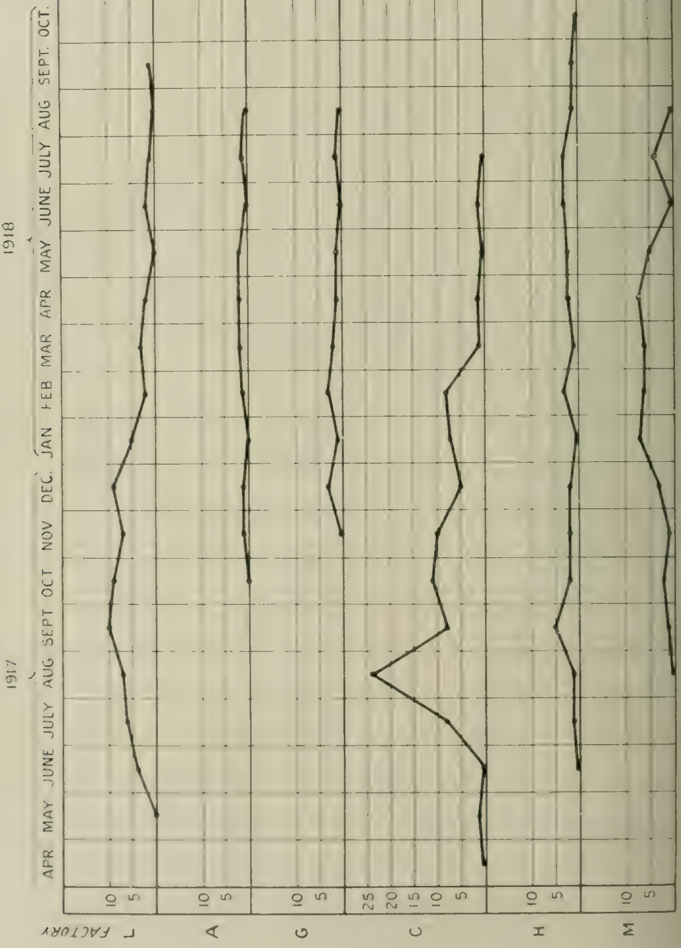
Week ending	Number employed	Sickness
December 16, 1916	390	4 cases
December 23, ,,	441	3 ,,
December 30, ,,	405	2 ,,
January 6 and 13, 1917	324	1 ,,
January 27, 1917	333	4 ,,
February 3, ,,	350	5 ,,
February 10, ,,	382	3 ,,
February 17, ,,	280	2 ,,
February 24, ,,	130	1 ,,
March 3, ,,		
March 10, ,,	320	1 ,,

This list should be read parallel with the following list of its cases of toxic jaundice which, besides illustrating the low sickness rate while jaundice was occurring, shows also a contrast between the deaths of the early cases and the recovery of the later, due, I think, largely to the early removal of jaundiced workers and the administrative arrangements made for the immediate hospital treatment of all cases of toxic jaundice.

July 31,	1916	T. S.	Dead
July 31,	,,	B. R.	Dead
August 1,	,,	E. D.	Dead
September 21,	,,	L. J.	Dead
October 30,	,,	J. H. J.	Dead
November 17,	,,	Mrs. H.	Alive
December 7,	,,	G. P.	Dead
December 8,	,,	L. D.	Alive
January 9,	1917	M. D.	Alive
January 31,	,,	G. B.	Alive
February 3,	,,	K. H.	Dead
February 3,	,,	L. B.	Alive
February 14,	,,	S. J. B.	Alive
February 21,	,,	B. W.	Alive
March 4,	,,	S. J.	Alive
March 7,	,,	M. S.	Alive
March 20,	,,	M. M.	Alive

Incidence of Toxic Jaundice.

I would like to submit here the curve of incidence of toxic jaundice at a factory (C.) which has filled about 40,000 tons of T.N.T. and, with wide variations, may be said to have employed without alternation 6,000 people upon T.N.T. The sudden rise in the incidence of jaundice is noteworthy; its peak of twenty-four cases actually occurring in August of 1916 and then a gradual fall to nothing in March of 1917. This is the factory whose minor sickness I have referred to above as reaching 11 per cent. of the workers on one process in October, 1916.


1917
1918
APR
MAY
JUNE
JULY
AUG
SEPT
OCT
NOV
DEC.
JAN
FEB
MAR
APR
MAY
JUNE
JULY
AUG
SEPT.
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L FACTORY
10 5
A
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G
10 5
C
25 20 15 10 5
H
10 5
M
10 5

Diagram I.

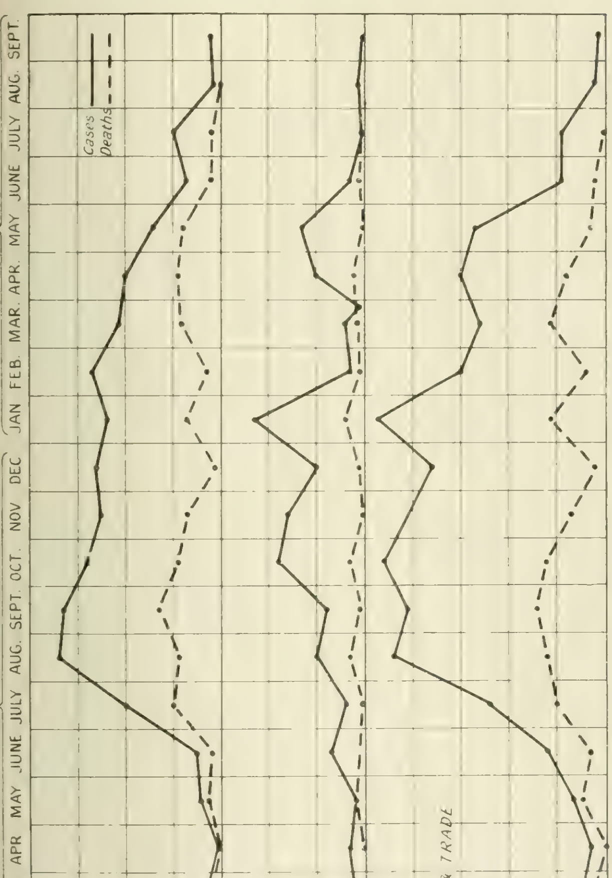

1916
1917
FEB. MAR APR MAY JUNE JULY AUG. SEPT. OCT. NOV DEC
JAN FEB. MAR. APR. MAY JUNE JULY AUG. SEPT.
Cases
Deaths
NATIONAL
TRADE
NATIONAL & TRADE
30
20
10
0
20
10
0
40
30
20
10
0

DIAGRAM II.

The curve of another factory (L.) shows its highest peak of cases in September, 1917, and, as to be expected, of deaths in the following October.

A factory in the North of England (G.) ran until December of 1917 without a single case of toxic jaundice, and then in that December had three cases, one in the following month, and a very occasional case since.

Another important national factory (M.) ran until September of 1917 with only one case, and one case in each of the following two months; three cases in December of 1917, and eight, seven, seven, eight, cases in the following months of January, February, March, and April of 1917, dropping to nothing in the following May.

Another national factory (H.) where T.N.T. was first handled in any bulk in March of 1917 no case occurred until there was an outbreak of four in October of 1917.

The remaining set of three curves show the total incidence of toxic jaundice and fatalities up to the latter part of 1917.

Prognosis.

Anyone perusing the *Proceedings* of this Society that dealt with toxic jaundice, in January, 1917, must have come away with a very gloomy picture of the prognosis of this affection. So heavy were the pathological contributions that the clinical aspect hardly attained its due importance, and hence certain fragmentary records in recent literature should be mentioned here.

Dr. Barbara Crawford, the medical officer to a very large T.N.T. manufacturing plant, reported in the *British Medical Journal*, April 20, 1918, four cases of toxic jaundice that recovered. In all four there was profound illness, jaundice, delirium, and marked diminution of liver area, and later recovery, with replacement of liver substance and function.

Dr. W. Bower, in the *British Medical Journal* of May 4, 1918, reported the result of his re-examination of past cases of toxic jaundice. Thirty-three cases examined by him six to eighteen months after their return to work showed no diminution in the area of liver dullness, and no interference with nutrition, or with the digestive functions. He draws attention, however, to the ill-effects of T.N.T. upon heart muscle, ten of his cases being still unfit to take on prolonged exertion or heavy work.

Ætiology.

Dr. Stuart McDonald's paper of January 19, 1918, *British Medical Journal*, on acute yellow atrophy in syphilis, cannot be passed over by anyone attempting to come to any conclusion as to the ætiology of toxic jaundice. In July, 1908, he stated in the *Edinburgh Medical Journal* that there is much to be said in favour of the view that in the causation of yellow atrophy we have two factors at least at work, and in this last paper on the subject he too deals with five typical cases, all occurring together in a limited period, although thousands have been treated by the same methods of salvarsan and mercury without these complications previously occurring. This is very comparable to our experience with tri-nitro-toluene.

Factories under constant working conditions have run for long periods without cases, and then toxic jaundice has appeared in an epidemic form, to disappear almost as mysteriously as it came.

No one special feature seems to predispose to this incidence. A poor physique will not necessarily fail on T.N.T. work, and the healthiest may succumb, as only too many post-mortems have shown. A puzzling feature is the long delay that may occur between the exposure to T.N.T. and the onset of toxic jaundice. In the January, 1917, *Proceedings* I published the case of a boy, C. M., who left his factory at the end of June, 1916, and was first jaundiced on September 1 of the same year. A remarkable case of toxic jaundice occurring six months after exposure is published by Dr. Legge in the Annual Report of the Chief Inspector of Factories, 1917, in which case the onset of jaundice appears to have been determined by a severe cranial injury. Even more remarkable is a recent case, M. B., at Liverpool, the first case of jaundice that has ever occurred at this particular factory.

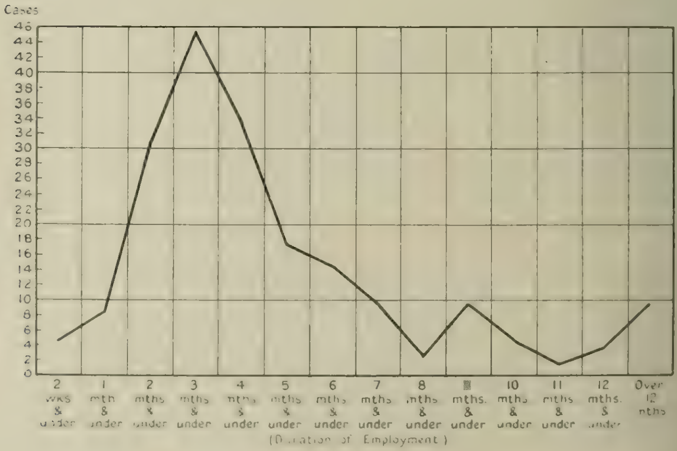
The girl began work on January 18, 1917, and put waxed block charges of amatol (T.N.T. and ammonium nitrate) into shells off and on until September 10, 1917. Since that date she was not in any contact with T.N.T. On June 6, 1918, she was well, and on June 14, 1918, she died, jaundiced and comatose. The post-mortem by Professor Glynn showed the liver 41 oz., gamboge in colour, soft and flabby, and on section a gamboge colour throughout, with no dark red atrophic areas. The microscopic report by Dr. P. N. Panton showed central necrosis of the liver lobules, with peripheral fatty degeneration uniform over the sections examined.

Taken by itself this case might be looked on as one of non-industrial acute yellow atrophy of the liver, but with the knowledge that there are other cases exhibiting similar, though shorter, latent periods, it would be rash to disassociate T.N.T. from this fulminating case of fatal jaundice occurring ten months after contact with the explosive.

The ætiology of T.N.T. sickness is still obscure. The channels of its absorption and excretion are known, but when one bears in mind its eclectic incidence at the various factories, and the want of correlation between the minor and the graver forms of T.N.T. sickness, and, above all, the puzzling feature of this latent period, that holds both for toxic jaundice and fatal aplastic anæmia, one is driven to the conclusion that the ætiology is not simple, but that two factors may well be involved :—

(1) A primary damage of the liver cells by T.N.T. or its isomers, that are always present in a small percentage.

(2) Some unknown bacterial or toxic invasion acting upon the damaged liver and producing the clinical jaundice, or post-mortem atrophy, that we all recognize.



DISCUSSION.

Dr. T. M. LEGGE: The diminution in sickness which the figures brought before us show to have taken place in national filling factories is due largely to the energy which Dr. O'Donovan has unsparingly thrown into his work. I want to direct attention to a point, well illustrated by T.N.T. poisoning, which may not strike the ordinary observer. I mean the difficulty of notification of industrial disease because, if care is not taken, the usefulness of the requirement may be greatly impaired. When illness—notably toxic jaundice from tetrachlorethane in dope late in 1914 and from T.N.T. in 1915 began to occur the first step necessary was to get information of the cases which would be a real guide as to the risk run and the need for precautionary measures. In notification of infectious disease differential diagnosis is the difficulty. With industrial disease not only is there this but, owing to the slow onset, the patient passes through a stage of absorption which does not amount to poisoning, and yet in which the stigmata of the particular compound as, for example, the blue line on the gums from lead absorption, and cyanosis from T.N.T., are apparent. Now T.N.T. is protean in the various ways in which its effects are shown. Dr. O'Donovan has described the five most prominent—on the skin, the alimentary canal, the blood, the liver, and the bone-marrow. With exception of the first they are all to be described as T.N.T. poisoning, but for *statutory* notification all are not equally important, although obviously the management of the factory should be aware of all cases. But when statutory notification has to be made the medical practitioner requires a definite symptom to guide him. Publication of statistics of notifications of T.N.T. poisoning would be quite unreliable, as the figures would refer to a number of T.N.T. workers seeking medical treatment for any and every complaint. But figures published monthly of incidence of toxic jaundice furnish a real indication of serious illness due to the compound calling in each case for inquiry as to causation and observance of precautions. It was for this reason that toxic jaundice and not T.N.T. poisoning was added to the list of diseases to be notified under Section 73 of the Factory and Workshop Act. The procedure has reduced expense to the State, has relieved the medical practitioner of doubt, and has saved endless worry and waste of time, which must have resulted had a wider requirement been imposed. The curve (p. 164) shows that for the development of toxic jaundice in 1917 the critical months of employment were the second, third and fourth, as in them some 60 per cent. of the cases occurred. And you will see the very close similarity the curve for 1917 shows with that prepared for most of the cases in 1916.[1] This fact goes far, in my opinion, to explain the curves of incidence Dr. O'Donovan has shown in certain factories. The sharp rise takes place a few months after the factory has started, a high level of incidence is maintained while precautions are inadequate and the "salting" process being undergone. Then as precautionary measures—notably clean working and diminution of dusty processes—are elaborated, the curve steadily drops. I should explain the curves in which there is no sudden sharp rise and steady fall as showing the effect of alternation of employment—a fortnight in T.N.T. and a fortnight out—because I believe that in the most striking curves shown sharp ascent goes with lack of alternation and never where there has been alternation.

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[1] ***Proc. Roy. Soc. Med.***, **1917, x (Special Discussion on Toxic Jaundice in Munition Workers), p. 72.**

Dr. A. S. MACNALTY: With reference to the question of possible secondary infection in tri-nitro-toluene poisoning, the recent preliminary communication of Professor Stuart McDonald,[1] already referred to by Dr. O'Donovan, is of interest. Professor McDonald records five cases of acute yellow atrophy, which occurred in syphilitic subjects in the course of or subsequent to treatment by salvarsan substitutes and mercury. In these cases cultures from the heart blood and lung have given copious growths of organisms, apparently of the coli-typhoid group. Professor McDonald surmises that the essential cause of acute or subacute yellow atrophy in munition workers is some poison, possibly of a microbic nature, produced in the alimentary tract and acting on a liver previously damaged or whose function at least has been disturbed. Assuming the truth of this view, T.N.T. workers might be exposed to three distinct attacks, which cumulatively might prove fatal to the individual. These are: (1) T.N.T. poisoning; (2) hepatic disease, brought about, for example, by syphilis or alcohol; (3) a secondary infection of the coli group or an intercurrent affection such as influenza or tuberculosis. Causes (1) and (3) together might be sufficient to cause death. In making this suggestion, it is noteworthy that Dr. O'Donovan found many of the workers suffered from gastro-enteritis.

Dr. E. L. COLLIS: Dr. O'Donovan's communication is of importance not only on account of the material which he has placed before us, but because of the clearness with which he has presented this material. I have personally been especially interested in his account of the T.N.T. position during the past twelve months. Previous to that time I was myself closely associated with the position, but since then a change of work has placed me so that I have only been able to acquaint myself with the position through official returns and reports. My active participation in the epidemic—if I may be allowed to use the word "epidemic" in regard to the occurrence of poisoning which took place—ceased when the epidemic was at its height twelve months ago, and I have been particularly struck with the extent to which new appliances have been introduced in the factories, which maintained output and minimized exposure to poisoning. Dr. O'Donovan has suggested that a second factor may be associated with T.N.T. in causing toxic jaundice, and there certainly are some points in the way in which cases have occurred which make this suggestion worthy of careful consideration. Still, I almost hope that such a second factor will not be found to exist, as, if T.N.T. itself is the only cause of poisoning, then the position at which we have arrived to-day is the more satisfactory; and we have before us an instance which I venture to think must become famous in the history of preventive medicine—the history of how a previously unknown form of poisoning was recognized in its earliest stages, was investigated chemically and pathologically in the scientific laboratory, was studied completely in the factory and at the bedside, and has been brought into subjection by preventive measures—and all within the period of a little over two years. This combination of chemists, pathologists, administrative officers, both central and in the factories, and lastly, but not least, of engineers who have ungrudgingly used their inventive powers in the production of ingenious devices, is hard to parallel in the history of any disease. Dr. Legge has suggested that the character of the curves which represent the incidence of cases of toxic jaundice in the individual factories may be explained by two influences. First, the peculiarity of the curve of period of employment of those affected—such a curve as is shown in the report upon the discussion which took place at this Society on T.N.T. poisoning in munition workers in January, 1917; and secondly, the introduction of improved methods of working. I am not, however, myself convinced that these two influences will account for the epidemic character of the cases in industrial factories. Were it so the curve in its rise should closely agree with the curve of period-of-employment incidence of those affected. I mean by this that the curve representing the period of employment of those in the factory ought to coincide with the curve of employment of those affected, and that the peak of the curve ought to mark the moment in which the personnel of the factory became steady. Reference however to the individual factories does not, I think, bear out this contention, but it is a point which is capable of being tested. Further, we find that in point of fact the epidemic in each factory began to fall off and was almost over *before* the steps taken, especially the introduction of new machinery, could have had any pronounced effect. In passing I may draw attention to one point namely, that lead poisoning has some similarity in the way in which the incidence of cases is related to the period of employment. (I do not here refer to very chronic cases which are recognized only to occur after nine to ten years of employment.) These cases are at their height in the first three months of employment and hereafter the curve falls steadily away until it is almost at the base line at the end of two years.

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[1] Stuart McDonald, "Acute Yellow Atrophy in Syphilis," *Brit. Med. Journ.*, January 19, 1918, p. 76.

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VOLUME THE ELEVENTH
SESSION 1917-18

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1918

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Section of Laryngology.

President—Mr. T. Mark Hovell, F.R.C.S.Ed.

(*June* 1, 1917.)

Primary Carcinoma of the Nasopharynx.

By Irwin Moore, M.B.

Members will remember that this case, a female aged 70, was shown at the meeting of the Section on November 3, 1916,[1] three and a half months after removal of the growth (July 14, 1916). The left tonsil had enlarged since the operation, was congested, and indurated, whilst patient complained of severe pains extending from the left side of the neck to the top of the head. No enlarged glands could be felt. Opinions were invited as to the question of recurrence in the tonsil, and advisability of enucleation. No opinions were, however, expressed on this point, though Dr. Brown Kelly and Dr. William Hill referred generally to the striking results obtained by the use of radium in malignant diseases of the nasopharynx.

On November 17, Captain Pinch kindly saw patient with me at the Radium Institute, and advised that a radium emanation needle should be inserted into the tonsil. In the meantime patient consulted her physician, Dr. Sydney Green, who recommended injections of colloidal copper, and I agreed, since there appeared to be no special indication or urgency for radium.

Dr. Sydney Green has carried out the treatment.

Patient is now exhibited, six and a half months later, to show the remarkable results of this treatment, which has apparently not only cured the pain, but also reduced the size and induration of the tonsil.

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[1] *Proc. Roy. Soc. Med.*, 1917, x (Sect. Laryng.), p. 32.

It is now ten months since removal of the growth and there is no recurrence, nor enlargement of glands in the neck. Patient feels very well and considers that her improved condition is directly attributable to these injections.

(*June* 1, 1917.)

Lupoid Tuberculosis of the Pharynx, affecting the Soft Palate and Uvula, in a Boy, aged 8, the subject of Congenital Syphilis.

By Irwin Moore, M.B.

This case was shown at the Section meeting on March 2, 1917, as lymphadenoma, but in consequence of the discussion and the chronic character of the infiltration, together with the more recent clinical and pathological examinations, the title has been changed.

The former diagnosis was based partly on the blood examination, the features of which were consistent with, but not diagnostic of, lymphadenoma, but mainly on the opinion of Dr. Barcroft, who kindly saw patient for me at the Margaret Street Hospital for Consumption, on February 12, and reported as follows: "I found large discrete glands in the neck on both sides, and also in the inguinal region, which, taken with the other signs, could only be caused by lymphadenoma. Although no actual relationship has been demonstrated, it is very usual to find pulmonary tuberculosis along with this condition. However, in this case the pulmonary signs have not as yet developed."

A month later, on March 10 (since this case was exhibited), patient was taken to the Mount Vernon Hospital for Consumption and there seen by Dr. Halls Dally, who reported as follows: "I regard this case as one of *slightly* active tuberculosis. There are palpable glands in both posterior cervical triangles and groins. There is now a slight cough, with night sweats and slight rise of temperature, and the child is losing flesh in spite of the appetite being good.

Clinical examination reveals scattered fine moist crepitations over the whole of both lungs. Liver and spleen not enlarged.

Radioscopic examination is negative. There is no enlargement of hilar glands, no peribronchial striation, and no enlargement of tracheo-bronchial glands. No tuberculous foci apparent in lung tissue. Temperature, 3 p.m.: 98° F.

White cell count 10,000 per cubic millimetre (normal, 6,000 to 8,000).

DIFFERENTIAL WHITE CELL COUNT.

Polymorphs	68·0 per cent.
Lymphocytes	26·5 ,,
(Normal about 25 per cent.)	
Eosinophils	2·0 ,,
Mast cells (none seen)	
Large mononuclears	3·5 ,,
	100·0 ,,

ARNETH COUNT.

Cells of Group I	8 per cent.
Cells of Group II	44 ,,
Cells of Group III	39 ,,
Cells of Group IV	9 ,,
Cells of Group V	0 ,,

Total nuclear bodies, 249 (normal, 275).

There is thus a slight leucocytosis, the lymphocytes are within normal limits, but a little on the high side, and the Arneth is low."

Since March 12 the infiltration of the soft palate has gradually extended downwards on to the faucial pillars and tonsils. Examination of the larynx on March 24 by Killian's suspension apparatus showed also infiltration of the epiglottis and arytænoids.

There are no signs of miliary tubercle nor of ulceration either in the pharynx or larynx.

Histological Report of Adenoid Tissue removed from the Nasopharynx.—"There is a certain amount of fibrosis with chronic inflammatory reaction, which is characterized by the formation here and there of cellular systems, which occasionally have a central multinucleated giant cell. The lesion is undoubtedly tuberculous." (Dr. Eastes' Laboratory.)

This case is of interest in that :—

(1) *Tuberculosis of the pharynx* is a relatively rare disease, for the pharynx would appear to be especially immune to the deposit or growth of the tubercle bacillus. It has been shown by statistics that it is only affected in from 1 to 2 per cent. of consumptives. It is uncommon to find tuberculosis of the uvula and soft palate. In 100 cases of local tuberculosis of the pharynx, the uvula and soft palate were found to be affected in eight.

(2) *Statistics show that tuberculosis of the nasopharyngeal adenoid tissue* has been found in 7·5 per cent. of individuals apparently free from pulmonary tuberculosis—based upon the presence of giant cells alone, or

together with the tubercle bacillus; whereas in those cases of advanced disease in the lungs, especially when complicated by laryngeal involvement, the percentage is greatly increased—viz., to 68 per cent.

(3) *The chronic lupoid character of the uvula and soft palate*, together with the latent infection of the pharyngeal tonsil and the apparently recent implication of the lungs, suggests in this case a primary affection of the pharynx.[1]

(*June* 1, 1917.)

Tertiary Syphilis of the Pharynx in a Female, aged 44, Clinically resembling Tuberculosis of a Lupoid Type.

By IRWIN MOORE, M.B.

PATIENT, a widow, was admitted to hospital on May 23, 1916, complaining of soreness of the tongue and throat, with pain and difficulty in swallowing for five years. There has been difficulty in breathing for two years, though patient has been able to sleep comfortably lying down; also difficulty in speaking for three months. She can only swallow liquid foods, and has taken nothing solid for six months.

Patient was married at the age of 22, has had one miscarriage at the age of 24, whilst two children are now alive aged 19 and 21. Husband died twelve years ago from enteritis, aged 37. The swelling in the throat dates back to a year or two before marriage. She has had no treatment until five years ago, when the trouble became more marked, and she attended the Croydon Hospital for "ulcerated throat."

Present condition: There is well-marked stridor on slight exertion. On examination of the throat, extensive nodular infiltration of the fauces and tonsils is seen extending on to the soft palate, uvula, and base of tongue. The posterior pillars are scarred, and have become adherent to the posterior pharyngeal wall. The oropharynx is markedly stenosed, the epiglottis is absent, and the larynx cannot be seen.

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[1] Since this case was shown further investigations have been made, some of which are still in progress, and will be reported at the next meeting. Two independent authorities have found a strongly positive Wassermann reaction, and both parents have admitted infection with, and treatment for, syphilis. Dr. Halls Dally has demonstrated by radiograms recent enlargement of the hilar glands, with associated *peribronchitis tuberculosa simplex*, and has obtained a definite tuberculin reaction after diagnostic injections of O.T. A portion of the uvula has been removed, and sections are under investigation. Professor Shattock has advised that the term "lupoid tuberculosis" should be retained.

A low tracheotomy was performed under local anæsthesia the day after admission to hospital.[1]

DISCUSSION *(on Dr. Irwin Moore's four cases—for fourth case see p. 22).*

Dr. W. HILL: If other members have had experience of the results of using colloidal copper locally, it is important we should have their views on it. I have not heard anything encouraging enough about it to lead me to use it, but I gather that the first case enormously improved under the treatment.

Dr. JOBSON HORNE: Will Dr. Irwin Moore give us details of the treatment—how it is carried out, how frequently, and over how long a period?

Mr. HOWARTH: I should like to know why these cases are labelled "lupoid tuberculosis"? Why have we given up the name "lupus" for these cases? One I looked at seems to be typical lupus, and it is a pity to be constantly changing the names of diseases.

Dr. JOBSON HORNE: May I ask Dr. Irwin Moore to give us his views as to what lupus is: whether it is tuberculosis or not?

Dr. BROWN KELLY: The second case is not lupus but a variety of hyperplasia. I have seen two or three somewhat similar cases and usually have found congenital syphilis present: they do not readily yield to specific treatment. The condition differs slightly from "sclerotic hyperplasia."

Dr. JOBSON HORNE: Has the Wassermann test been done in the second case?

Dr. W. HILL: I suppose there is a tendency to call anything lupus which looks like chronic tuberculosis and in which tubercle bacilli are not found, or they may be called "lupoid tuberculosis." The last is a sort of "portmanteau" word, convenient to distinguish a condition from ordinary tuberculosis. I do not know whether there is a lupus which is non-tubercular. But there is a suspicion in these cases in which the lung is involved. Possibly an error in diagnosis has been made, but the original diagnosis was rather nearer the truth.

Dr. H. J. BANKS DAVIS: I am very interested in these cases, because one constantly meets with this type. If one sees a tubercular looking ulceration or infiltration of the palate and there is no pain, I put the case down as lupus: if there is pain, I regard it as tubercular. I do not think

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[1] Since showing this case at the meeting on June 1, the uvula, also pieces of the pharynx, have been removed, and several sections have been cut and examined.

I have ever seen a case of tubercle of the palate or pharynx which was not very painful; so great is it that the patient will not eat, and slowly loses condition. Lupus is not painful, though the involvement may be extensive. Some years ago I saw, with Dr. Jobson Horne, a well known singer at a London music hall, who developed tuberculosis of the palate; he had such pain that he could not eat, and was slowly going down-hill. He had had syphilis, and went away to Eastbourne. It was a question whether he ought to have injections of mercury, and iodide of potassium internally. If there is infiltration of the larynx, unless it be syphilis or malignant disease, and the patient is given iodide, the infiltration is tubercular, breaks down and the patient becomes worse. This man was given iodide, and when he came back the whole of the palate had sloughed off. Iodides, in my experience, never reduce infiltrations in the upper air passages, if they are due to tubercle, but are liable to cause rapid destruction of the tissues.

Mr. DAWSON: With regard to the second case, I suggest that a small piece ought to be removed and submitted to microscopical examination. Even though tubercle bacilli may not be found, the structure shown by the section may reveal its nature. It reminds me of chronic enlargements of the lower lip.

Sir STCLAIR THOMSON: The boy's throat did not appear to me characteristic of lupus and, of course, tuberculosis in the pharynx rarely occurs except in advanced cases of pulmonary disease. The condition suggests a form of hyperplasia, possibly associated with congenital syphilis.

On the epidiascope I now show a very good coloured drawing of the rare form of chronic tuberculosis attacking a healthy adult, who had nothing the matter with his chest, but showed tubercular ulceration on his arm. The picture was done some years ago; the patient was treated with tuberculin and the galvano-cautery, and was seen recently in a condition of perfect health.

Mr. MOLLISON: In reference to what Dr. Banks Davis said, in regard to tuberculosis of the pharynx, I think the cases of the kind he mentioned are invariably secondary to phthisis. Lupus is primary.

Dr. JOBSON HORNE: Are Dr. Irwin Moore's cases to be permanently labelled "Lupoid Tuberculosis"? I speak for the credit of our *Proceedings*. Case III certainly does not belong to the lupoid class; and I do not think the others are tuberculous, or the patients would not have lived as long as they have.

Dr. IRWIN MOORE (in reply): Dr. Sydney Green, who carried out the injections of colloidal copper in the first case, gave, between February 20 and March 23, seven intramuscular injections of cuprase (5 c.c. doses), a preparation obtained from the Anglo-French Drug Company. The injections were painful, so they were preceded by a 5 c.c. injection of quinine and urea. The patient is a panel doctor's dispenser, and the injections did not interfere with her work. Her health has improved remarkably. The pain in the neck has disappeared and the enlarged fibrous tonsil, which it was thought might be a recurrence of the growth, has become softened and reduced in size. With regard to the cases of tuberculosis of the pharynx, I have not, as yet, ascertained the Wassermann reaction in the second case, because I thought the diagnosis was sufficiently clear from the investigations which had already been carried out. In a publication by Lockhart, of St. Louis, on "Tuberculosis of the Nose and Throat," 1909, Plate xxvi, there is a water-colour drawing of an almost identical case to mine. Mr. Edward Davis kindly examined the larynx with me by Killian's suspensory apparatus, and by this means we both had a view of the larynx. He also regarded the case as one of tuberculosis. I received a report this morning that the Wassermann reaction in Case III is positive. It is my intention to remove a piece of the infiltration for microscopical examination.

(*June* 1, 1917.)

Specimen of Epithelioma of Left Vocal Cord Two Years after Operation.

By JAMES DONELAN, M.B.

PATIENT, a male, aged 54. Some of the chief points in the history are that the patient was an excessive user of cigarettes and alcohol. He also contracted syphilis in 1888. I first saw him in 1908. He had then a pronounced deflection of the nasal septum to the left, almost completely blocking that side and causing sinus retention. An irritating discharge passed down the left side of his pharynx and caused some laryngeal irritation and cough. He was averse from any operation at that time. Several negative Wassermann reports were received. Nevertheless energetic antisyphilitic treatment was given. The nasal septum was subsequently resected and the local and general condition being much improved he returned to India.

He was seen again on August 22, 1914. Hoarseness. Small sessile growth at junction of anterior and middle thirds of left vocal cord. Mobility unimpaired. Mercury and iodides were again ordered and as no effect was apparently produced exhibitor removed the growth by the indirect method on the eighth or tenth day after seeing him, and Dr. Wyatt Wingrave reported verbally that it consisted of merely inflammatory tissue with no malignant elements. The gap left by the operation filled up rather too rapidly and the appearances were such ten days later that a consultation was suggested as to whether laryngo-fissure should be performed. The patient was seen with Dr. Dan McKenzie and he suggested that a more extensive removal of tissue should be made by direct method. It was arranged that Dr. McKenzie should remove specimens from the most suspicious parts of the swelling. This was done on September 14, 1914. Dr. Wingrave again reported that no malignant elements were present. Though advised to keep under observation the patient disappeared for six months, three of which were spent at Harrogate, where he had a course of treatment by means of sprays, &c. As the condition of his larynx did not improve he returned to London at the end of March, 1915. All the appearances were then those of malignant disease and the removal of the cord was advised. Patient desired a further consultation and saw Sir StClair Thomson who recommended operation as soon as possible.

On April 13, 1915, I removed the whole of the left cord including the vocal process. As the condition of the commissure appeared suspicious it was thought best, even with the greater loss of voice, to remove the anterior third of the right cord as well. The after-course was quite uneventful and patient went for a drive on the eighth day. He continued remarkably well until April, 1916, when he had decided symptoms of tertiary syphilis with increasing stenosis of the larynx. The curious thing about this is that the granuloma and cicatricial bands first developed on the right side, affecting chiefly the arytænoid and remainder of cord, and the patient could breathe only through the space left by the removal of the left vocal cord. In consultation with Sir StClair Thomson it was decided that tracheotomy should be done and patient has worn a tube since June 4, 1916. Since then the appearance of the larynx has at times given rise to considerable anxiety as to recurrence of malignant disease, but improvement has always followed the use of arseno-benzol in suppositories and energetic mercurial inunctions and injections. The patient's general condition has greatly improved and the view is held that up to the present there are no positive signs of recurrence.

Dr. Fletcher, acting on behalf of Dr. Eastes, reported that the section of the left cord showed a distinct epithelioma while the posterior part was free from the disease. Through an accident the portion removed of the right cord was not preserved at the time of the operation.

DISCUSSION.

Dr. DONELAN: The discussion on this case was adjourned from last time, on the motion of Dr. Dan McKenzie, so that he and Sir StClair Thomson might discuss the advisability of waiting for the microscopic proof before operating.

Dr. W. HILL: I think this should be described as a case of double lesion in a syphilitic subject.

Sir STCLAIR THOMSON: This case teaches several lessons. One is, that the direct method of removing growths from the vocal cord goes no deeper and is no more effective for obtaining a specimen than is the indirect method. A specimen was removed by the indirect method by Dr. Donelan, but was not deep enough to disclose the malignant growth, as often happens. Dr. Dan McKenzie attacked this growth by the direct method, but he also failed to get down to the malignant growth. On these two negative bases this patient goes untreated for six months before removal is carried out. Not having seen the patient earlier, I cannot say whether a diagnosis could have been made earlier. In many of these cases it is a physical impossibility to get a satisfactory specimen from a cord. So we are reduced, in such cases, to purely clinical grounds—i.e., to the appearances which we all know—and to watching the case, looking for the slow flagging coming on in the cord, the cauliflower appearances, the dimpling, and the cupping which often takes place in these growths in the middle. An important side issue from epithelioma in this case is that I understand this syphilitic subject gave six negative Wassermann reactions. Yet, from the view I got when Dr. Donelan allowed me to see the case I was convinced that it was the syphilis which had returned. He was treated with antisyphilitic remedies, and he is alive and well and there is no recurrence of his malignant disease. Here, again, too many people are apt to trust to the "penny-in-the-slot" diagnosis, and conclude that if a patient does not give a positive Wassermann he has not got syphilis.

Dr. DONELAN (in reply): I am much indebted to Sir StClair Thomson for bringing out the points of interest in this case. I regret very much the absence of Dr. Dan McKenzie, as I know he had some observations of the highest interest to make. I have only to call attention to the notes which show that from the first I advised this patient to have his cord removed because the appearances were those we recognize as those of epithelioma. It is admitted that as much as 10 per cent. of negative Wassermann reactions are unreliable, and this case appears to be one of the 10 per cent.

(*June* 1, 1917.)

Case of Nasal Fibroma (of the Right Fossa) with Specimen and Microscopic Section.

By L. H. PEGLER, M.D.

THE case is that of an elderly man, with specimen and section (described at the last meeting),[1] enabling members to note the subsequent behaviour of the root on the middle turbinate, which has not been disturbed. The fuller histological report promised is as follows :—

Surface much fissured. The fissures and intrusions are invested with the ordinary epithelium; some of them run deeply into the substance, but though continuous with the surface appear as circular spaces, the long palisade epithelium contrasting with that lining the gland ducts.

Stroma of fibrous tissue, parts of which are of considerable density; it is remarkably non-vascular.

Glands are conspicuous and generally distributed. There are groups of acini and many ducts.

This growth, which bears some pathological relationship to a polypoid mucous hypertrophy, is best described as a fibroma, the pedunculation and volume of the mass justifying the diagnosis. (This is also Mr. Shattock's view, who has carefully studied the specimen, and has requested it for the College of Surgeons' Museum.)

DISCUSSION.

Mr. FRANK ROSE: I examined this microscopical section with considerable interest, because it is something which one seldom sees. My difficulty consists in giving a suitable name to it. It is called a fibroma. There is fibrous tissue in it, but that is not its most conspicuous feature. The epithelium, both that covering the outside of the specimen and that forming the gland tissue and the ducts, is quite as conspicuous, and I hesitate to apply the term "fibroma" to a specimen in which there is so much glandular tissue. It reminds me more of the structure of a nasal polypus, one in which the basis is tough and firm. I shall be glad to hear Dr. Pegler suggest a title which does justice to those features.

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[1] See *Proceedings*, p. 121.

Mr. CLAYTON FOX: Why not call it "non-œdematous fibroma" in contradistinction to œdematous fibroma—the ordinary nasal polypus?

Dr. W. HILL: I suggest "fibroglandular hyperplasia" as a name for it.

Dr. PEGLER (in reply): Valuable though the microscope may be in determining the nature of a tumour formation, it has its limits as an aid to diagnosis. One is at a great disadvantage in forming an opinion if unable to study the macroscopic aspect of a growth *in situ*, its behaviour during operation, and its appearance in the fresh state after removal. There are signs of recrudescence in the middle turbinal attachment, which has not been disturbed since evulsion, and so far the appearance differs greatly from that which we should have seen in a common polypus case. I shall keep the patient in view. Several members have commented upon the title fibroma for the specimen. The only alternative that Mr. Shattock could suggest when we studied the specimen together was "fibro-adenoma," but we concluded that the glandular element was not conspicuous enough to justify this name. On the other hand, I quite admit that the growth has its affinities, and any leaning here is certainly towards the turbinal moriform hypertrophy type, but the affinity is not sufficiently close to indicate any clear reason for altering the selected title which best covers its main features. Mr. Shattock's verbatim report having been asked for, and promised by myself, is here appended:—

"(1) *Stroma* of fibrous tissue nowhere of great density. It has a conspicuous amount of gland tissue distributed in it, but this does not assume sufficient proportions to allow of such tissue being viewed as neoplastic, and enable us to class the growth as a *fibro-adenoma*. The surface of the growth is much fissured, the processes being invested with the ordinary (palisade) epithelium. Some of the fissures run deeply into its substance, and though doubtless continuous with the surface, the connexion in many instances is missed in the section. The long columnar epithelium lining such spaces is ciliated, and so differs from that lining the gland ducts. I should think the growth best described as a *fibroma*. I have looked at sections of inferior turbinals with moriform hypertrophy; there are quite as many glands in that tissue just below the epithelium. I take it that this fibroma is somewhat akin to a moriform hypertrophy in which similar deep fissures occur; but in the latter the signs of inflammation are obvious, whereas in the specimen under consideration the pedunculation and volume of the mass justify it being classed as a neoplasm."[1]

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[1] In a subsequent note sent to the exhibitor since the date of the meeting, Mr. Shattock points out that the real gland tissue not being in excess of that found in the normal nasal mucosa should decide against a diagnosis of fibro-adenoma or of fibro-adenomatous hypertrophy.

(*June* 1, 1917.)

Case of Hyper-adduction of the Left Arytænoid Cartilage

By L. H. Pegler, M.D.

PATIENT, a male, aged 30, has always been hoarse; voice weak and easily fatigued. The cords approximate imperfectly, owing to the left arytænoid overlapping its fellow on vocalization. The case, which appears to be congenital, is shown on account of its simulating in one respect the behaviour of an arytænoid when the opposite cord is paralysed.

DISCUSSION.

Dr. JOBSON HORNE: I should like to hear the idea of others as to whether there is movement in the right cord. It did not seem to me to approximate at all. If the right cord is fixed, what is the cause? Perhaps there is some long-standing condition which accounts for an over-action of the healthy cord.

Dr. H. J. BANKS DAVIS: I did not think the right cord moved. The left cord seemed to be at a higher level, and swung across the arytænoids.

Captain SMURTHWAITE: I agree with what Dr. Banks Davis said, and I therefore ask if he has had chest symptoms or anything involving the nerve. There is hyperaction of the left cord, so that it comes behind the right cord, nearly touching it. The right cord seems almost fixed in the cadaveric position.

Dr. W. HILL: The right cord shows post-paralytic contracture.

Dr. BROWN KELLY: At first glance it looked to me like "crossing of the arytænoids," which occurs physiologically, as exemplified in Case II, shown by Sir StClair Thomson. In the case under discussion, however, the cords do not quite meet: it is difficult to say which is at fault, but as the left arytænoid is inclined forward, as in recurrent paralysis, probably the lesion is situated on this side of the larynx.

Dr. DONELAN: Sir Morell Mackenzie showed me a case in which the arytænoids overlapped in this manner, and he said it was very rare to see it in a normal larynx.

Dr. PEGLER (in reply): In spite of adverse criticism to my stated opinion I am disinclined to change it, so long as I realize that members who differ from me have not had the opportunity to watch the movements in this patient's larynx as leisurely and carefully as myself. I feel further strengthened owing to the independent corroboration of a competent laryngologist to whom I referred the case. We both believed that we have seen this peculiar overlapping by one arytænoid cartilage over another before. I am glad, however, that at least one member who has spoken (Dr. Donelan) agrees with this view, quoting Sir Morell Mackenzie as having described such a condition, though rarely seen in a normal larynx. I hope to show the case again in the autumn session if any clearer evidence of palsy becomes manifest to me.

(*June* 1, 1917.)

Epithelioma of the Larynx, in a Lady aged 46, Ten Months after Laryngofissure.

By Sir STCLAIR THOMSON, M.D.

PATIENT had been hoarse for no less than one year and four months when she presented herself in August, 1916, with a rough, pale-red, cauliflower-like infiltration of the right vocal cord, involving the posterior two-thirds, and most marked over the vocal process. The movement of the cord was sluggish. As the growth projected well above the surface, a portion was removed by the indirect method with Mackenzie's forceps and reported on as follows: "This tiny fragment shows the histological structure of an undoubted epithelioma."

Laryngofissure on August 9, 1916. The growth was found to extend right back to the arytænoid, and the naked-eye examination showed that it must have extended close to the arytænoid cut. A further portion was therefore removed from the arytænoid. Tracheotomy tube was removed at the end of operation. The report of the microscopic examination was as follows:—

The specimen comprised four pieces of tissue:—

(1) Marked "main growth." This piece has been cut in three pieces—anterior edge, posterior edge, and through the centre. In the centre is a typical epithelioma, the columns of cancer cells penetrating into the tissues as far as the small muscles. The removal is wide of the growth above and below, and towards the perichondrium and anteriorly, but not posteriorly, where the growth extends to the arytænoid region.

(2) Marked "round the arytænoid." This piece in all probability completes the removal of the growth, for although the cancer is to be seen along one edge of it there is none on the other edge.

(3) Marked "deep perichondrium." This piece consists of small muscle, blood-vessels and areolar tissue. There is no sign of the growth in it.

(4) Marked "aryepiglottic fold." This piece consists largely of mucous glands. It is free from growth.

Ten months have passed. The patient has a good voice.

The following are interesting points in this case:

(*a*) The patient is a female and comparatively young. I have only performed laryngofissure for epithelioma in two female patients in comparison with over thirty males.

(*b*) The invasion of the arytænoid region involved extensive removal, and this has been followed by some cicatricial contraction and slight glottic stenosis.

(*c*) The voice is as good as before the operation, and there is only slight dyspnœa on exertion.

N.B.—The patient has not been inspected from March 5 until today (June 1).

DISCUSSION.

Dr. JOBSON HORNE: It is difficult to discuss this case, as we have so little to go upon. We have seen the patient, who is cured, but we have not seen the growth. I have been a histologist in my time, and I have got into hot water for not seeing carcinoma of the vocal cords when it ought to have been there. And I think I can say, without hesitation, that there is no disease more frequently diagnosed where it does not exist, than cancer. We should like to have an opportunity of seeing the growth which was removed from this patient, so that we may be able to revise our stereotyped notions on epithelioma of the vocal cords.

Dr. W. HILL: Sir StClair Thomson is to be congratulated on the case. As he says, the voice is as good as before the operation.

Sir STCLAIR THOMSON (in reply): I welcome Dr. Horne's suggestion: in fact I shall be glad to submit the specimen to a Morbid Growths Committee, or to a subsequent meeting of the Section, because I think something can be learned from these cases—I have had thirty-two cases, and have the sections of all of them. Some were so slow of growth that there was as much as a seven years' history of hoarseness before I saw the patient. Others I have myself watched for a whole year. I recently operated upon a case, which I watched for a year, and yet when it came to laryngofissure I was astonished to see how small was the nodule of growth. This very slow growth of epithelioma of the vocal cords must be a subject of pathological interest to morbid histologists, and some light might be thrown on the whole subject of cancer by the study of these sections. Perhaps they might all be submitted to a Committee who might give us a report upon them. I am rather disappointed over to-day's case: it is only the second in which the patient complained of some stenosis, and there is no doubt that she does not get enough air through her larynx. This growth was close to the arytænoid, and when I removed it I examined it with the naked eye and with the touch, and felt that the growth went so near the posterior extremity of the cord that I must remove some more, so as to be sure of having gone wide of the growth. One of the reports shows it was fortunate that I did so. Then I got nervous about recurrence, and removed a little more still, which proved to have been unnecessary. I fear she will have to have a tracheotomy tube inserted and wear it permanently, as it is not worth while to put her to the trouble of wearing an intubation tube. The case shows the liability to stenosis when the arytænoid end of the cord is extensively removed, but it is a condition in which one may be excused causing stenosis if the patient's life is saved.

(*June* 1, 1917.)

Tuberculosis of the Larynx, in a Gentleman aged 63, under Treatment with the Galvano-cautery.

By Sir StClair Thomson, M.D.

This gentleman was admitted to a sanatorium with tubercle bacilli in his sputum, and mischief in both apices, so that he was included in Group II. There was a deposit with deep ulceration in both vocal processes, more marked on the left and extending along two-thirds of the corresponding cord. After two months in a sanatorium he had his first treatment with a galvano-cautery in November, and this was repeated in January, March and April. When last inspected two weeks ago, the right side had completely healed, and it was doubtful whether the slight catarrh still remaining over the left vocal process was due to tubercular deposit or to the scar of the cautery.

Patient still has tubercle bacilli in his sputum, but the advantage of arresting mischief in the larynx is important, as he is by profession a tutor.

(*June* 1, 1917.)

Extensive Tuberculosis of the Larynx, Completely Cicatrized under Sanatorium Silence and Galvano-cautery Treatment.

By Sir StClair Thomson, M.D.

Mr. B. W., aged 29, a schoolmaster, had suffered from general symptoms for fifteen months and hoarseness for three months, when he was admitted to a sanatorium on April 29, 1915. He then had a temperature, tubercle bacilli in his sputum, and lesions of two lobes in each lung. He had tubercular deposit in the epiglottis, both arytænoids, and both ventricular bands, with ulcerating infiltration along the edge and inner surface of both vocal cords. The left vocal cord was nearly fixed. The patient was put on strict silence.

Progress at first was slow and it was not until March 24, 1916 (i.e., after twelve months' sanatorium treatment) that the left cord became mobile, and not until June, 1916 (i.e., until fourteen months' watching) that the infiltration of the epiglottis and arytænoids had subsided.

The galvano-cautery was applied in July, August and September of 1916.

It will be seen that the larynx now appears completely healed and free from deposit or abrasion. There is much scarring of the larynx and the rough voice is chiefly produced by the vocal bands.

When the patient came under observation first in April, 1915, he had an otorrhœa on the right side which had come on painlessly. The drum is now closed, but recently he has developed a similar condition on the left side. It is likely these lesions are also tubercular.

It will be noted that the case has required much time and patience. The patient is still living up to sanatorium principles and supporting himself by rearing chickens. He has been at his "cure" for two years. It took fourteen months before his larynx was ready for the cautery. It has now been cicatrized for seven months, but he still has tubercle bacilli in his sputum.

DISCUSSION.

Dr. Brown Kelly: I should like to ask Sir StClair Thomson whether we are to gather from this case that he is an advocate of the use of the galvano-cautery in the treatment of laryngeal phthisis in patients whose general condition is fairly good. Shortly after I began practice, the surgical treatment of laryngeal phthisis was boomed. I went to Warsaw and saw Heryng remove large pieces of infiltrated arytænoid, ventricular band, and epiglottis. You will remember that he came to London and, in his forceful manner, told us of the value of his method. Papers were also translated and published in the *Journal of Laryngology* giving his statistics of cures and improvements. I mention these facts to show that although my present attitude is one of very restricted interference, I was brought up otherwise. Heryng's treatment is now limited almost entirely to the removal of the diseased epiglottis for the relief of dysphagia. Is the galvano-cautery going to fare better? In Scotland we see many cases of tuberculosis of the nose, and in my experience, in spite of thorough treatment by curetting and applying the cautery and various medicaments, it is very difficult to effect a cure. Now, if we have difficulty in the nose, where the disease is so accessible, and in patients whose general condition is favourable, what can we expect in the larynx? I shall be glad if Sir StClair Thomson's great experience in this matter allows of his contradicting what I have said.

Dr. BARDSWELL: It should be stated that in these two cases the treatment by galvano-cautery followed an extended course of sanatorium treatment. Both these patients were treated in the King Edward VII Sanatorium. Case II was one of acute pulmonary tuberculosis involving the upper and lower lobes of both lungs; in the larynx there was also advanced tuberculous disease. On admission, the patient had marked fever and was in poor general health. It was not until twelve months later that Sir StClair Thomson operated upon him. During this period, the patient's condition had improved most materially. He had gained $1\frac{1}{2}$ st., which more than restored him to his normal weight: he had lost his fever and was doing heavy work as manager of the sanatorium poultry farm. At this date, he was a case of apparent arrest of pulmonary tuberculosis, although tubercle bacilli were still in the sputum. The laryngeal disease had improved extraordinarily, as the result of general treatment and prolonged vocal rest, but still showed evidence of active disease. The time had now arrived for the use of the cautery. The result you have all seen. After following Sir StClair's work at the sanatorium for some six years, I have come to look on the galvano-cautery, in most cases, as a means of completing the work of the sanatorium. Invariably, in a seemingly suitable case, Sir StClair Thomson, before deciding to operate, asks me—"What is this patient's condition and prospect from your point of view?" If I can say that the patient has lost all symptoms of active disease, and can take considerable exercise without producing fever—in other words, that he is on the rising tide of convalescence, local and general—Sir StClair Thomson goes forward; otherwise he does not interfere. I have had cases of active consumption sent to me from London and elsewhere in which the cautery had been applied with deplorable results. So far as I can judge, the galvano-cautery is likely to benefit only when the patient is able to live under good hygienic conditions and when what we may term natural cure has commenced both in the lungs and in the larynx. This case also illustrates one of the difficulties of a consumptive. It is rarely that a patient can afford anything like two years of sanatorium treatment such as this patient has enjoyed. He owes his long residence to his having been appointed to the charge of the sanatorium poultry farm. But for this prolonged sanatorium treatment I am convinced that Sir StClair Thomson would never have had the occasion, probably not even the opportunity, to operate on his larynx. Briefly to summarize my experience: I would say that to apply the cautery to an advancing lesion in the larynx results usually in an aggravation of the condition. For success, the cautery should be used in association with the best possible general conditions, preferably in a sanatorium, and only in cases in which natural cure is in progress.

Dr. JOBSON HORNE: I would go a stage further than the last speaker has gone, and say that if a patient has proper sanatorium treatment the cautery is not required at all—that is to say, the disease of the larynx invariably goes *pari passu* with the disease in the lungs. Given an ulcer in the larynx, you may be sure the man has at least one cavity in the lungs, and if you can heal that, the larynx will heal. It is best to leave the larynx alone, once it has commenced to heal. It is a pity to bring into the discussion cases of tuberculosis of the nose, because those form a different category.

Dr. KELSON: While agreeing as to the importance of sanatorium treatment, I cannot agree with these two gentlemen, because I have seen cases cured by galvano-cautery other treatment having failed, and I have a case attending the hospital who was at Ventnor and his lungs were cured there, but they could not get his larynx right. At the hospital with cautery treatment, his larynx quite healed. It is true he has had recurrences at intervals of six or seven months, but the cautery cures these recurrences. Seven or eight years ago I showed a case at this Society apparently cured, the patient being a postman. He had not had sanatorium treatment, but the galvano-cautery brought about what appeared to be a complete cure. [Dr. JOBSON HORNE: Was there a history of syphilis? It does not sound like pure tubercle.]

Mr. CLAYTON FOX: I have seen for some time at the Brompton Hospital, cases of tubercular infiltration of the larynx treated by galvano-puncture, and I can say without hesitation, the results on all occasions have been good. The cases were not acute ones. In a case I had in private practice lately, the patient had pain in his ear from an infiltration of one arytænoid and ventricular band. He had undergone all kinds of treatment: insufflations of carbamide powders, and alcoholic infiltration of the superior laryngeal nerve, but even this only gave relief for a time. I applied the puncture cautery at intervals of ten days, fairly deeply, making four punctures at a time. In a short time the arytænoid resumed its normal size and appearance. I have seen a good deal of treatment of tuberculosis of the larynx by the galvano-puncture, and I can say it appears to be excellent treatment, provided the patient has not a continued high temperature—in other words, provided the tuberculosis is not exceptionally active. There must, of course, also be as much hygienic treatment as can be brought to bear on the case, and this should come first: but galvano-puncture, as far as I have seen, has been absolutely successful.

Sir STCLAIR THOMSON (in reply): This is such a wide subject that one cannot, in a short reply, express oneself with all the saving clauses which are necessary, especially as there is nothing more kaleidoscopic than tubercle. I am much obliged to Dr. Bardswell, because he has answered the chief points. The galvano-cautery should only be brought in when the case promises to, at least, become arrested. it is of no use putting patients to the trouble and annoyance of it if they are likely to die of their tubercle in two or three years; because death in these cases comes from the disease in the lungs. The two patients I have shown to-day still have bacilli in their lungs, and very likely they will die before their due time. But, in the meantime, they can use their voices—an important matter, as one is a schoolmaster, and the other a tutor: so that they can enjoy the great mental exhilaration of being able to talk—a matter which some members of this Section will appreciate! The indications for which I have been asked were given in Dr. Bardswell's remarks. I cannot agree with Dr. Jobson Horne when he says disease of the larynx goes *pari passu* with the disease of the lungs. In some cases that may happen, but the lungs may improve and yet the larynx may be going down-hill. On the other hand, the larynx may so improve under the hygienic conditions, that, with silence, Dr. Bardswell is able to tell me I can go ahead with the cautery. Or he may say the patient is still erratic, that he has little bouts of temperature, that he cannot stand work, that he is not putting on weight, that he is discontented—an important matter—and then I do not use the cautery, although the larynx seems to require it. And I do not agree with Dr. Horne that these cases get well without the cautery, for I have watched cases for years after they have left the sanatorium. I have shown here a clergyman who was in the sanatorium where I was a patient fourteen years ago. He tried eighteen months' silence, and could not get arrest in his larynx. At last I applied the cautery, and within a year he was again preaching and enjoying it. I have not looked through my records, but I do not think I have had, in private, thirty cases of lasting cure of laryngeal tubercle, whereas I have had over thirty cases of malignant disease, and I would sooner have a malignant growth on the vocal cord than I would have tubercle of my larynx, because the cases of the latter which are suitable for the galvano-cautery and cure are few and far between. As for doing it on patients in an average clinic who have not gone through sanatorium treatment, I would not embark on it at all. I agree with Dr. Bardswell, that the chief necessity is to get the patient's resistance raised. I do all my applications by the indirect method.

(*June* 1, 1917.)

Male, aged 22, who presented himself with a Saddle-back Nose, consequent on a Resection of the Septum, performed in the Country.

By Sir StClair Thomson, M.D.

PATIENT presented himself because of his nasal deformity and unrelieved nasal stenosis. For the latter complaint he reports that he had an operation on his septum under chloroform; gauze plugs were kept in for forty-eight hours, and at the end of that time he left hospital. On his return home the same day his friends noticed the falling in of his nose, and this increased during the week, at the end of which time he states it was as marked as it is now. Unfortunately, he never returned to his hospital to report himself or secure treatment.

He has a marked saddle-backed nose apparently from collapse of the septal cartilage. The septum is intact and plumb in middle line. There is a puckered scar on each side, suggesting that the deformity may have resulted from a septal abscess which was not evacuated in good time. No cartilage is to be felt anteriorly in the septum. There is free space on each side. The patient had a large pad of adenoids and very follicular tonsils. These were removed by Dr. Irwin Moore on April 1. It is now proposed to try to remedy the external deformity by a subcutaneous injection of paraffin. The patient is shown before this is done to elicit opinions on the cause and prevention of the collapse of the bridge of the nose. I have never had a case of collapse although I have practised this operation since the year 1903.

DISCUSSION.

Mr. HARMER: I am interested in the question of the treatment of depressions of the bridge of the nose. At St. Bartholomew's Hospital we had a large number of cases of paraffin injections in the nose, performed many years ago by Mr. Walsham, and at the time they looked good results. But later some of them returned in a bad state. One man, for instance, had so much fibrosis resulting from his injection of paraffin that the nose was thickened to more than twice its original size, and he no longer had binocular vision when looking straight ahead. I should like to ask whether other members have seen bad results from injections of paraffin. Of recent years we have abandoned paraffin injections at the hospital in favour of transplantation of bone or cartilage, which can be obtained from the seventh or eighth rib, and can be accurately shaped, so as to make an almost perfect nose. If necessary, more than one piece can be placed in the nose—a central fragment and two lateral ones. If suitable care is observed in transplanting the cartilage there is no reaction, either at the time or at a remote date, and no scarring, because only a puncture incision is necessary. If the nasal cavity is aseptic, cartilage can even be implanted from inside.

Mr. NORMAN PATTERSON: I have seen very good results follow taking the septal cartilage for the purpose. It is removed by submucous resection.

Dr. H. J. BANKS DAVIS: What is the cause of the falling-in of the bridge of the nose? I never operate upon any lady who is at all good looking without telling her or the husband that there is a chance the shape of the nose may be altered, and often that statement deters them from having it done. In this case the cartilage seems to have been removed too high up. If that is done, I think that it is due to wrenching away of the cartilage, instead of using sharp cutting forceps, which is liable to displace the lateral cartilage. It is not possible to alter this by paraffin; all you may do is to make the nose broader and flatter.

Dr. W. HILL: Submucous resection should not be accompanied by any deformity; I do not expect it in my cases, and so I do not do paraffin injections.

Mr. DAWSON: Sinking-in of the nose is not very rare. In the last five or six years I have seen five cases after the Killian operation which was done by men of repute, cases in which the end of the cartilage has sloughed or been removed. The patients complained of dropping in of the end of the nose, and on putting the finger on the end of the nose it sank in easily. With regard to the operation for repair I have a case now, into whose nose I introduced a piece of cartilage from another patient's septum. It did not do at all well.

Dr. DONELAN: Leaving the packing in for forty-eight hours appears to have had much to do with the unfortunate result in this case. I have notes and drawings of over 5,000 septums upon which I have operated since 1889, my first case having been done without adrenalin. I have never had a septal abscess or seen such a result as this. I do not think operation on the cartilage alone remedies the nasal obstruction, as the most important deformity occurs at the chondro-vomeral suture, and in this instance the removal has been carried too far upward and not far enough back. I have used paraffin with good results, mostly in syphilitic cases, but also in some four others in which the sinking-in of the nose was the result of injury, as from foreign bodies, during childhood. The paraffin injections were followed sometimes by fibrosis in the syphilitic cases but not in the others. If paraffin is injected in this case I should recommend supporting the sides of the septum by means of

Adams' forceps or by a small screw clamp during injection, which might be done from within or without.

Dr. JOBSON HORNE: This is a very big subject, and I think we ought to have a stocktaking and a review of this subject of submucous resection. The question is whether this treatment should be continued at all.

Captain SMURTHWAITE: During the last year I have examined many noses on which the submucous resection operation had been performed. In most of the cases too little cartilage had been removed. In the present case too much has been taken away. Most men, when doing the operation for the first time, do not remove sufficient, so the obstruction is as before the operation. I have not previously seen a falling-in of the nose following the resection operation.

Sir STCLAIR THOMSON (in reply): As to the cause of this falling-in, several explanations have been suggested. I consider that it is not a question of too much or too little removal, but that the danger in these cases is the formation of a septal abscess which has not been recognized promptly. In early days I was surprised to find what could be kept up by a narrow strip of cartilage. I have had some cases of hæmatoma which have suppurated, and I have been afraid on discovering an abscess of the septum. But by free drainage, in good time, I have never seen collapse occur in any case. It is what may have happened in this case. We should tell our patients, both private and hospital, to be sure to report, because in such an event we can open the abscess and stave off collapse. I am interested in hearing Mr. Harmer's remarks, because Mr. Walsham was one of the pioneers in paraffin injections, and it is highly interesting to know that the ultimate results are not always good. I will have a photograph of this boy taken, and show him again in a few months.

(*June* 1, 1917.)

Lupoid Tuberculosis of the Nose and Larynx in a Girl aged 17.

By IRWIN MOORE, M.D.

I FIRST saw patient on February 16, 1915, when she complained of hoarseness of the voice for two months. The nasal septum and right inferior turbinal showed marked lupoid infiltration, whilst the left nasal cavity appeared to have been less affected and recently healed. Both nasal orifices were somewhat contracted. The glottis was considerably narrowed, and the left vocal cord was partially fixed, causing inspiratory stridor on exertion. The right arytænoid, interarytænoid region, and upper margin of the epiglottis were infiltrated, whilst the left arytænoid was ulcerated on its inner surface—as also the edges of both vocal cords. The lungs were normal. Patient's weight was 7 st. She was advised sanatorium treatment, and went to Torquay in October, 1915, for six months, where she was told that there was no disease of the larynx, and that she would get back her voice suddenly.

I did not see the patient again until May 29, 1917. The hoarseness is no better, though the general health has improved. There has been no marked change in the laryngeal or nasal condition during the past two years. There is a nodular infiltration of the epiglottis. The glottis is still very narrowed, and the left vocal cord ulcerated, whilst both the aryepiglottic folds and interarytænoid region show distinct infiltration. The left choana is occluded by adhesions, there being only a very small aperture present, whilst a small patch of infiltration is seen on the roof and lateral wall of the nasopharynx. Patient's weight is now 7 st. 4 lb.

A recent water-colour drawing of the larynx is shown.

(*June* 1, 1917.)

Result of Treatment by Diathermy of Extensive Epithelioma, affecting the Soft and Hard Palate, Tonsils, and Faucial Pillars.

By NORMAN PATTERSON, F.R.C.S.

THE growth would have been quite inoperable by any other means. It was removed entire. A portion of the hard palate is evidently in a state of necrosis, and will ultimately separate. This shows that destruction of tissues has taken place well beyond the actual parts removed. The patient has indulged freely in tobacco and alcohol, and has been in the habit of drinking very hot tea.

DISCUSSION.

Dr. W. HILL: Mr. Patterson dissected this out with the diathermy point. I was not surprised to learn the time the operation took, because it is a slow process.

Mr. FRANK ROSE: I noticed a somewhat large area of bare bone exposed. In doing diathermy in these cases of inoperable malignant disease, I have noticed that if the disease has already penetrated into the substance of the bone, the result is disappointing, even if one applies the heat so as to cause a fairly deep necrosis of bone. I wonder if Mr. Patterson has had similar experience.

Mr. NORMAN PATTERSON (in reply): The first case in which I removed a growth entire was one of malignant disease of the tongue and floor of the mouth. The growth, together with a good margin of healthy tissue, was cut out by using the diathermy needle instead of a knife. The removal was facilitated by the employment of scissors to cut through the tissues as soon as they were thoroughly "cooked." If the area does not contain any large vessels a blade with a fairly sharp edge may be used (e.g., in dividing the tongue down the centre). After removal of the mass of tissue containing the growth I always go over the whole surface of the wound with a button shaped instrument; this increases the chances of the growth being completely destroyed. With regard to cases in which bone is involved, I agree with what Mr. Rose has said, and I can recall grave disappointment in a case of malignant disease affecting the upper jaw. In the patient shown, I am quite certain that the disease did not involve the bony palate, but was confined to the mucous membrane. Notwithstanding this fact, I am glad to see necrosis occurring, as it shows that destruction has taken place well beyond the diseased area.

(*June* 1, 1917.)

Case for Diagnosis.

By GEORGE BADGEROW, F.R.C.S.Ed.

MALE patient, aged about 38. Web extending from the soft palate to the pharyngeal wall; probably due to diphtheria in early life. It did not cause any inconvenience, and it was discovered by chance.

The PRESIDENT: I looked upon it as cicatrization following ulceration. The patient was very ill with scarlet fever when he was aged 4.

Section of Laryngology.

President—Dr. A. Brown Kelly.

(*November* 2, 1917.)

PRESIDENT'S ADDRESS.

(ABSTRACT.)

By A. Brown Kelly, M.D.

(The President having pointed out that it was a fitting time to consider whether the Section was furthering in the best possible manner the progress of laryngology, proceeded to discuss its present and possible future relations to investigation, teaching and publication, said:—)

First, with reference to original investigation, I should like to impress upon the younger members of the Section that for the solution of the more purely scientific questions we are mainly dependent on them. If they are still in touch with the anatomical, physiological or pathological department of their medical schools they can obtain material and opportunities for research work.

The problems to be solved are innumerable. In the anatomical department the following subjects might be studied: The innervation of the soft palate and the causes and effects of paresis of its individual muscles; the histology of the olfactory region with reference to the changes causing anosmia, including if possible the transitory type, which is a feature of vasomotor rhinitis; a comprehensive description of the anatomical variations met with in the upper air tract; this last might serve as the basis of a useful chapter in all text-books on diseases of the nose and throat.

In the physiological laboratory work might be done towards the elucidation of the functions of the inferior turbinate, and its variations in bulk and secretory activity. An instrument indicating the amount of air passing through each nasal fossa is still a desideratum. We know that nasal hypersecretion is sometimes checked by calcium salts, but we are still unable to identify the cases in which these drugs are likely to prove useful. Information on such points would help to raise the treatment of the so-called nasal catarrh from the region of empiricism to that of scientific exactitude. The whole subject of vasomotor instability is a mystery, the unravelling of which, however, is unlikely in our present state of knowledge. It is not improbable that vagotonia may account for at least some of the nasal neuroses. Every day we are questioned as to the use of tonsils and adenoids and the cause of their enlargement; and often we have reason to ask ourselves why the removal of a comparatively small mass of adenoids is followed by a physical and mental transformation quite out of proportion to the improvement in the respiratory conditions. Did the adenoids set free a deleterious internal secretion, or inhibit a beneficial one? or, how else are we to explain the great change? All these questions suggest matter for investigation.

We are still in need of post-mortem examinations of the central nervous system in neuroses affecting the larynx alone or in association with other regions.

In Poor-law hospitals and asylums permission might be obtained whereby laryngological and other cases of special interest could be observed throughout life and the morbid tissues examined subsequently.

Of the relations of diseases of the eye to those of the nose much has been written in recent years, but more information on the subject is required.

Lastly, to any member with opportunities of seeing diseases of the skin, I might mention that we are still in want of an account of the dermatoses as they affect the mucous membranes of the mouth and throat.

In any of these directions a young medical man with limited clinical material could make valuable contributions to our specialty, and as an incentive to research work the Section might announce that applications for membership accompanied by a thesis would receive special consideration.

An increased production of original papers would allow of the enlargement and improvement of the *Journal of Laryngology*.

Having indicated some lines along which investigations may be pursued, let us consider whether the Section gives adequate encouragement to work of this kind.

At present, all communications made to this Section must be based upon a patient, specimen or exhibit of some kind, with the result that those living at a distance from London, and therefore almost precluded from showing cases, are unable to contribute to the most interesting part of the proceedings.

According to an unwritten law of the Section notes for the agenda-paper must be as brief as possible. Most of the old members will agree with me in ascribing the success of this Section largely to the practical policy insisted upon in the past. I regret to have to point out, however, that this very conciseness has been seriously detrimental to the study and progress of laryngology in this country. Many of the rare and interesting cases that have been brought before the Section should have formed the basis of extensive investigations, or of analyses of all previously recorded cases of the same nature.

These considerations make it obvious that this Section, although it has attained a high measure of success in relation to practical laryngology, might benefit further by encouraging original investigation and by gathering to itself some of the work done in the provinces and colonies, and abroad. But how are these objects to be accomplished without damaging established and proved usages? Any proposal to have papers read at the meetings would, I am sure, be at once negatived by the majority of the members.

Two ways of settling the difficulty suggest themselves. First, a member desirous of bringing a paper before the Section might submit it to the Council; if approved, it would be printed and sent to the members with the programme of the next meeting; the title only, or, at most, the conclusions, would appear on the agenda paper; at the meeting, the subject, in turn, would come up for discussion; and the paper would afterwards be published in the *Proceedings*.

The second method would be once a year to devote an entire day—say the first Friday in May—to the work of the Section. In the forenoon, papers which had previously been printed and circulated amongst the members would be discussed; in the early afternoon the museum and display of instruments would be inspected; later, the Annual Business Meeting would be held, followed by an ordinary meeting with the exhibition of cases, &c., and in the evening the dinner of the Section would take place. A laryngological gathering such as this would prove an attraction not only to provincial members but also to our foreign confrères; it might even serve as an international congress of rhino-laryngology.

If the Otological Section resolved to institute a similar annual reunion the meetings of the two Sections might be held on successive days.

Laryngologists with whom I have spoken on the subject have all expressed the opinion that the teaching of our specialty should now be improved and extended. Consideration of the matter and necessary action lie, of course, with the individual centres or schools. Our young graduates and those of our allies will have no wish to study, as has been the fashion, at the birthplace and early homes of laryngology; their needs, however, can be amply provided for in London. The metropolitan members of the Section whom these matters directly concern might find it suitable to take their initial steps here. If a great scheme of instruction is elaborated for London, might I suggest that the students be sent occasionally on a provincial tour?

I trust that at an early date the question of granting a diploma in laryngology will be raised. A qualification of this kind would be a stimulus to teachers and to the more exact study of the subject, besides affording a ready means of distinguishing the trained from the self-styled specialist.

The last matter of which I would ask your consideration is the production of a comprehensive and advanced treatise on the diseases of the upper air and food passages. Several members of the Section have written on the subject books of which we may be justly proud, for they are unequalled by publications of like aims and scope in any language. The work which I foreshadow is, however, quite beyond the productive power of any one man and could be evolved only as the result of the experience and industry of many. If, as we trust, our language is to be that of the future, it is incumbent upon English-speaking laryngologists to plan and carry out such a work. In these circumstances would it not be fitting that the Section undertake its supervision and accept the monetary responsibility?

I shall conclude my remarks by emphasizing their twofold aim. I have tried to show that this Section of a great Society, in addition to its functions at home, has certain imperial and international obligations, one of the first of which is to open its portals to our overseas and foreign brother specialists and to extend a welcome to their work, also that it can profoundly influence the destinies of laryngology in this country by fostering research, organizing teaching, and subsidizing approved publications—in short, by becoming the Alma Mater of laryngology.

(*November* 2, 1917.)

Fusiform Dilatation of the Œsophagus, coated with *Oïdium albicans*, in a Girl, aged 15, and apparently Idiopathic.

By Sir StClair Thomson, M.D.

W. R., aged 15, was admitted to Lister Ward, King's College Hospital, on April 21, 1917, with a suspicion of an œsophageal pouch. Symptoms started one year previously with violent fits of coughing and regurgitation of food. She had no difficulty in swallowing and ate anything, but, shortly after each meal, food was regurgitated into her mouth. Condition varied: for fourteen days she might have little or no difficulty in retaining everything, and then for days she might be unable to get any food down, as regurgitation followed fast on every effort to swallow. It was rare for food to be regurgitated which had been swallowed on the previous day. No blood. Discomfort referred to middle of sternum. No loss of weight or strength and patient is particularly vigorous, well grown and intelligent. Patient has had every single tooth in her upper jaw removed, as it was thought that her trouble was due to pyorrhœa.

Present condition: On ordinary examination there is nothing to be detected. Examination on the screen shows that a bismuth feed passes freely to the middle of the œsophagus, where it is held up for a short time. Plate examination shows a definite obstruction at cardiac end of œsophagus, with considerable dilatation above the obstruction, and some irregularity of the shadow about the middle of the œsophagus. Œsophagoscopy is particularly easy and shows an evenly and widely dilated, fusiform gullet, irregularly coated with extremely adherent plaques of dirty white material, like curdled milk. On removal, this material was reported to consist of a mass of *Oïdium albicans* with other bacteria. No pus, blood, or fragment of growth.

A series of lavages, including peroxide and eusol, were carried out in May and the patient was relieved. She has again come under treatment and remains much *in statu quo*.

DISCUSSION.

The PRESIDENT: As Sir StClair Thomson has put this case down as fusiform dilatation, and has not used the terms "achalasia," "cardiospasm," or "atony of walls," we should, in the discussion, keep to the clinical side. I hope that in the course of the Session cases of this class will be specially considered some afternoon, when members will have an opportunity of expressing opinions as to the ætiology.

Professor SHATTOCK, F.R.S.: The whole interest of this case lies in the fact that the interior of the œsophagus is coated with oïdium. The subject of œsophagectasis was so fully discussed last year, in the Medical Section, that I hardly think it can be necessary to go into the matter again now.

Sir STCLAIR THOMSON: I shall be glad to leave out what might complicate the discussion, as to why the œsophagus is dilated, whether it is the consequence or the cause of the *Oïdium albicans*. But the comfort of the patient is a consideration, and this patient has come up twice from Southampton to be cured, and I have failed! Therefore I shall welcome any suggestions. A physician suggested that the oïdium occurs in the mouth when there is an excess of alkalinity. Therefore we washed out this case with dilute hydrochloric acid, and we have used boric acid, peroxide, and painted it with nitrate of silver. She has had the condition since April.

Dr. DAN MCKENZIE: I should be inclined to see what would be the effect of a piecemeal removal of the accessible part of the fungus, followed by a rubbing in of some such substance as tincture of iodine. I have seen such a condition in the pharynx of adults not infrequently, but usually in persons who were dying. In them one noticed that it produced a peculiar interference with swallowing. It was obvious that the growth had made its way amongst the muscular fibres in the neighbourhood, producing, no doubt, an interference with muscular movement. The dysphagia in this case may be of that nature.

Mr. MARK HOVELL: I have never seen a case similar to this one. I think a remedy of a penetrating nature is required. For that reason I would try collosol iodine, or collosol silver, or collosol copper, locally. Collosol iodine and collosol argentum is made up in suppositories, and these could be passed down to and lodged in the œsophagus, and being non-toxic are quite harmless; 2 to 4 dr. by the mouth is a dose. If that did not succeed, I should try a germicide solution, such as cyllin.

Mr. HERBERT TILLEY: Before trying ionization, I should advise the free application of a weak solution of sulphurous acid: it seems very effective in the destruction of various forms of "mould."

The PRESIDENT: Do members think the two conditions here are related, that is, is not the oïdium accidental? I have met with the oïdium in the gullet on several occasions, but not always associated with fusiform dilatation. I would be inclined to treat the fusiform dilatation first and see whether the oïdium did not afterwards disappear. The treatment I have employed for the underlying affection, the so-called cardiospasm, has been dilatation of the cardiac end of the gullet. This has occasionally effected striking benefit, patients who were practically invalids having been restored to perfect health, and others having been improved for six months, or a year, when the dilatation had to be repeated; only in a few cases has no improvement been obtained. The instrument used was Gottstein's dilator, the bag of which is distended by pumping in water. Other dilators are on the market –e.g., Brünings' and Plummer's.

Dr. JOBSON HORNE: We have got somewhat off the line in attaching so much importance to the oïdium, and so little to the stricture. Does Mr. Shattock consider the oïdium a causative factor, or is it merely a coincidence?

Professor SHATTOCK, F.R.S.: I entirely agree with the President that the growth of the oïdium or *Blastomyces albicans* is a pure epi-phenomenon: that it is not causally connected with the dilatation of the œsophagus. It occurs, probably, from some accidental infection having taken place in a passively dilated tube. As to the depth to which growth penetrates, I must protest against assuming that in this case it penetrates into the muscular wall of the œsophagus. I believe the growth is confined to the mucosa, and dilatation of the cardiac orifice would probably cure the infection. The fungi of proper blastomycotic granulomata belong to a different species from that of common thrush.

Sir STCLAIR THOMSON (in reply): I agree the case might form the basis for a much larger discussion. I do not know what to dilate, because, although the cardiac end of the œsophagus will not let food through, yet there is no obstruction. You have seen how the patient can herself pass a feeding-tube. We cannot get forward with the treatment until we are agreed as to the pathology. Is this a spasm of the cardiac end, or atony of the œsophageal muscles. Am I to dilate the cardiac orifice, or stimulate the constrictor peristaltic muscles? I will try to do both, and report.

(*November* 2, 1917.)

Epithelioma of the Ethmoidal Labyrinth; Lateral Rhinotomy; Removal.

By DAN MCKENZIE, M.D.

THE patient is a woman aged 41 who was sent to the Central Throat and Ear Hospital by Dr. Cowie, of Denmark Hill, in July of this year. The only symptom was epistaxis. But as this had been continuing for about four years in ever-increasing severity she was pitiably anæmic. The growth was polypoid and occupied the left ethmoidal region and diagnosis was made by Dr. Wyatt Wingrave microscopically before operation.

In view of the grave anæmia, efforts were made to complete the operation as rapidly and with as little hæmorrhage as possible. The inner aspect of the nose under the nasal bones was therefore liberally treated with adrenalin. And after the external incision and manipulations had cleared the bone, the lower edge of the nasal bone was defined and a septum elevator introduced below it to separate it from its underlying muco-periosteum. Thus isolated the bone was rapidly nibbled away with mastoid gouge-forceps, and this method proved to be much more rapid than the use of the chisel would have been.

The rest of the operation proceeded along familiar lines, and the whole was completed in fifteen minutes with a trifling loss of blood.

The patient has made a good recovery both from the operation and from her anæmia, and at the time of writing there is no sign of recurrence either locally or in the cervical glands.

DISCUSSION.

Lieutenant-Colonel PERRY GOLDSMITH: Up to what age would Dr. McKenzie accept a patient for this operation? Probably you have had cases of sarcoma of the nose, in which intranasal operation has led to disastrous results in elderly people: there may be severe hæmorrhage, and septicæmia following it, while recurrence has been rapid. I have noticed this in several cases aged 50 and over. A patient may have sarcoma of the nose and yet live in comfort for a considerable time.

Mr. HERBERT TILLEY: What is Mr. Shattock's opinion on this case? How curious it is that these malignant growths in the ethmoid and the nose in general are often so amenable to surgery, or even, in some cases, to radium emanation! Probably in this patient the epithelioma has the same macroscopic and histological appearances as an epithelioma elsewhere, yet in the last named we would not be surprised at a rapid recurrence, in spite of radical surgical treatment, while in the nose we rather expect them to get well. At the hospital to-day I saw a woman from whom, last May, a large endothelioma was removed from the inside of the nose. There is no sign of recurrence, the pain has gone, and the hæmorrhage, which had been very profuse, has ceased. Yet it grew in a space which is practically honeycombed with cells, where one would imagine the most careful operator would be liable to overlook small extensions of primary growth. Furthermore, in the nose, it is extraordinary how 100 mg. of radium emanation, buried in the growth for twelve hours, will cause them to melt away, and they will then remain passive or disappear entirely for years, and some seem to be cured. If there is anything which Mr. Shattock can tell us as to the explanation of the different degrees of such malignant growths in different regions, we shall be much obliged to him.

Sir **WILLIAM MILLIGAN**: What forceps did Dr. McKenzie use? Possibly he used Citelli's mastoid forceps: if not, I would suggest that instrument. I agree with what Mr. Tilley said about the frequent non-malignancy of these growths in the nose, which have been diagnosed as malignant. I remember the case of a lady on whom I operated nineteen years ago, for what was pronounced, by competent pathologists, to be a round-celled sarcoma of the ethmoidal labyrinth. It occupied practically the whole of her right ethmoidal region, and the operation was performed under considerable difficulty, as the patient nearly died of hæmorrhage. Many years ago she developed a similar growth in the left nasal passage. It was so extensive in the upper ethmoidal labyrinth that its exact point of origin could not be detected. It was successfully dealt with, and again pronounced—though not by the same pathologists –to be round-celled sarcoma. I did not expect recovery, but she has recovered, and is well, with the exception, naturally, of suffering from a marked nasal atrophic condition. I have also been struck with the fact that there appears to be a type of malignant disease of the nose which, clinically speaking, appears to be a non-malignant as well as a very malignant type, the growth in the latter being surprisingly rapid. I do not know that there is any difference in the actual objective external appearances.

Sir **STCLAIR THOMSON**: This point was brought out by the late Sir Henry Butlin, who said in 1901: "Our present knowledge and prognosis in regard to this type of growth is almost impossible, some cases recurring rapidly, while others give excellent results."[1] Mr. Shattock will remember a specimen I sent him from a case which I had seen in consultation with Sir Henry Butlin in 1909. Mr. Shattock's report on the microscopical section sent was: "A typical specimen of lymph-endothelioma." I operated upon it, and now, seven years afterwards, the patient is well, and without recurrence.

Dr. **SYME** (Glasgow): This case recalls to my mind one or two similar which I have seen, bearing out what Mr. Tilley said. One I showed before the Scottish Otological and Laryngological Society, in which there was extensive destruction of the interior of the nose by sarcoma. I turned down the cartilaginous part and split the nose in the middle line. The whole nasal cavity was invaded as well as the accessory cavities. This was cleared away to the base of the skull. Although eight years ago, there has been no recurrence. Another case was one of epithelioma of the ethmoid which had invaded the antrum. I operated through the antrum and cleared away the ethmoid by this route, and intranasally. That was at least eight years ago, and beyond a slight recurrence a few months afterwards there has been no further sign of the disease. I have had an interesting case of a malignant polypoidal growth, attached by a narrow pedicle, just in front of the middle turbinate. It was epithelioma. I removed it two years ago, and there has been no recurrence.

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[1] *Lancet*, October 19, 1901.

Dr. IRWIN MOORE: A year ago I showed before the Section the case of a female aged 50,[1] on whom I had recently performed lateral rhinotomy (Moure's operation) for small-celled sarcoma of the antrum. Three months later I showed a second case, in a patient aged 62,[2] the growth being epithelioma. In each case the growth affected the right ethmoid region, and the anterior wall of the antrum, which was necrosed, the orbital floors being intact. In the sarcoma case the muscles of the cheek were also deeply infiltrated. In the epithelioma case, three months later, I found that the growth had extended under the malar bone into the zygomatic fossa and around the orbit, causing proptosis, necessitating a second operation. In April the eye was removed, since it had become disorganized. This patient died yesterday, just a year after the first operation, and the post-mortem examination showed that the growth had extended most rapidly, involved the whole side of the face, the antral and orbital walls, and backward into the sphenoid sinus. There was a large sloughing cavity in the side of the face, into which one could put one's fist. In the sarcoma case the patient is well, and shows no signs of recurrence. These two cases, under observation side by side, are interesting as tending to show the lesser degree of malignancy of small-celled sarcoma in this situation compared with epithelioma.

Dr. DAN MCKENZIE (in reply): I should not be disinclined to operate on a patient of any age. With regard to the question of the mild degree of malignancy of nasal cancers, I generally look upon sarcoma as less malignant, but when I get the name "epithelioma" given me by the pathologist, I have more apprehension. The forceps used were Jansen's mastoid forceps.

(*November* 2, 1917.)

Excision of the Upper Jaw for Carcinoma of Antrum and Palate, followed by Diathermy.

By NORMAN PATTERSON, F.R.C.S.

WOMAN, aged 56. Admitted to London Hospital on April 3, 1917. Complained of pain in the mouth for three months. Examination showed two large patches of growth on the right side of the hard palate joined together by a narrow neck; alveolus not involved.

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[1] *Proc. Roy. Soc. Med.*, 1917, x (Sect. Laryng.), p. 29.

[2] *Ibid.*, p. 60.

The antrum was quite dark on transillumination. Microscopic report by Dr. Turnbull—epithelioma.

Operation, April 5: Preliminary laryngotomy, followed by complete excision of right superior maxilla including the orbital plate which was involved. The usual method was employed, omitting altogether the horizontal incision along the lower orbital margin. I consider this incision unnecessary and by its omission subsequent œdema of the lower eyelid is avoided. By retracting the flap upwards and outwards the orbital periosteum can be separated, and the malar bone divided in the usual situation. The superior maxilla on the right side was found to be infiltrated with growth, and in many places the bone was very soft. The antrum was full of growth.

The patient's condition was rather bad and the operation had to be stopped, although it was quite certain that diseased tissues had been left behind, especially in the ethmoidal region.

A week after the operation the patient developed cellulitis.

April 25 (three weeks after the first operation): The whole surface of the wound was destroyed by diathermy, especial attention being paid to the ethmoidal region.

When seen on October 4 the patient showed no sign of recurrence. She has been fitted with a denture.

Mr. E. D. D. DAVIS: The facility with which intratracheal ether can be given for these cases is remarkable. I have had a number of cases of injury of jaws and face in addition to cases similar to Mr. Patterson's, in which this method of anæsthesia was employed, and the shock after the operation is much diminished; the patients make an uninterrupted recovery, and do well. If this patient had been given intratracheal ether, Mr. Patterson would have been able to complete the operation. Laryngotomy can be simple enough, but intratracheal ether is a great comfort to the surgeon.

(*November* 2, 1917.)

Excision of the Retro-pharyngeal Gland for Recurring Retro-pharyngeal Abscess of Tubercular Origin.

By NORMAN PATTERSON, F.R.C.S.

BOY, aged 7½. Retro-pharyngeal abscess, evidently tubercular, opened through the right side of the neck in November, 1915. Seen on March 8, 1917, at London Hospital. There was a large swelling, causing the right posterior pharyngeal wall to bulge forwards and extending past the middle line. A swelling was also present in the upper part of the right side of the neck. Operation was carried out the same day. A long incision, passing through the old scar, was made behind the sterno-mastoid and the abscess evacuated. The dissection was then carried inwards behind the carotid sheath and pharynx, and the retro-pharyngeal gland, which was much enlarged, caseating in parts and in parts calcified, was removed. The abscess wall was gently curetted and the cavity packed with gauze. Healing took place slowly. There has been no recurrence.

In September, 1917, the patient developed an abscess in the left axilla, and there is now an enlarged gland on the left side of the neck.

(*November* 2, 1917.)

Sarcoma of the Antrum.

By F. A. Rose, F.R.C.S.

H. F., MALE, aged 16, noticed obstruction of the right nostril in July, 1917; later a swelling appeared in the alveolus of the upper jaw. When seen in September the antrum was dark to transillumination. Since then the growth has invaded the zygomatic fossa and is increasing rapidly.

Pathological report: Round-celled sarcoma.

DISCUSSION.

Professor SHATTOCK, F.R.S.: We must, of course, beware of errors in pathological diagnosis. The other day I examined a section from a soft tumour of the ethmoid, which had clinically suggested sarcoma: in my opinion it was a papilloma; but others might have regarded it as a carcinoma. Clinical facts, however, are clinical facts.

Dr. WILLIAM HILL: I have treated growths like Mr. Rose's with radium. Although I have seen the antrum apparently clear up and the nose very much improved, every one of my cases has died eventually. In one case the face was bigger than that of Mr. Rose's patient. That swelling went down, but the ethmoid was invaded as well, and although I have inserted radium tubes, I have never succeeded. In one case, an old and debilitated subject—on whom, perhaps, I ought not to have tried radium—who came from Australia and whose mind was set on radium, the growth was in the top of the nose, and the cribriform was involved. I applied 200 mg. of radium bromide. On the second day he had a rise of temperature, with septicæmia, and it was evident general toxæmia had been set up by the action of the radium. He died four days later from what we regarded as perhaps thrombosis. There were enlarged veins inside his skull. Before death he had a high temperature and low muttering delirium. The dose I gave was not excessive, for only 50 mg. was applied to the top of the nose, and it was duly screened. Perhaps one ought to have applied the radium consecutively to various areas. It has been recorded that sometimes radium causes profound toxæmia. I have seen these growths disappear from the nasopharynx without recurrence.

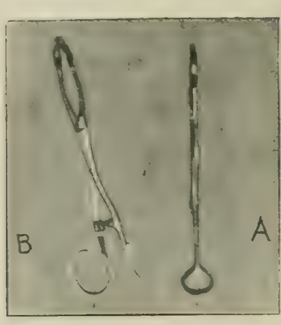
Mr. ROSE (in reply): My own experience does not include any case of round-celled sarcoma of the nose which has run a favourable course as did the two cases mentioned. I have only had to treat three: one was in a child aged 4, who died within a year of my seeing him. Another was a child aged about 12, and then there is this boy, aged 16. This boy is moribund, and the swelling in his face, which is so conspicuous, has appeared within the last ten days: in fact, when I first saw him, six weeks ago, there was no external swelling visible at all. Therefore, as my experience of round-celled sarcoma is so unfavourable, I cannot help thinking, when I read and hear of cases which run a favourable course, that two different diseases are called by the same name.

(*November* 2, 1917.)

A Tonsil Forceps.

Exhibited by W. H. KELSON, M.D.

I HAVE found that in difficult cases of tonsil enucleation, where there is a brittle tonsil to remove, tenaculum forceps, however well made, are unsatisfactory, as they are apt to tear. Therefore, for some time past I have used ordinary gland forceps. When one completes the operation with the snare there is no difficulty in getting the wire over the handle, but if the operation is completed with the tonsillotome there is difficulty. I have, therefore, partly cut away the handle of the forceps on one side, and cut through the loop of the handle on the other, which allows a medium-size tonsillotome to be easily slipped over the forceps whilst the tonsil is grasped (*see* next page).



A, shows blades of forceps well adapted for securely seizing the tonsil; **B**, shows handles cut away so as to allow of the blade of a medium sized tonsillotome being passed over.

(*November* 2, 1917.)

Paralysis of the Left Vocal Cord in a Man, aged 39.

By W. H. Kelson, M.D.

Patient, a clerk (passed C 1), came to hospital complaining of hoarseness, which he states came on suddenly in September. He gives a history of having had syphilis fourteen years ago. On examination the left vocal cord is seen to be almost motionless, the right coming across to assist when phonation is attempted. The left pupil is rather larger than the right. Examination with X-rays shows a distinct bulging in the region of the transverse and descending portions of the aortic arch.

Major Watson Williams: Was this patient treated for syphilis? A case which I have illustrated I diagnosed as lupoid tuberculosis of the pharynx, curetted it and applied lactic acid. Then by some accident I gave antisyphilitic treatment, and it cleared up. I ask whether that has been done here, because tertiary syphilis of the posterior wall of the pharynx sometimes strongly resembles lupoid infiltration.

(*November* 2, 1917.)

Lupoid Tuberculosis of the Pharynx, affecting the Soft Palate and Uvula, in a Boy aged 8, the Subject of Congenital Syphilis.

By Irwin Moore, M.B.

This case, first exhibited at the Sectional Meeting on March 2, 1917,[1] and described in the Agenda as " (?) Lymphadenoma," was again shown at the meeting on June 1, 1917,[2] as " Lupoid Tuberculosis." Considerable discussion followed, various opinions being expressed, and objections raised to the term " Lupoid."

When the case was last exhibited the following history had been obtained, and since then certain investigations have been carried out, as follows :—

(1) *Enlargement of Glands.*—Patient was first seen on January 16, 1917, suffering from enlargement of the submental, posterior cervical, and inguinal glands of eight months' duration. The submental glands, which were matted together, disappeared in six months under potass. iodide treatment. When last exhibited the enlarged discrete glands in the neck and inguinal regions were still present and had undergone no change.

(2) *Infiltration of the Pharynx.*—When patient was first seen the pharynx presented a semi-translucent swelling of the uvula, extending over the greater part of the soft palate ; it was solid, rigid, and leathery in consistence. This did not vary in appearance until March 12, two months later, when the infiltration was observed to be gradually extending downwards along the posterior faucial pillars. (Compare condition seen in the watercolour drawing of February 15 with that of March 20.) There had been no signs of ulceration or miliary deposit.

(3) The *tonsils*[3] were not enlarged. The microscopic sections of the

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[1] *Proc. Roy. Soc. Med.*, 1917, x (Sect. Laryng.), p. 84.

[2] *Ibid.*, 1917, xi, p. 2.

[3] Since this case was shown both tonsils have been enucleated. Sections have been cut and searched for tubercle bacilli, with negative result, but typical giant-celled systems were present.

adenoid tissue removed from the nasopharynx on February 14 "show a certain amount of fibrosis with chronic inflammatory reaction which is characterized by the formation here and there of cellular systems, which occasionally have a central multinucleated giant cell. The lesion is undoubtedly tuberculous" (Dr. Eastes' Laboratory).

For a later opinion *see* Professor Shattock's report, pp. 41, 42.

(4) *Examination of the larynx* on March 24 by Killian's suspension apparatus, with the co-operation of Mr. E. D. D. Davis, showed œdematous infiltration of the epiglottis and arytænoids, but no signs of ulceration. No difficulty in breathing has been observed.

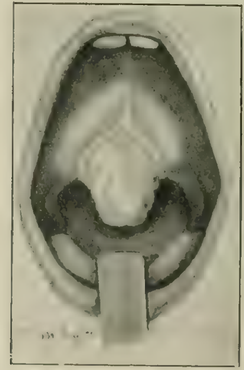

Fig. 1.

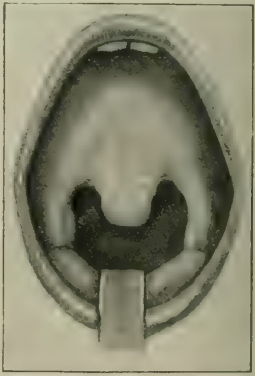

Fig. 2.

Fig. 1.—Lupoid tuberculosis of the pharynx affecting the soft palate and uvula, in a boy, aged 8, the subject of congenital syphilis. Case shown March 2, 1917. Drawing made February 15, 1917.

Fig. 2. Shows the infiltration extending downwards on to the faucial pillars and tonsils. Case shown June 1, 1917. Drawing made March 20, 1917.

(5) *Examination of the Lungs.*—No history of tuberculosis and no pulmonary signs were present until March 10. On this date (*a*) *Clinical examination* (Halls Dally) revealed scattered fine moist crepitations over the whole of both lungs. At this time a slight cough, night sweats, and slight rise of temperature were present. The child was losing flesh in spite of his appetite being good. (Note: Patient's usual

weight, 3 st. 8 lb., does not vary much.) (5) *Radioscopic examination* (H. D.) showed no enlargement of hilar glands, no peribronchial striation, and no enlargement of the tracheo-bronchial glands. No tuberculous foci in lung tissue. (For a later examination see further report below.)

(6) *Blood Report.*—"Showed on February 16 that it was not typical of any exact condition; it indicated a mild anæmia of secondary type with leucocytosis. Leukæmia may be excluded" (Dr. Fletcher, Dr. Eastes' Laboratory). *On March* 10: "A slight leucocytosis, the lymphocytes were within normal limits but a little on the high side. The Arneth count is low" (Halls Dally).

The following additional investigations have been carried out since this case was exhibited on June 1 with the kind assistance of Professor Shattock, Dr. Halls Dally, and Dr. A. N. Leathem:—

(1) Two independent authorities (Dr. Eastes' and Mount Vernon Hospital Bacteriological Laboratories) have found (June 8 and July 6, 1917) *strongly positive Wassermann reaction* and the parents have admitted infection with and treatment for syphilis.

(2) Dr. Halls Dally has shown (July 16, 1917), by radiograms recent *enlargement of hilar glands* with associated *peri-bronchitis tuberculosa simplex* and will demonstrate this at the Meeting (radiogram of chest will be shown).

(3) A definite *tuberculin reaction* was obtained on June 26, after diagnostic injection of 0·001 c.c. O.T., the temperature rising from normal to 101·2° F. (H. D.), whilst a few days later it was observed that the soft palate looked pinker and more normal. On palpation marked softening of the infiltrated palate was observed, probably the result of the tuberculin injection (temperature chart will be shown).

(4) *Blood Examination.*—The first Arneth count on March 10, 1917, showed: Cells of group I, 8 per cent.; cells of group II, 44 per cent.; cells of group III, 39 per cent.; cells of group IV, 9 per cent.; cells of group V, 0 per cent. Total nuclear bodies, 249, as against normal 275: this being distinctly on the low side and suggestive of tuberculosis. The second Arneth count on July 3, 1917, showed: Cells of group I, 9 per cent.; cells of group II, 50 per cent.; cells of group III, 0·34 per cent.; cells of group IV, 7 per cent.; cells of group V, 0 per cent. Total nuclear bodies, 239, being a still lower count, which is still more in favour of tuberculosis (Halls Dally).

(5) Histological report of the microscopic sections of uvula, &c., submitted to Professor Shattock:—

(*a*) *The Uvula* (removed June 29).—The basis or ground substance of the section consists of a felt-work of fine undulating fibrils, amongst which there lie flat connective tissue corpuscles furnished with large oval nuclei. In places the mesh of fibrils is abnormally open from œdema. Many of the capillaries show pronounced endothelial proliferation, some of them being quite blocked with the cells (fig. 3). The proliferated cells are large and flat, and notable numbers of lymphocytes occur amongst them, but no polymorphs. In the groups there occur a certain number of multinucleated cells: these have mostly but few nuclei (fig. 4); in one occluded vessel, however, a multinucleated cell holds nine, and these are peripherally disposed (fig. 5). Distributed

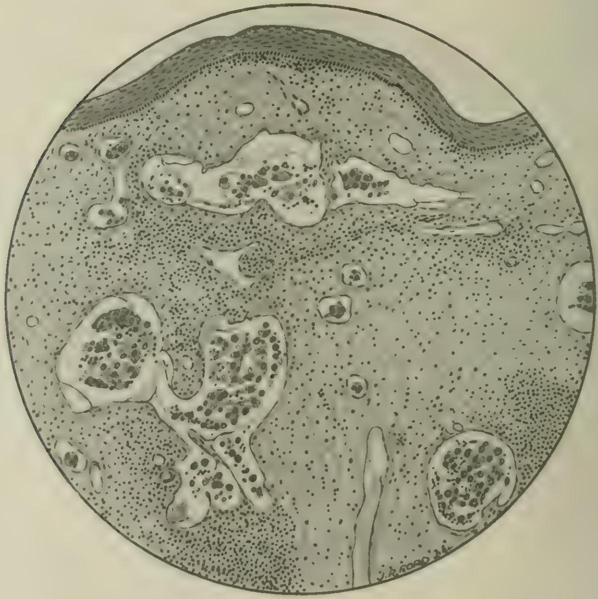

Fig. 3.

Section of uvula, showing lymph vessels occupied with varying numbers of proliferating endothelial cells, and lymphocytes, lying in a connective tissue infiltrated with lymphocytes and plasma cells ($\frac{2}{3}$).

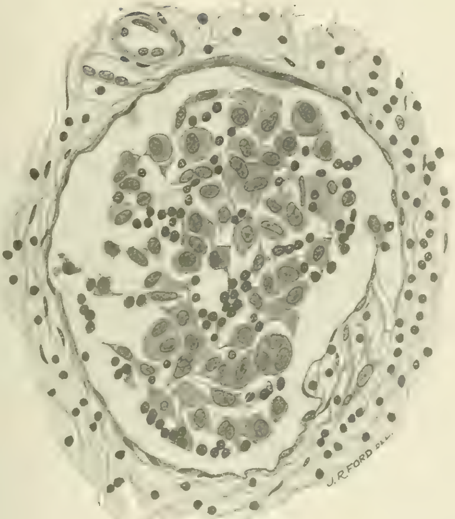

FIG. 4.

A transverse section of one of the lymph capillaries shown in the preceding section ($\frac{1}{6}$). Some of the proliferating endothelial cells contain two or three nuclei. Amongst the cells are a certain number of lymphocytes.

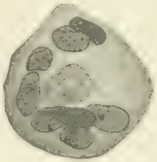

FIG. 5.

A multinucleated giant cell, selected from a group of proliferating endothelial cells in a lymph capillary. It contains nine nuclei disposed peripherally ($\frac{1}{12}$ oil immersion).

through the connective tissue are groups of closely packed plasma cells and lymphocytes; none of the groups in the two sections examined include any giant cells. The relationship of some of these groups with capillaries is clear, for they lie in their immediate vicinity. The capillaries in which endothelial proliferation is proceeding may be regarded as lymphatic, from their size (they are larger than the blood capillaries) and varicosity, and because those of chief size which contain a fine mesh of fibrin in addition to endothelial cells and lymphocytes, show no red cells nor polymorphs entangled in the mesh. The arterioles throughout the section are quite normal. The investing epithelium of the uvula is intact. The muscle fibres are unaffected. In regard to the glands, the epithelial tissue is normal; but there is an abnormal number of cells, lymphocytes, and plasma cells in the supporting connective tissue. Histologically, the lesion of the uvula is a capillary lymphangitis with an accompanying œdema and the presence of groups of plasma cells and lymphocytes in the connective tissue (S. G. Shattock).

No tubercle bacilli nor other organisms were found in a number of sections of the uvula stained (A. N. Leathem).

(*b*) *The Adenoids* (removed from the nasopharynx on February 14).—The section contains typical tubercular giant cells of full size, but by the side of such are others isolated in fibrosing tissue, which include only a few nuclei. No caseation is present. The endothelial cells around the giant cells are mostly elongated and fibroblastic (S. G. Shattock).

(*c*) *The Cervical Gland.*—This contains one or more caseous foci in which chromatolysis has occurred; these are surrounded with well advanced fibroblastic tissue; the giant cells lying in this fibrosing zone are few, but some are quite typical of a tubercular lesion (S. G. Shattock).

(*d*) *The Inguinal Gland.*—The section shows areas of endothelial proliferation, but no giant cells. There is no caseation (S. G. Shattock).

These lymphatic glands were kindly removed for me (during my absence) for microscopic examination, on July 20, by Mr. Norman Patterson.

(6) *Inoculation of Guinea-pig.*—Report: The surface of the piece of tissue was sterilized by careful searing. The tissue was then minced and implanted subcutaneously into a guinea-pig. The guinea-pig was inoculated on July 13, and killed on October 16, and showed no signs of tuberculosis, either at the seat of inoculation or elsewhere. The guinea-pig weighed 650 grm. at the time of inoculation, and 870 grm. when killed (A. N. Leathem).

Note.—With regard to the title of this case—i.e., "Lupoid Tuberculosis," the term "lupoid" was employed to differentiate these cases, intermediate in degree between the two types of tuberculosis and lupus—i.e., to describe the clinical appearance—viz., a deep infiltration of a lupus character, showing no evidence of miliary tuberculosis or ulceration. The retention of this title has since been approved by Professor Shattock.

I suggest that it would be more accurate to describe the case as "Primary tuberculosis of the nasopharynx and pharynx, in the latter position being of a lupoid type, and affecting specially the uvula and soft palate."

DISCUSSION.

The PRESIDENT: Dr. Irwin Moore has taken much trouble with this case, and has been well warranted in doing so, because we are not clear as to the significance of the appearances. On the one hand, he adduces evidences of the presence of tuberculosis, and, on the other, the Wassermann test came out strongly positive. Are we to regard the appearances in the pharynx and larynx as tubercular or as syphilitic? I am unacquainted with tubercular manifestations such as here seen, but I have had two or three cases similar in aspect in which congenital syphilis was undoubtedly present. I would therefore regard the case as one of congenital syphilis of the throat in a tubercular subject. I think Mr. Hunter Tod and Dr. Lambert Lack have also shown cases with similar appearances in syphilitic patients.

Sir STCLAIR THOMSON: I suggest that Dr. Irwin Moore treats this patient more thoroughly for syphilis and reports to the Section.

(Professor SHATTOCK, F.R.S., demonstrated the slides of the case.)

Professor SHATTOCK, F.R.S.: We must allow that pathologically this is an obscure case. It is obviously a mixed one of tuberculosis and syphilis; but the problem to settle is whether the local condition is due to a tubercular or to a syphilitic infection. I am inclined to view the chronic œdema of the uvula and palate as having a tubercular genesis for the following reasons: (1) It was originally continuous with the site of the adenoid disease of the nasopharynx, which is, histologically, undoubtedly tubercular: a post-operative infection can be excluded, therefore, as a cause of it. (2) The secondary disease of the cervical gland is tubercular. (3) The condition improved after subcutaneous injections of tuberculin. The chief histological feature in the uvula is the remarkable proliferation of the endothelium of the lymphatic capillaries, some of which are quite blocked; amongst the large endothelial cells there are no red blood corpuscles nor polymorphs, but a moderate number of multinucleated cells, some containing two or three nuclei; and one as many as nine, peripherally arranged. The microscopic findings suggest that the disease of the uvula is due to the transference of tubercle bacilli by the lymph channels, and their subsequent location in the lymphatic endothelium, with a resulting endothelial proliferation, and a scanty giant-cell formation—a tubercular capillary lymphangitis. The improvement under tuberculin injections points to a tubercular genesis of the lesion, in spite of the fact that no tubercle bacilli (nor other organisms) were found in sections of the piece of the uvula removed at the first operation; and that no tuberculosis was set up in the guinea-pig inoculated with the second, basal, portion of the uvula excised. Short of an actual transference of bacilli, the changes might be attributed to the access of toxin produced in the nasopharyngeal tubercular lesion. This would put them ætiologically on a line with lupus erythematosus, whether of skin or buccal mucosa, many cases of which presumably result from the action of tubercle toxin apart from a growth *in loco* of the tubercle bacillus, since a large proportion of the subjects of lupus erythematosus are tubercular, although the lesion as a rule contains no tubercle bacilli; yet not only may giant cells occur in lupus erythematosus, but tubercle bacilli have been found in some cases. Clinically the amelioration of lupus erythematosus is not to be observed under the use of tuberculin. This suggests that the condition in Dr. Irwin Moore's case is more than toxic, and that there may be bacilli, few and far between, in the tissues. The alternative view would be to regard the œdema as due to syphilitic infection, and to look upon the presence of the adenoids and their tubercular infection as things independent of the other condition. There is one point in conclusion which I venture to suggest in connexion with the cases of chronic œdema occurring in the adult, and that is whether some may not have a gouty pathogenesis. I do not mean that the tissue would show microscopically the cell proliferation, necrosis, and deposition of urate occurring in a proper gouty lesion, but whether the condition may be a manifestation of what is clinically sometimes called "goutiness."

Dr. IRWIN MOORE (in reply): The patient has not had any treatment for syphilis. Since he was exhibited here he has lived in Devonshire, and had no treatment of any kind. My conclusion is that tuberculosis is the main factor in the case, superadded to congenital syphilis. It is my intention to give the patient antispecific treatment, as suggested by Sir StClair Thomson.

(*November* 2, 1917.)

Tertiary Syphilis of the Pharynx in a Female, aged 44, clinically resembling Tuberculosis of a Lupoid Type.

By Irwin Moore, M.B.

This case was shown at the previous meeting of the Section on June 1, 1917,[1] and described in the Agenda as "lupoid" tuberculosis, on account of the clinical appearance. A positive Wassermann reaction was obtained the morning of the meeting. Since showing the case the following investigations have been carried out:—

On June 5 the uvula, also pieces from the pharynx, were removed, and several sections examined by Dr. Fletcher. He reports that "they show thick infiltration of the tissues by small round inflammatory cells. This is especially marked near the surface, while the deeper areas are densely fibrotic. The surface epithelium is puckered but there is no ulceration. No giant cells nor epithelioid cells were found, nor was there evidence of vascular thickening. The features are those of a chronic inflammatory condition. The Wassermann reaction being positive, the condition is presumably syphilitic" (Dr. Eastes' Laboratory). On June 12 an intravenous injection of galyl (0·25 grm.) was given with remarkable results, as may be seen by comparing the drawing of the pharynx made on May 22 (fig. 1) (which represents also the condition when shown at the Section meeting on June 1) with that on July 10 (fig. 2).

It will be observed that in the space of one month almost the entire lupoid-looking infiltration had disappeared, and its place been taken by cicatricial tissue. The nodular-looking uvula seen in the first drawing (fig. 1) is not seen in the second drawing (fig. 2) since it was removed for microscopic sections. The tongue, which was to a great extent fixed (fig. 1), could be protruded further (fig. 2), and the larynx, which was not previously seen, could be easily examined with the laryngeal mirror. Patient's condition has so much improved since the injection, and the airway has become so free, that respiration can now be carried on with the tracheotomy tube corked. It is, however, being retained a little longer for safety.

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[1] *Proc. Roy. Soc. Med.*, 1917, xi (Sect. Laryng.), p. 4.

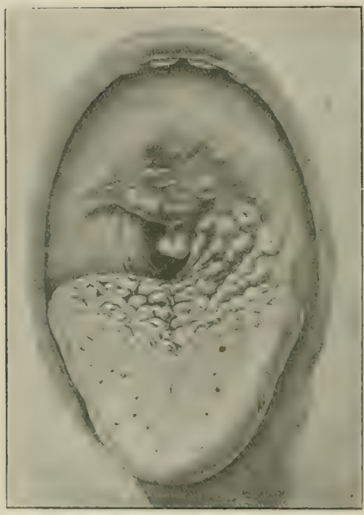

FIG. 1.

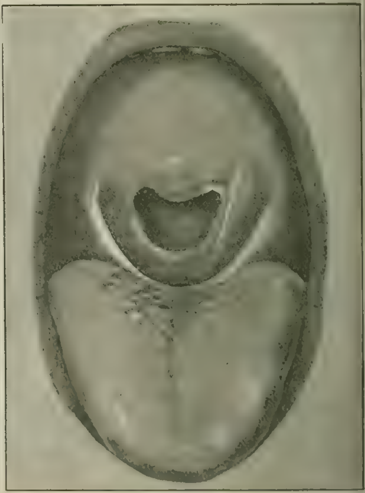

FIG. 2.

Fig. 1.—Tertiary syphilis of the pharynx, in a female, aged 44, clinically resembling tuberculosis of a lupoid type. Shows extensive nodular infiltration of the tonsils and fauces, which has extended on to the soft palate, uvula, and base of tongue. The posterior faucial pillars are scarred, and have become approximated, and united to the posterior pharyngeal wall. The oropharynx is markedly stenosed. Case shown June 1, 1917. Drawing made May 22, 1917.

Fig. 2.—Shows the result, one month later, of an intravenous injection of galyl (0·25 cg.). Case shown November 2, 1917. Drawing made July 10, 1917.

(*November* 2, 1917.)

Complete Paresis of the Left Vocal Cord due to a Mediastinal Growth.

By Irwin Moore, M.B.

Patient, a male, aged 45, attended the Great Portland Street branch of Golden Square Hospital, on September 18, complaining that three weeks previously a piece of meat had stuck in his throat and was only released by being pushed down by a bougie. Since this he has had a sore throat, hoarseness of the voice, and has only been able to swallow semi-solid food. His former weight was 11 st., but since this occurrence he has lost 2 st. When first seen by the exhibitor on October 5 considerable œdema of the arytænoids and upper part of the larynx was observed and the vocal cords could not be seen.

There has been progressive emaciation during the past two months. Patient has a cough, with slight sputum and dyspnœa. His previous health has been good, and there is no history of syphilis.

On October 12 the œdema having cleared up, the left vocal cord was seen lying in the "cadaveric" position.

On October 18 Dr. Halls Dally kindly saw patient for me, and reports as follows:—

Examination of the chest revealed impaired percussion note over the upper two-thirds of the left lung, front and back, the note being dull from the lower border of the clavicle to the third rib. Over this area the breath sounds are feeble, and crepitations were audible above and below the left clavicle, and in the left interscapular space at the level of the sixth and seventh thoracic vertebræ.

Blood Count.—Red cells, 5,000,000 per cubic millimetre; white cells, 12,000 per cubic millimetre; colour index, 0·7 per cent.; hæmoglobin, 70 per cent.

The radiogram of the chest shows a shadow with ill-defined outline extending from the mediastinum over the left upper lobe and upper portion of lower lobe. The appearances of this shadow suggest a mediastinal new growth invading the left lung (H. D.).

DISCUSSION.

Dr. SYME: I should like to mention two cases of recurrent paralysis. A man was operated upon under ether for hernia, and a week later he lost his voice. I saw him three weeks after his operation. He had complete paralysis of his left cord. The œsophagoscope and screen showed no cause. In October his voice had returned. He has now abductor paralysis. I thought there might have been dragging on the chin at the time of the operation, and hæmorrhage into the sheath of the nerve, but there was no trouble with the anæsthetic. The other case was that of a girl with a scar on the left side of the neck, the result of a removal of glands three years previously. Two months ago she lost her voice, and had complete paralysis of the left cord. This is passing off, and there is now paresis of the abductor.

Dr. W. HILL: I think it is mediastinal growth, but it is curious there should be recurrent paralysis, unless the aorta has been pulled down, which does not appear to be the case. It might be due to a gland higher up.

(*November* 2, 1917.)

Illustrations of Nasal Endo-rhinoscopy (Epidiascope).

Shown by P. WATSON-WILLIAMS, M.D., Major R.A.M.C.

THIS series of twenty-nine drawings, illustrating endo-rhinoscopy, emphasizes the value of the exact inspection of the nasopharynx and the posterior portion of the nasal passages, more particularly for the detection of pathological conditions which, by all other means, are invisible.

Some of the cases illustrated show small streams of muco-purulent discharge from one or both sphenoidal sinus ostia, or from the middle meatus posteriorly, and in some of the examples probes are shown entering the sphenoidal and frontal sinuses. Examples of minute polypi in the spheno-ethmoidal region of early malignant growths in the nasopharynx are included. Other illustrations show changes in the opening of the Eustachian tube in swallowing as compared with the appearance during quiet respiration. The great majority of the illustrations have been taken with the Holmes' modification of Valentin's nasopharyngoscope; there is only one example of the picture obtained by Wolff's instrument to show the disadvantage the inversion of the image yields: the more restricted area coming into view.

DISCUSSION.

Dr. KELSON: When working with this instrument, I was so disappointed in my results owing to pus and mucus getting on to the reflecting surface of the instrument, so that one could not see, that I gave it up. How does Dr. Watson-Williams prevent the mirror being soiled by the discharge, as it is in cases when such is present that it should be most useful?

Lieutenant-Colonel PERRY GOLDSMITH: I know this instrument, as I saw it when first exhibited. It is Valentin's instrument, with the curve slightly altered, to which somebody else's name has been attached. While that is common in the United States, it is not unknown in this country. When I saw the instrument exhibited by Dr. Holmes, in Boston, I could not see very much with it, nothing as much as he saw, nor could I do so for some time, until I had the instrument and used it. I find it of value in a limited number of cases. It shows you are in the Eustachian tube when passing a bougie: not infrequently we think we have passed it there, and it is not so. Secondly, there are cases with chronic catarrhal otitis which do not clear up, and the nasopharynx is invisible. With this instrument you can examine the fossa of Rosenmüller, and occasionally the opening of the sphenoidal sinus, and a polypus in the posterior ethmoidal region. I regard the instrument as of great value, and the more one uses it the better one likes it. It is not a toy, and one can see better than with a post-pharyngeal mirror.

Dr. DUNDAS GRANT: I have not used this endo-pharyngoscope as often as I might have done, because I am constantly doing posterior rhinoscopy, and getting a good view. To-day, however, I had a case of slight adhesion, owing to which I could not get a good view with the posterior rhinoscope, and this instrument got me out of the difficulty, which was to prove absence of disease at the mouth of the Eustachian tube. No doubt one has to study and correct one's own eyes, but I do not find the instrument easy.

Major WATSON-WILLIAMS (in reply): After cocainization one is much less likely to have the mirror clouded. If that should happen, I draw the instrument out, clean it, and replace it. But once or twice I have had to mop out accumulations of pus. My experience at the beginning with the instrument was much like Dr. Dundas Grant's. I am now using it almost constantly, and in many cases would feel lost without it, so I keep three instruments at hand, lest I should get stranded.

(*November* 2, 1917.)

A Piece of Mutton Bone removed from the Entrance to the Œsophagus.

By Sir William Milligan, M.D.

A large piece of bone removed from the entrance to the œsophagus. Patient, a soldier, swallowed the bone while taking a hurried meal in the trenches. Complained of severe pain and experienced the greatest difficulty in even swallowing fluid. Was sent home at once. First attempt to dislodge the bone (under general anæsthesia) failed. Second attempt, forty-eight hours later, succeeded. Irwin Moore's forceps were used upon both occasions.

(*November* 2, 1917.)

Fibro-papilloma of Trachea; Removal by External Operation; Recovery.

By Sir William Milligan, M.D.

M. W., aged 29, female. Gradual and progressive dyspnœa for a few months before becoming a hospital out-patient. While attending medical out-patient clinic, June, 1917, developed pneumonia. Admitted as an in-patient and, after recovery, was transferred to the Throat and Ear Department, August, 1917. Upon indirect examination an elongated growth was seen upon the posterior wall of the trachea and apparently occupying about five-sixths of its lumen. The slightest exertion caused great dyspnœa.

Direct examination showed that the growth was springing from the posterior wall of the trachea, and was about $\frac{3}{4}$ in. in length, firm and resistant to palpation with a probe and presenting a slightly lobulated surface.

Tracheotomy was performed under local anæsthesia followed at once by a crico-thyrotomy. The growth was removed piecemeal by forceps and scissors and its broad attachment to the posterior wall of the trachea rubbed freely with chromic acid (40 per cent.). The external wound was immediately closed, but the tracheotomy tube was retained for a week. Complete recovery. Patient able to resume her work as a table maid.

(Slides demonstrated by epidiascope.)

DISCUSSION.

Dr. WILLIAM HILL: I wrote an article on this subject for an encyclopædia, and I described it as Tilley's method of combined bronchoscopy and screen examination. I employed a method which was published by one of my assistants at St. Mary's Hospital, with skiagrams. We saw the pin grasped, and it gave way. We again seized the pin, drew it down, then altered the position of the forceps, grasping the point, and we got it up with ease. I did this, having seen Mr. Tilley do it at University College Hospital eight years ago.

Dr. DUNDAS GRANT: I have on a former occasion mentioned the case of a child on whom I did tracheotomy on account of papillomata of the larynx. I found, however, that the child did not breathe better. I removed the tube, and discovered a portion of growth presenting at the tracheotomy orifice. I removed a large papilloma growing from the wall of the trachea, and the breathing was restored. I do not remember another such case, and agree that it must be a rare condition. In operating on the bronchial tubes under the fluorescent screen, the difficulty would seem to consist in looking into the screen and manipulating the forceps at the same time. All that I have done in that way with the screen has been to pass an œsophageal bougie in a case of dilatation of the œsophagus from "cardiospasm": by that means I was able to guide it through the abdominal œsophagus and the cardiac orifice, steering clear of the dilatation in which the bougie is so apt to lose itself.

Dr. KELSON: Was the diagnosis verified by microscopical examination, as so many of these growths are found to consist of thyroid tissue?

Sir WILLIAM MILLIGAN (in reply): I have used this method for years, but as this was a particularly good case I thought it might be interesting to record it. With regard to the case of tracheal growth, I am sorry there has not been more discussion, as it opens up a very important matter. Many of us are doing Army work, and must have seen a good number of men wearing tubes which they cannot do without. The question arises as to how to deal with these cases, and how to deal with this girl also becomes an important question. I have always found it very difficult to re-establish the patency of a trachea which has been occluded in the manner described. I have not much sympathy with a laryngostomy, because it is very difficult to get success with after-treatment. In answer to Dr. Kelson, the report on the character of the growth was made by Professor Dean, our pathologist.

(*November* 2, 1917.)

Fracture of Four Rings of the Trachea; Urgent Dyspnœa; Operation; Recovery.

By Sir WILLIAM MILLIGAN, M.D.

A. S., FEMALE, aged 20, a munition worker, while at her work got a scarf which she was wearing round her neck caught in rapidly revolving machinery with the result that she was all but strangled. Owing to the scarf giving way the patient's life was probably saved. Urgent dyspnœa, pain in the neck and expectoration of blood immediately followed. She was admitted to the Manchester Royal Infirmary and a tracheotomy was at once performed under local anæsthesia. On opening the windpipe four tracheal rings were found to be fractured. At the end of three weeks the tracheotomy tube was removed and the patient transferred to the throat department. Owing to increasing dyspnœa the tube was re-inserted. Direct examination showed a mass of granulation tissue occupying the greater part of the lumen of the trachea, opposite the second, third and fourth rings, and nearly occluding it.

By passing bronchoscopic tubes of increasing size the stricture in the trachea was dilated and at the present moment the girl is wearing a large O'Dwyer's intubation tube.

Suggestions as to further treatment will be welcomed.

(*November* 2, 1917.)

Case of Complete Laryngectomy.

By WALTER HOWARTH, F.R.C.S.

THE larynx was removed for carcinoma on June 3. As there does not appear to be any development of a pharyngeal whisper, the question of speaking apparatus arises. Opinions are invited as to the advisability of this, and the form of apparatus most likely to be useful.

(*November* 2, 1917.)

Congenital Deformity showing a Nose with Four Nostrils.

By Sir StClair Thomson, M.D.

The patient is a female infant, aged 4 weeks. The extremity of the nose shows three good-sized vestibules with two columellæ, and, on closer inspection, a smaller fourth orifice is visible. Further examination with a probe shows that it is the most external orifice on each side which is the true air passage, the two supplementary orifices being internal to them. These latter orifices are blind cul-de-sacs, the smaller one being only about ¼ in. long, while the larger one passes inwards for ½ in. There is a deviation of the end of the septum to the right; this is interesting in view of the general agreement that nearly all deviations are acquired and that most of them are traumatic.

Opinions are invited as to (*a*) the embryological origin, (*b*) the operation advised, and (*c*) the age at which treatment should be undertaken.

(*November* 2, 1917.)

Nasopharyngeal Growth for Diagnosis.

By Buckland Jones, M.B.

Patient, a male, aged 28, complains of nasal obstruction and occasionally of feelings of suffocation. There is no history of pain or hæmorrhage, or of any previous operation. Opinions are requested as to (1) the nature of the growth, and (2) as to the advisability of operative interference.

(*November* 2, 1917.)

Case of Fibrosarcoma ?

By A. L. Macleod, M.B.

Patient, a male, aged 17, by occupation a labourer. Duration of the disease, eight months. In January last he began to bleed from left nostril; this continued every few days, and he found difficulty in breathing. In March there was yellowish discharge from left nostril. No feeling of growth in the throat was present. He came to Leicester Infirmary in October. Two attempts to remove the growth with the snare failed, even a very thick wire breaking.

Pathologist's Report.—The section shows huge masses of bacteria (probably staphylococci) throughout the granulation tissue. There is no evidence of neoplasm in the section.

Section of Laryngology.

President—Dr. A. Brown Kelly.

(*December* 7, 1917.)

Photographs, Specimen and Slide from Case of Teratoid Growth or Hairy Polypus of the Nasopharynx.[1]

By A. Brown Kelly, M.D. (President).

Female infant, aged 6 weeks, was brought to the Victoria Infirmary on account of the frequent protrusion from the mouth of a long, tongue-like growth, which had to be pushed in again by the mother. The growth was first noticed when the baby was a week old, and had since been getting bigger and more troublesome.

On examination, the elongated growth was found to hang behind the tongue by a fine pedicle coming from the left side of the nasopharynx; the exact site of origin could not be determined. On being drawn out of the mouth it projected ¾ in. beyond the lips. The pedicle was severed as high as possible with a fine snare.

The growth on removal had an elongated clubbed shape. It measured slightly over 2 in. in length and ½ in. across at its thickest lower part. It was of fleshy consistence, and was covered with skin furnished with numerous fine hairs.

Microscopical Report.—External layer of stratified squamous epithelium is four to five cells deep, with indippings of a few hair follicles. The underlying connective tissue is of a loose rudimentary character, with two or three minute glands. The central and larger portion of the polypus is composed of tissue closely resembling fat (Dr. John Anderson).

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[1] Fully reported in *Journ. Laryngol.*, 1918, xxxiii, p. 65 (with photograph)

(*December* 7, 1917.)

Collection of Dental Cysts and Cysts of Floor of the Nose.

Shown by A. BROWN KELLY, M.D. (President).

THE points to which it is desired to direct attention are:—

(1) *The Confusion of Dental Cysts with Supposed Diseases of the Antrum.*—Within the last few years it has come to be recognized that the conditions described as "hydrops antri Highmori," "distension of the maxillary sinus by fluids or mucous cysts," "empyema of the antrum with blocking of the ostium and bulging of the walls," do not exist as such, but are really produced by dental cysts invading the antrum. As a result of our more exact knowledge dental cysts are now diagnosed much oftener than formerly. I have met with over thirty cases during the past eleven years; previously, with few exceptions, such cases were regarded by me as varieties of antral disease, or, as due to periostitis of the superior maxilla.

(2) *The Differentiation of Cysts of the Floor of the Nose from Dental Cysts.*—It has been shown[1] that retention cysts occasionally develop in the anterior part of the floor of the nose, presumably from long ducts present in this situation. Various stages in the development of these cysts have been observed, from a small delicate rounded bulging of the mucous membrane just beyond the vestibule, to a sac as large as a cherry, with dense walls, causing elevation of the ala and extensive erosion of the bony edge bounding the nasal cavity below. As these cysts have been proved to have no genetic connexion with the teeth it has been claimed that they should be placed in a special class. These conclusions are based on three cases I previously reported and seven I subsequently observed. Dental cysts may extend upwards, raise the ala, and also produce a bulging of the nasal floor. Some writers, apparently unaware of the existence of cysts of the floor of the nose, have described this cushion as a useful diagnostic sign of a dental cyst. The reports of several of their cases indicate, however, that they were dealing with cysts which had really originated in the nasal floor.

(3) *Treatment.*—Most of the dental cysts were removed by dissection: the smaller were scraped out, and in a few cases only a

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[1] *Journ. Laryngol.*, 1898, xiii, p. 272.

portion of the wall was excised, the remainder afterwards shrivelling and the cavity becoming obliterated. Cysts of the floor of the nose when small were punctured in the nose and cauterized. The larger were dissected out from the mouth.

(*December* 7, 1917.)

Retention Cyst of the Nasal Floor.

By IRWIN MOORE, M.B.

PATIENT, a male, aged 53, first seen on September 12, 1916, complained of a swelling on the right side of the nose and adjoining portion of the cheek, which had existed for six months.

On examination: A round, tense, fluctuating swelling, the size of a small walnut, was seen occupying the floor of the right nasal vestibule, causing considerable obstruction, and gaping of the nasal orifice. There was marked bulging outwards of the right ala nasi with partial obliteration of the naso-labial sulcus, and protrusion forwards of the upper lip, whilst the swelling extended downwards to the right of the middle line under the lip, causing obliteration of the gingivo-labial furrow. Considerable facial disfigurement resulted, as may be seen from the photograph exhibited. Many septic stumps were present. The case was to have been shown at the Meeting on November 3, 1916, and it was my intention—after the mouth had been made more healthy—to remove the cyst sublabially. In the meantime the patient attended the Dental Department of Charing Cross Hospital to have some teeth extracted, where the cyst was incised sublabially by the dental surgeon, and emptied of its contents, which consisted of a thick, pale yellow exudation. As a result of drainage and packing with gauze wicks the wound gradually healed up, and the cyst has not again refilled.

Short preliminary notes of the case were published in the *Proceedings*, 1917, x (Laryng. Sect.), p. 31.

DISCUSSION.

Mr. HERBERT TILLEY: I have found that most of these cysts are connected with a carious incisor tooth, either central or lateral, and that their contents include cholesterin crystals. I would ask the President whether he has found such crystals in the retention cysts. With regard to operation, I have followed a procedure similar to the President's. The large cysts sometimes extend back, like the finger of a glove, and raise the floor of the nasal fossa and I prefer to approach them from underneath the lip, to scrape out the mucous membrane lining of the cyst, and then pressing down the whole cyst wall from within the nose one gets a very quick and excellent result. An advantage of that method is the avoidance of prolonged suppuration within the nasal cavity.

Dr. D. R. PATERSON: There are difficulties with some cysts even after dissecting out the sac. We have had to remove a considerable quantity of bone in such cases, and it has been suggested to me that the use of decalcified bone tissue, grafted, would help. Have members had experience of it in these cases? In two cases, when dealing with a very large cyst which encroached on the antrum, so that there was very little antrum left, instead of removing the whole cyst wall I threw the cavity into the nose, just as is done in the radical antrum operation, and both the cases did well. These cases sometimes require considerable attention, and do not always heal up as smoothly as has been said.

The PRESIDENT (in reply): I am not sure that I have found cholesterin crystals in retention cysts, but those I have termed retention cysts have no connexion with the teeth. In reference to Dr. Paterson's remark, the cyst does not require to be dissected out, but if well opened it will shrivel up, and need very little further attention.

(*December* 7, 1917.)

Pharyngeal Pouch treated by Diverticulo-pexy.

By WILLIAM HILL, M.D.

MAN, aged 43, complained of suffering from dysphagia, regurgitation of food, and gurgling for six months. The diagnosis was confirmed by endoscopic and X-ray examinations. The pouch, which was bound to the gullet by fascia, was dissected free, and the fundus of the unopened pouch stitched to the left side of the inferior constrictor muscle. As the result all symptoms have for the time disappeared.

(Epidiascope Demonstration.)

DISCUSSION.

Mr. E. D. D. DAVIS: Was the diverticulum or sac firmly attached by fibrous tissue to the pharynx and was dissection difficult? Mosher has published a paper in which he records three successful cases in which he divided the party wall between the sac and the pharynx as far as the fundus of the sac by the use of the endoscope. After the first case he feared mediastinitis, but it did not occur. If the diverticulum is not adherent to the pharynx Mosher's treatment may result in mediastinitis. Perhaps the relief of the symptoms is due to having divided the lower horizontal band of fibres of the inferior constrictor muscle. These diverticula are really herniæ between the oblique and horizontal sets of fibres of the inferior constrictor and consist of mucous membrane only, and this has been shown by the sections of the diverticula cut by Professor Keith and myself. The pathology of these diverticula is similar to that of inguinal hernia, viz., there is increased pressure due in this instance to the act of swallowing and secondly to a weakness in the wall of the pharynx where the pouch protrudes. I think that the increased pressure can be augmented by the sphincter action of the lower fibres of the inferior constrictor, though, if one examines these cases during operation, no stenosis is found below the sac. Mr. Waggett and I recorded a case five years ago in which the sac was free and there was no difficulty in separating it from the wall of the pharynx and turning it upwards. Mr. Waggett inverted the sac into the pharynx and stitched up the slit-like opening left on the outer aspect. Two cases treated by this method were reported by Girard of Geneva, and I wrote asking him what the final result of these cases was, but I received no reply. In Mr. Waggett's case, the patient sneezed violently eight months after operation and out came the pouch with recurrence of the symptoms, and at the second operation the pouch was found in the same position as before. With regard to excision of the diverticulum, I know of two cases of recurrence in which the sac was excised and the aperture in the pharynx carefully stitched with two layers of sutures: one recurred five years after operation by Mr. Charters Symonds, and in Mr. Waggett's case the symptoms commenced to recur two years after operation. I should like to know if members have heard of recurrence after excision of the sac.

Sir StClair Thomson: I saw in consultation a case upon which Mr. Charters Symonds had operated, and there was a slight recurrence.

Dr. Dundas Grant (in reply to a reminder from a member as to cases of pharyngeal pouch occurring in hospital): One case died, the other recovered, and we have not heard of any recurrence. But there was no close adhesion between the sac and the œsophagus, and in neither case could Mosher's operation have been practised with any likelihood of success. In the first case the sac on the right side was stretched a long way down, and when released from its attachments it shrank up to the size of a finger-stall. To prevent recurrence, I think it important to continue dilating the upper orifice of the œsophagus. I am sure the contraction there has to do with the formation of the sac. I described a case in which all symptoms of pharyngeal pouch were present when there was no pouch, though there was pouching. The symptoms disappeared after dilatation of the upper orifice of the œsophagus. It might have been well to obliterate the neck of the pouch, because if left open for drainage, the chance of food getting into it is great. Goldmann's operation was accompanied by a ligature and twisting up of the pharyngeal pouch before bringing it to the surface. Its tail was stitched into the wound ready to be pulled out when loose at the site of the ligature.

Dr. KELSON: Will the result be permanent? In two cases of complete removal of pouch I have had, there was no recurrence in one for two years, and in the other for three years, after which the patients disappeared. Both were between 60 and 70 years of age. The extraordinary way in which these patients improve in health and put on flesh afterwards fully justifies the operation.

Mr. E. D. D. DAVIS: Halstead informs me that he had one case in which he ligatured the sac near the wall of the pharynx and allowed it to lie in the neck wound, which was packed with gauze, but the patient died of pneumonia a few days later.

The PRESIDENT: Dr. Hill has introduced an important modification in the treatment of pharyngeal pouches. I have had a few cases of the kind, but most of them have been in very old persons in whom surgical interference was inadmissible, while for the others I have hesitated to urge operation owing to the mortality. Has Dr. Hill found the pouch arising, as usual, above the fundiform fibres of the inferior constrictor, or, in the weak triangular space bounded above by these fibres and laterally by the converging bundles of longitudinal fibres of the œsophagus?

Dr. HILL (in reply): In the two cases which have been operated upon, the pouch was between the horizontal and the oblique fibres of the inferior constrictor: that is to say, the neck of the sac was surrounded by the inferior constrictor, and I assumed it was the position, because both Killian and Keith worked it out at that site. Zenker was the first to demonstrate that they perforated the constrictor muscle, though he pointed out that Rokitansky had found post mortem, in one case, that it did come through the substance of the inferior constrictor. Zenker visited the museums of Europe and got permission to dissect and examine specimens. He found they were all pharyngeal, not œsophageal. Mr. Davis has shown the untenability of Hackermann's linear triangle myth; there is no weakening of the upper part of the gullet itself. I have looked for it in all the cases I have seen, but there was a hiatus between the two constrictor bundles of muscles in both operations. Dr. Grant asked why I did not obliterate the neck. It was for the reason that Halstead did so, and, as Mr. Davis said, he had a death. It is quite likely that that patient died from the sloughing at the ligatured site, and the supervention of coli infection, mediastinitis and pneumonia. As to whether the pouch was bound down by fascia, it was so in both the cases, and careful dissection was necessary, and I was glad to have a bougie in the pouch, so that I could dissect the fascia between the gullet and the pouch. I have read of many cases, and in some they separated easily: I mean the large ones, in which one tears the fascia readily when it is put on the stretch. But in these two cases there had been much binding down, and I felt nervous about the recurrent laryngeal nerve. I should never think of doing Mosher's operation, running the risk attendant on operating in the dark when an open method was available to me. At the smallest indication of the pouch filling I shall dissect it out, and ligaturing the neck, in view of what has been said about recurrence, I shall stitch the bundles of muscular fibres together, to see if that makes any difference. The tendency, in both operations, is towards recurrence.

(*December* 7, 1917.)

Case of Tuberculosis of the Larynx in a Female Patient; Dyspnœa and Regurgitation of Liquids during Drinking relieved by Intralaryngeal Operation.

By J. Dundas Grant, M.D.

Mrs. B., aged 31, had suffered from gradually increasing hoarseness for three years, and complete loss of voice for six months. She went to the Sanatorium at Bournemouth in August of this year. While there she began to have difficulty and pain on swallowing, which first showed itself by a feeling of choking during drinking. Shortness of breath and difficulty in breathing developed later and about fourteen days after its development (on October 30) Dr. Davison, of Bournemouth, who sent her up to Brompton Hospital, reported that there were signs of pulmonary mischief. I found shiny infiltration of both aryepiglottic folds and ventricular bands, with nodular infiltration of the upper surface of the right vocal cord. There was a tuberculous deposit in the interarytænoid space interfering with a closure of the vocal cords, producing inspiratory stridor and also regurgitation of liquids into the larynx during swallowing.

I removed a thick transverse piece of tissue from the interarytænoid space. The breathing was then easier. She was ordered anæsthesin powder for inhalation by means of a LeDuc's tube, and to drink fluids through an indiarubber tube.

On November 13, galvano-caustic puncture was made in the right ventricular band and vocal cord and in the left ventricular band, and on November 23 again in both ventricular bands.

On November 24 the ventricular bands had shrunk so as to permit of a view of the anterior commissure where there was a rounded nodule projecting between the cords. This was removed by Landgraf's forceps. It was seen that the cords were approximated at their posterior extremities and did not separate sufficiently during inspiration. My intralaryngeal forceps, opening from side to side, were introduced and opened so as to dilate the larynx.

There is now no pain on swallowing and no regurgitation of liquids during drinking, the patient not finding it necessary to use anæsthesin before taking food, and although there is a slight degree of stridor on inspiration there is no difficulty in breathing and she can run upstairs, which she was frequently quite unable to do. The voice is a little more than a whisper. She is taking syrup of garlic, but as she had little cough or constitutional disturbance, it is scarcely a case for demonstrating the efficacy of the remedy. Subjectively, however, she is feeling very much better.

Regurgitation of liquids in tuberculosis of the larynx is generally due to loss of substance from ulceration in the interior of the cavity, combined with rigidity of the framework produced by infiltration, and recovery of the power of drinking normally is exceedingly rare. The action of the transverse bar of tuberculous tissue in propping apart the side-walls of the larynx is a most unusual cause of regurgitation.

Dr. W. Hill: The suggested cause is important. Entrance of food into the larynx causes coughing, and is followed by regurgitation, and that is probably because the arytænoids cannot come together. The vestibule of the larynx prevents things entering into the larynx.

(*December* 7, 1917.)

Case of Fibro-papilloma of Larynx simulating Laryngeal Tuberculosis in a Middle-aged Soldier; Pendulous Epiglottis; Removal of Growth by means of Snare and Forceps.

By J. Dundas Grant, M.D.

The patient, a soldier, states he was discharged on account of tuberculous laryngitis; he had suffered from sore throat, shortness of breath and huskiness for six months, and from pain in front and back of the chest for nine months. He is under Dr. Batty Shaw, at Brompton Hospital, for pulmonary tuberculosis, the upper and middle lobe of the right lung being involved. Tubercle bacilli had been found in the sputum in France but none here.

I saw him on October 23, when he was complaining of extreme hoarseness. The epiglottis was so pendulous as to conceal the interior of the larynx, but on elevating it by means of Mount Bleyer's epiglottis-lifter I found the larynx occupied by a large mass which was manifestly a fibro-papilloma.

On the following occasion an endeavour was made to extract it by ordinary forceps without any effect beyond the removal of a tiny fragment. The site of the growth was doubtful but it appeared to be at the front of the larynx. I then used a simple wire snare, the loop being placed transversely, while the epiglottis was raised by Mount Bleyer's instrument, and I removed a good half of the tumour; I was then able to see that it arose from the anterior part of the left vocal cord, and by means of my safety intralaryngeal forceps I cleared away the bulk of the growth, leaving the vocal cord practically clear. Without this combination of instruments the removal of this growth would, under the circumstances, have been of the utmost difficulty.

In a week's time the voice was almost normal and I applied a solution of chloride of zinc to the site of removal.

On November 27 the voice was quite good and the larynx clear.

Microscopically the section showed the structure to be that of a papilloma with round-celled infiltration at its base. "In the absence of giant cells or caseation it was not possible to attribute the round-celled infiltration to tubercular infection."

Dr. WYLIE: Dr. Grant has removed the papilloma with a snare, clean and without leaving a tag. I consider the best way to draw the epiglottis forward is by a thread inserted through the epiglottis with Horsford's needle. An assistant is therefore not required. I have removed several papillomata with the snare but the growths are generally sub-glottic and any piece remaining is removed by forceps. I have sent a list to the *Journal of Laryngology*. I consider no forceps can be compared with those which I bought in Berlin four years ago. These forceps are absolutely steady and easily manipulated. Other laryngeal forceps often move slightly at the moment of grasping the growth.

(*December* 7, 1917.)

Case of Multiple Papillomata of the Larynx in a Child; Tracheotomy and Removal under Suspension Laryngoscopy; Decanulization; Subsequent Recurrence of Stridor; Reintroduction of Tube; Larynx found almost clear by Indirect Laryngoscopy; Tentative Removal of Tube.

By J. Dundas Grant, M.D.

A boy, aged 4, was sent to me at Brompton Hospital in June, 1917, on account of the impossibility of removing a tracheotomy tube which had been inserted two months previously on account of difficulty in breathing, such as to threaten his life. I was able, on laryngoscopic examination, to see that the larynx was occupied by several papillomata. Under suspension laryngoscopy I removed a large number of small papillomata about the size of small split peas. Subsequently I removed his adenoids.

On July 4, owing to the high position of the former tracheotomy, which involved the cricoid cartilage, I made a fresh opening, about 1 in. lower down, and inserted the same tube as before. A repetition of suspension laryngoscopy revealed the complete absence of papillomata. His voice soon returned; he was able to breathe without the tracheotomy tube, and he was sent home.

Two months later his difficulty in breathing returned, and the tracheotomy tube had to be re-introduced. He was sent back to me, and I saw a papilloma attached to the left vocal cord, there being, however, a fair amount of breathing space in the larynx. I removed the papilloma by direct laryngoscopy and also enucleated his tonsils, which were extremely large and buried.

He can now breathe through the larynx, and his voice is good, but I am advising the retention of the tracheotomy tube during the winter months in the expectation that in the interval the tendency to regeneration of papillomata may disappear, remembering the beneficial effect of tracheotomy, even without removal, in cases of papillomata of the larynx, on which the late Hunter Mackenzie, of Edinburgh, laid great stress.

DISCUSSION.

Mr. TILLEY: Did Dr. Grant try the peroral method first, and find he could not succeed with it? It seems a pity to connive at tracheotomy unless there are difficulties which are insurmountable by the peroral method. We should reserve tracheotomic bronchoscopy for cases in which there is severe dyspnœa, or some other emergency, which makes it imperative to open the trachea.

Dr. DUNDAS GRANT: The answer to the first part of the question is in the affirmative. The operation was done under suspension laryngoscopy, and salicylic acid was applied at the same time.

Dr. W. HILL: Without doubt there is a mortality from these cases in young children. There is a very small glottis, and tracheotomy has often to be done, and the last condition of the case is sometimes worse than the first. I have not had a case of multiple papilloma of the larynx for six years; if I had, I should have used either radium or thorium. Results from thorium are said to be marvellous, and we have heard nothing of recurrences. The treatment of warts by thorium has been most successful. There are difficulties in the way of keeping a child under an anæsthetic and holding it in one position for half an hour.

(*December* 7, 1917.)

Lipoma of Larynx removed by Operation (Specimen exhibited).

By HUNTER TOD, F.R.C.S.

MR. C. L., aged 58, was admitted to London Hospital in June, 1916. He had suffered from increasing difficulty of breathing for over twelve months, chiefly at night time and after exertion. Later there was an aggravating cough, sometimes resulting in spasmodic attacks of dyspnœa. A variable inspiratory stridor was present.

On examination the interior of the larynx could not be seen owing to a large sessile tumour, almost blocking up the entrance of the larynx. It seemed to be attached to the posterior part of the larynx and interarytænoid space on the left side. On quiet breathing the growth did not move very much, but on deep inspiration it was sucked down towards the glottis. The surface was smooth and sparsely covered with fine vessels. On touching it with a probe it seemed semi-solid. It had a yellowish appearance and not the bluish translucent look of a cyst.

Under a general anæsthetic the growth was removed by the "suspension" method by means of an ordinary Krause's snare passed along a Hill's slotted laryngoscope. The patient did well.

On examining the larynx two days after the operation the point of origin of the growth could be seen as an irregular, yellowish, raw surface, as if the growth had not been completely removed; but healing gradually took place and three months after the operation the larynx looked normal. The growth itself was about the size of a pigeon's egg, and seemed to be composed of fatty tissue. The covering mucous membrane was only loosely attached to the underlying tissue from which it rapidly retracted.

(*December* 7, 1917.)

Multiple Papillomata of Larynx removed by Operation (Suspension Method) after Tracheotomy; Death Five Weeks later from Acute Membranous Tracheitis and Bronchitis.

By Hunter Tod, F.R.C.S.

Boy, aged 4, admitted to London Hospital on September 7, 1917, suffering from attacks of dyspnœa and stridor, especially when asleep. Symptoms were stated to have dated from birth, but got worse for the two weeks previous to admission to hospital. The vocal cords could not be seen on examination with the laryngoscope owing to an overhanging epiglottis. The child was not hoarse, but would only speak in a whisper.

On September 12 the child was given a general anæsthetic, but before complete anæsthesia was obtained marked signs of laryngeal obstruction occurred rendering it necessary to perform tracheotomy at once as the child was *in extremis*. After the skin incision was made the trachea was incised straight away; to do this the thyroid isthmus, which seemed to be very large, had to be cut through. After the insertion of a tracheotomy tube the breathing soon became normal again. The larynx was examined by the direct suspension method and multiple papillomata were found to fill up the larynx. They grew from the right vocal cord and just above it. They were completely removed, a clear view of both cords afterwards being obtained.

At first the result of the operation seemed most successful and after the third day there was free breathing through the mouth when the tracheotomy tube was plugged. The tracheotomy wound was small and healed well.

Three weeks after the operation there was some redness round the wound and some mucus began to be coughed up from the trachea. This was accompanied by an evening rise of temperature from 100° to 101° F. for two or three days. The red area round the wound slowly increased with induration of the subcutaneous tissues. For the next two weeks the mucous secretion increased in quantity, especially after severe attacks of coughing. These attacks became worse and were associated with extreme dyspnœa, and finally, in addition to the mucus secreted, there was occasionally a considerable amount of blood.

As it was obvious that the child was getting steadily worse, I decided on October 17 to make a further examination. To relieve spasm a small quantity of 1 per cent. cocaine with 1 in 8,000 adrenalin solution was made to trickle into the trachea along the tracheotomy tube. A very small quantity of chloroform was administered. On removing the tracheotomy tube the opening was found to be lined with thick gelatinous looking membrane about $\frac{1}{8}$ in. thick. On inserting a small bronchoscope this membrane could be seen flapping in the trachea during the act of respiration and extended down in irregular masses as far as the bifurcation of the trachea. I removed a considerable quantity of this membrane which lined the opening and upper part of the trachea.

During the examination the child had an extremely severe spasm of dyspnœa and death seemed imminent. Oxygen was administered and a few drops of the cocaine and adrenalin solution were again applied. It was obviously useless to do anything further in the way of surgical interference, but I advised that if further attacks of dyspnœa occurred a soft rubber catheter should be passed into the trachea a few inches beyond the tracheotomy tube. This certainly relieved one or two of the subsequent attacks of dyspnœa, but eventually the child died on October 19 in extreme distress.

The clinical picture was very similar to that seen in a patient suffering from an intrathoracic growth situated at the bifurcation of the trachea, and causing death by obstruction of the bronchial tubes.

Report of Autopsy (Dr. H. M. Turnbull, London Hospital).—Specimen shown. "Patches of thin, granular membrane in upper compartment of larynx. Gelatinous, somewhat tough, yellowish grey membrane (0·3 cm. thick) lining the lower compartment of the larynx and the walls of the tracheotomy opening in the first, second, and third tracheal rings, and almost completely filling the trachea, the left bronchus and the bifurcation of the left bronchus into the bronchi to left and lower lobes. Right bronchus free. Purulent bronchitis and a few areas of collapse in portions of both lungs."

Films from Larynx (prepared at time of autopsy).—"The films were stained: (1) with Gram and neutral red, and (2) by Neisser's method. There were numerous Gram-positive streptococci, in chains up to twelve cocci. There were a few staphylococcal groups of Gram-positive cocci. There were still fewer Gram-positive bacilli. These were slightly curved rods measuring 3 to 8 μ by 1 μ; they exhibited none of the morphological characteristics of diphtheria bacilli."

Microscopical Sections of growth removed from larynx confirm the diagnosis of papilloma of larynx (specimen shown).

DISCUSSION.

Dr. D. R. PATERSON: With regard to Mr. Hunter Tod's second case, I had a case where membranous exudation occurred a few days after operation in similar circumstances, and the child died. Another child had membranous exudation some time after the operation, and we had difficulty in getting it free: the child was ill for a long time, and an autogenous vaccine had a good result. Ultimately the child was operated upon three times, and the tube was left out as the condition subsided. In neither case was there diphtheria infection.

Mr. E. D. D. DAVIS: I have had an epidemic of cases of papillomata, and have curetted four cases, but they all recurred. In three cases the papillomata disappeared when the children reached 10 years of age. With regard to the membrane of obscure origin, it is curious, after enucleating tonsils, how a thick white slough collects on the surface of the cavity. Can this case be of a similar nature? I had one case of enucleation of tonsils in which the inaccurate diagnosis of diphtheria was made, and the patient was sent to a fever hospital.

Mr. HUNTER TOD (in reply): The point about my second case has been rather missed. Mr. Davis compares the condition to that of the membranous slough seen after enucleation of the tonsils; that is common knowledge. This case is almost unique. The operation was successful and, as you will have noticed, there are no papillomata in the specimen. They were removed by the operation. As the child was well and was running about the ward for three weeks the infection of the wound cannot be considered the result of the operation. The reason for postponing a further examination under a general anæsthetic was owing to the symptoms of lung infection. It was only because the spasms increased with attacks of hæmorrhage that one felt that something more must be done. At the second examination a thick gelatinous membrane was peeled off, and although a culture was not taken at the time, this was done at the autopsy, when streptococci, but no Klebs-Loeffler bacilli, were found.

(*December* 7, 1917.)

Case of Foreign Body (Collar Stud) in Left Bronchus of a Child, producing Collapse of Lung of a Year's Duration.

By J. Dundas Grant, M.D.

A boy, aged 9, was taken ill suddenly on December 6, 1916, with vomiting, pain on the left side of the chest, and cough, which soon subsided. He was ill in bed for weeks, and then went away for five weeks.

In August, 1917, he was admitted into hospital. Dr. Perkins found that the right lung extended to the left parasternal line and that there were physical signs of supplemental breath sounds, with no crepitations; the left side was flat and contracted, the lung collapsed, and the stomach elevated. Von Pirquet's test was negative. He complained of pain in the left basal region; there was cough and expectoration; the leucocytes were 9,000. X-ray examination on the left side showed general diffuse opacity, obscuring the heart and diaphragm, the latter being made out with difficulty and appearing to be raised (stomach translucency was higher than normal); the heart was withdrawn to the left; there was no evidence of effusion but of general pleural thickening.

Another examination by Dr. Melville on October 1, showed at the level of the left bronchus a shadow of characteristic shape suggesting the appearance of a collar stud at the spot where an indistinct shadow had been previously noted.

On October 17 I endeavoured, with suspension laryngoscopy, to introduce a tube through the larynx but found that a large tube would not pass. I therefore performed a high tracheotomy and painted the trachea down to the bronchi with 20 per cent. cocaine, and then introduced a bevelled ended bronchoscope, 8 mm. in diameter, for a distance of 4½ in., when I was able to detect the foreign body and to seize it with Killian's pronged forceps, drawing it out with the bronchoscope tube. A good deal of mucus escaped. A Durham's "lobster-tailed" tracheotomy tube was introduced, as the attempt at translaryngeal bronchoscopy had caused œdema of the larynx. The tube was removed in two days.

The condition of the patient rapidly improved, but expansion of the lung must necessarily be slow, and to assist this he is learning suitable exercises in breathing under Mr. Cortlandt MacMahon.

DISCUSSION.

Dr. D. R. PATERSON: Dr. Dundas Grant was wise in doing a low bronchoscopy, because if the foreign body was displaced in getting it out, it might easily have slipped into the other bronchus, and then there would have been very great difficulty, as the lung on the affected side was totally collapsed. Such cases require tubes of as large calibre as possible to facilitate the easy extraction of the foreign body.

Dr. H. J. BANKS DAVIS: In 1912[1] I exhibited before the Section a pathological specimen, now in the Museum of the Royal College of Surgeons, of the larynx of a child, aged 3, with a collar stud impacted between the vocal cords, which caused death. The case had been mistaken by the casualty officers for diphtheria and treated as such with antitoxin. Later a tracheotomy was performed, but in spite of this the child died.

(*December* 7, 1917.)

Case of Pre-tracheal Abscess of Acute Development, in a Middle-aged Woman, producing Dysphagia and Dyspnœa.

By J. DUNDAS GRANT, M.D.

THE patient, a woman aged 40, was seen at Brompton Hospital on October 30, complaining of occasional loss of voice for two years. The larynx was normal, with exception of paresis of the internal tensors of the cords; the posterior wall of the pharynx was glazed with yellow secretion. The sinuses were clear. There was a perforation of the quadrilateral cartilage from a simple ulcer, and active lupoid ulceration on the anterior edge of the right inferior turbinate. A solution of chloride of zinc was applied to the vocal cords and she was ordered a nasal wash.

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[1] *Proc. Roy. Soc. Med.*, 1912, v (Sect. Laryng.), p. 98.

A fortnight later she attended with a painful swelling in the neck, difficulty in breathing and pain on swallowing. She stated that before leaving the hospital on the previous occasion her voice failed, and next day she had "an inflamed throat and high temperature."

On November 13 beyond the paresis of the internal tensors, the larynx was otherwise normal, but there was a red tender swelling over the region of the thyroid body. On the following day I opened the swelling. Pus was evacuated by Hilton's method. The abscess cavity was cleaned out and drained; two days later the tube was removed and a plug of gauze inserted. After ten days the wound had practically healed. The pus contained no tubercle bacilli, but streptococci and staphylococci.

In July she had erysipelas, but had no evidence of tubercle except lupus in the nose. There was no previous swelling to indicate a thyroid cyst which might have suppurated. I am at a loss to trace the causation of this abscess.

(*December* 7, 1917.)

A Surgical Contretemps, illustrating the Value of Endoscopy.

By HERBERT TILLEY, F.R.C.S.

ON November 15 I performed a submucous resection of a septal deformity on a young lady, aged 17. The anæsthetist asked if he might give intratracheal ether by means of a new tube. The latter consisted of two parts: (1) a narrow rubber tube occupied by a coil of wire so that the patient could not obstruct the entry of vapour by biting the tube; and (2) a distal tube about 5 in. long, with a funnel-shaped proximal end to occlude the larynx. The distal end of (1) was intended to fit tightly into the funnel of (2). The conjoined tube was passed through the larynx by direct vision by the anæsthetist, anæsthesia was induced, and the operation went off without a hitch. When it was completed he attempted to withdraw the tubes but only the proximal portion (1) came away. Passing my finger to the upper aperture of the larynx I was unable to feel the funnel-shaped portion of the distal tube (2)—it was obviously in the trachea! A friend motored to my house for my endoscopic outfit, and after some fifteen minutes' interval I was able to see and withdraw the funnel-shaped tube from the trachea—no harm has resulted from the accident. Obviously the only alternative would have been a tracheotomy, and in pre-endoscopic days a "difficult situation" might have been created.

(Intratracheal tubes exhibited.)

DISCUSSION.

Mr. FRANK A. ROSE: Recently, when doing submucous resections of the septum, Mr. Boyle has anæsthetized for me *per rectum*. In all these cases, including bronchoscopy, the anæsthetic has been very satisfactory.

Mr. W. M. MOLLISON: I have operated upon one or two septa with intratracheal insufflation of ether. The object of this method is to prevent blood entering the trachea. There is one disadvantage in this method, for the back stream of air through the nose is embarrassing: bubbles of air come through the nose and form a froth with any blood present. I have given it up for that reason, though it is an ideal anæsthetic for most nose operations. Rectal anæsthesia does not prevent blood going down the trachea.

Mr. TILLEY (in reply): The object is to prevent blood entering the larynx, and from that point of view the intratracheal method is ideal, but as Mr. Mollison said, the only inconvenience of the intratracheal method is that blood tends to froth forwards into the nasal cavities. For bigger operations about the nose and throat I think the intratracheal method is ideal. I have not seen rectal anæsthesia induced for two or three years, but the objection used to be that the patient afterwards suffered from tenesmus and diarrhœa and bloody stools.

(*December* 7, 1917.)

Osteoma of Left Frontal Bone.

By HUNTER TOD, F.R.C.S.

MALE, aged 33. For eighteen years he had noticed that the left eye was becoming more prominent and was being gradually pushed down and outwards. He had suffered constantly from headaches, worse on stooping. There had never been any discharge from the nose.

He was operated on in April, 1913, by a surgeon in Lincolnshire. I saw him first in December, 1913, and admitted him into the London Hospital in January, 1914, for operation.

A curved incision was made just beneath the eyebrow. On exposing the bone the roof of the orbit was found to be irregular and nodulated, and a great deal of very hard bone was chipped away from the roof, but the frontal sinus could not be found. The dura mater was not exposed for fear of setting up meningitis; and it is possible that owing to this the operation was not sufficiently extensive. The wound healed well after the operation and the patient was much relieved of his headaches.

I have not seen the patient since he left the hospital, but as his medical man has written to me saying that there is a recurrence of the severe headaches and apparently of the growth itself, I have asked the man to attend this meeting in order that the members may see his present condition which I cannot for the moment foretell.

A photograph of the patient is shown, taken just before the operation was performed in January, 1914.

DISCUSSION.

Mr. TILLEY: The recent increase or recurrence of headaches is probably due to re-appearance of the condition which Mr. Tod dealt with in the early stage. If you pass a finger as far backwards as possible over the upper surface of the globe you can feel a small hard tumour.

Dr. PERRY GOLDSMITH: In view of what the patient says I should suggest that he is better left alone.

Mr. HUNTER TOD (in reply): I agree with Mr. Tilley that on examining with the finger one can feel a growth. At the first operation that was the chief characteristic. I removed bone as far back as I could, but did not completely remove the growth, being afraid to injure the optic nerve. I have not seen him till now since the middle of 1914. The pain in the head may be due to pressure. Mr. Gilbert Scott has shown a case in which, after one of these growths was removed, the patient, a year afterwards, came up suffering from headache with rattling in the head. A skiagram showed the brain to be compressed and, above the level of the brain, a layer of fluid and air. Probably some puncture had been made through the dura mater in removing the osteoma. It was the recollection of this case which made me fear to do more here.

(*December* 7, 1917.)

Stenosis of the Glottis, from a Combination of Tubercle and Syphilis, in a Woman, aged 51, requiring Tracheotomy.

By Sir StClair Thomson, M.D.

This patient was shown before the Laryngological Society of London on November 2, 1906.[1] The case was discussed by Messrs. Barwell, Powell, Smurthwaite, Dundas Grant, Cresswell Baber, Watson-Williams, de Havilland Hall and Atwood Thorne.

Mrs. D. first came under notice in 1906 for hoarseness and marked dysphagia. The condition in her larynx at that time was entirely attributed to tubercle, for, although tubercle bacilli were never found in her sputum, there were signs in the upper lobes of both lungs. Low tracheotomy under cocaine was performed in February, 1906. Since those days the Wassermann test has been discovered, and later on we found that the patient gave a positive reaction. Patient has quite a good voice. Although warned on the subject, the patient has neglected to come up as frequently as necessary to have a worn-out tracheotomy tube replaced by a new one. (Her tube has been worn, unwashed and unchanged, for seven years.) It will be noticed that the one I show is a mere shell, and that the vertical limb has completely worn away. The interesting points of the case are the following: (1) Combination of tubercle and syphilis; (2) comfort of a low tracheotomy; (3) the permanency of the cicatrization of the larynx; (4) the absence of pulmonary symptoms in spite of wearing a tracheotomy tube for eleven years; (5) the necessity of renewing tracheotomy tubes; (6) the risk that some patients will run, even although warned.

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[1] Vide *Proc. Laryng. Soc. Lond.*, 1906, xiv, p. 4.

(*December* 7, 1917.)

Stenosis of the Glottis, from Combined Tubercle and Syphilis requiring Tracheotomy, in a Man, aged 41.

By Sir StClair Thomson, M.D.

Mr. P. C. was discharged from the Navy two years ago for tubercular laryngitis. Tubercle bacilli were reported to be present. When he passed under the care of a County Tuberculosis Officer repeated examinations failed to find them, and his Wassermann reaction was reported negative. He came under observation in June, 1917, with marked stridor, general infiltration of the larynx, ulceration of the cords, the glottic space reduced to a chink, no dysphagia and no enlarged glands, and the temperature was normal. The Wassermann reaction was positive; tubercle bacilli were found in his sputum. A low tracheotomy was done under cocaine, and he has since been treated with salvarsan and mercurial injections.

Both these patients will have to wear a tracheotomy tube permanently. A Durham tube, inserted as low as possible in the trachea, appears to me the best.

DISCUSSION.

The President: The first case would be a remarkably good cure if it were tubercular, the only unfortunate feature now is double abductor paralysis.

Dr. Irwin Moore: I remember the first case very well, and many times changed the tube when I was senior clinical assistant to Sir StClair Thomson. The patient was constantly warned of the risks of wearing the tube too long without removal and cleaning, but there was always great difficulty in getting her to attend for this purpose. But, as is so well shown in this case, when a low tracheotomy has been performed, patients wear their tube without any discomfort, in fact they scarcely know that they are wearing a tube at all.

Dr. Smurthwaite: I have tried to elicit a movement of the arytænoid joints, but they do not seem to work at all: they are fixed. If the patient says "E," the cords come together and are made more tense by the action of the internal tensors, but there is not the slightest movement in the abductor direction. I think the fixation is of a fibroid nature, due to the former tubercular or specific implication of the joints.

Dr. DONELAN: I have had under observation a case which has been examined by other members, in which fixation of the arytænoids was the earliest symptom, and there was difficulty in deciding whether or not it was tuberculous. The articulations on both sides were fixed for eighteen months, and only in the last month has the patient shown definite signs of tuberculosis. It appears to be a primary case. The present case I thought was one of fixation of the arytænoid articulation. I was uncertain as to the evidence of tuberculosis in the notes. Perhaps Sir StClair Thomson will tell us what was said at the meeting of the old Society.

Sir STCLAIR THOMSON (in reply): With regard to the first case, it was accepted by myself and by all who spoke as one of tuberculosis. In those days the Wassermann reaction had not been discovered, and we saw no other signs of syphilis. It was thought to be a rare case of tubercle of the larynx, which had not only healed spontaneously. but in doing so had produced stenosis, which is rare in my experience. The patient turned up after four years; in the interval the use of the Wassermann test had come in, and we found it was positive. From my own experience, I think we were all wrong, and that though she may have tubercle in her lung, I think the condition really is syphilis, because it is rare to get tubercle in the larynx advanced enough to produce contraction in a patient who is not a good deal worse in health than this woman ever was. I view it as a false ankylosis I do not think it could be a case of involvement of both recurrent laryngeal nerves or their centres, and that would not account for the interarytænoid space having disappeared, and the two arytænoids being so closely locked together. With regard to the second case, we must not rely implicitly on the pathologist, as many are inclined to do. We should trust to our own clinical acumen more than we do. The man tells me he has not been to a sanatorium, but has gone to live in his cottage. He has put on a lot of weight, and he comes back feeling well. We cannot now find any symptoms in his lungs, and the tubercle bacilli have disappeared. Dr. Emery made three examinations with the same result. Tracheotomy in both cases has been not only necessary, but I think that in both it will be permanent. I plead again for prompt and low tracheotomies. Mr. Hunter Tod's case was allowed to go on -not through any fault of Mr. Tod's—to dyspnœa and stridor, before tracheotomy was done, and then tracheotomy was done on the same day as a fairly extensive operation on the larynx. Dr. Banks Davis has handed round a picture of a child with a stud in its glottis, and that child had extreme dyspnœa: tracheotomy was performed, and yet the child died. That often happens in hospitals because people will not do tracheotomy in time, and the patient gets back pressure on the heart.

(*December* 7, 1917.)

Cyst of the Epiglottis of Unusual Size.

By W. Jobson Horne, M.D.

The patient is a woman, aged 20. No symptoms noticed before last August. Since then there has been a steadily increasing difficulty in swallowing, but sufficient food can still be taken. The voice has become "deeper." The patient sleeps comfortably. The tumour springs from the left ary-epiglottic line and occupies the greater part of the brim of the larynx, so that the right ary-epiglottic ligament alone is visible. The absence of any material discomfort or inconvenience with so large a tumour in the brim of the larynx is remarkable.

(*December* 7, 1917.)

A Larynx with a Cystic Tumour of the Epiglottis.

By W. Jobson Horne, M.D.

The cystic tumour of the epiglottis gave rise to no symptoms during life. It was accidentally found at the post-mortem examination.

Dr. Perry Goldsmith: I once had a comical experience in a case like this, which I showed at the Laryngological Section of the Toronto Medical Society. Everybody said they failed to see any tumour at all. I put my mirror on to look, and it had gone. The man then said something had "given way" when he was coming to the meeting, but he had said nothing about it.

(*December* 7, 1917.)

Paralysis of the Left Vocal Cord in a Woman aged 32.

By W. H. Kelson, M.D.

I first saw the patient in August, 1916, when she complained of hoarseness. She gave the history that eleven months previously the left tonsil had been removed by a surgeon, and a month after this a gland, said to be tubercular, was removed from the neck on the same side. Immediately after this operation great hoarseness and difficulty in swallowing came on.

On examination the left vocal cord was found to be almost immobile, and the mucous membrane of the larynx when touched with a probe appeared somewhat insensitive. The voice was very husky. Since then the voice has somewhat improved and the difficulty in swallowing very much lessened. The patient, who before marriage was a nurse, says she has never had any lung trouble, nor children, nor miscarriages. She tells me she also had food regurgitation through the nose after the operation for removal of glands.

(*December* 7, 1917.)

"Fibrosarcoma" of Nasopharynx.

By Norman Patterson, F.R.C.S.

Patient, a boy, aged 18. Two years ago noticed small swelling in left cheek. In September, 1915, a portion of the swelling was removed at Eastbourne and reported on as a myxo-fibroma. He complains of long standing obstruction on the left side of the nose, alteration in speech, pain in the left cheek and over the left eye. There is a firm swelling in the temporal fossa and another below the zygomatic arch; the zygoma itself lies in a hollow. The nasopharynx is occupied by a large firm swelling. The patient is evidently suffering from a growth which started in the nasopharynx and has passed out through the pterygo-maxillary fissure into the zygomatic fossa and then upwards into the temporal fossa.

I propose to treat the case by radium passed into the substance of the tumour and X-rays applied over the side of the face. Before treatment a portion will be removed for microscopic examination.

(*December* 7, 1917.)

Endothelioma of the Post-nasal Space.

By W. H. JEWELL, M.D.

A GIRL, aged 13, first noticed something amiss four months ago on account of speaking through her nose; lumps (glands) in throat three months; intermittent headache and earache two months. The growth is attached to the left lateral and posterior wall of the nasopharynx, bulging the soft palate forward; it is fairly firm, extends as high as the left choana, and bleeds readily on digital examination. The lymphatic glands of the left side of the neck are enlarged and extend nearly as low as the clavicle; in places they are firm and discrete.[1]

(*December* 7, 1917.)

Salivary Calculus.

By IRWIN MOORE, M.B.

PATIENT, a male, aged 56, attended hospital on November 16, 1917, complaining of a swelling on the right side of the neck under the angle of the jaw for one year, together with a swelling under the tongue, from which a month previously some watery fluid stained with blood had been discharged, followed by considerable diminution in the size of the neck swelling. A number of septic teeth had been recently removed from the neighbourhood of the swelling.

On examination: The right submaxillary gland was found to be enlarged, hard, but mobile. On the right hand side of the floor of the mouth, about midway, in the position of Wharton's duct, an irregular, hard, swelling was observed from which pus exuded in small quantity. A piece was removed for microscopical examination.

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[1] Shortly after this child was shown she developed cerebral vomiting, followed by blindness of the left eye, then of the right, and ending in coma and death two months later.

Report on Section.—"Sections of this tissue show considerable thickening of squamous epithelium with œdema and inflammation of the underlying tissues. There is certainly considerable hypertrophy of the epithelial processes, which dip into the submucosa, but in my opinion the condition is inflammatory, probably the result of local irritation, and not malignant. It is only fair to state, however, that in consultation the opinion has been expressed that this is an incipient squamous epithelioma" (Dr. Eastes' Laboratory).

This section is being submitted to Professor Shattock.

DISCUSSION.

Mr. TILLEY: I remember a similar case of salivary calculus where there was a swelling under the jaw, and when the calculus moved into the duct there was considerable pain, especially during meals. If the calculus ulcerates through the duct the condition becomes septic, and the induration with granulations produce an appearance like malignant disease. I suggest that the hardness towards the median line is due to a few crystals of the calculus blocking the opening of the duct.

Dr. IRWIN MOORE (in reply): When I first saw the case my diagnosis was salivary calculus, but, as Sir StClair Thomson has just pointed out, we are al somewhat inclined to assign too much importance to the pathologist's report. In this case Dr. Fletcher had expressed his opinion that the piece removed from the floor of the mouth was not malignant. Yet in consultation with others it was considered to be early squamous epithelioma. However, since sending in the notes for the agenda, Dr. Ironside Bruce made a film skiagram which shows a large calculus embedded in the sub-maxillary gland. There is a fistula in Wharton's duct which is discharging freely, and accounts for the swelling of the gland having subsided since I saw the case a week ago. Professor Shattock reports that the section of the growth examined by him is not malignant.

Postscript.—On December 11, under an anæsthetic, the calculus was located in Wharton's duct, close to the submaxillary gland, and was removed by incision. It measured 10 by 7 mm.

(*December* 7, 1917.)

Malignant Disease of the Larynx.

By Irwin Moore, M.B.

Patient, a female, aged 58, when first seen on October 4, 1917, complained of hoarseness of the voice for one year following a severe cold, accompanied the last six months by cough, and for three months by blood-stained sputum. There has been some difficulty in breathing for four months. On admission to hospital on October 13, considerable inspiratory stridor was present, which necessitated tracheotomy, and this was performed by Mr. Macnaughton-Jones. There is no history of tuberculosis nor of syphilis. The Wassermann reaction is negative. A few enlarged glands are present on both sides of the neck. There is no difficulty in swallowing, but patient has wasted considerably, her former weight having been 10 st.

Examination of the larynx shows marked stenosis caused by infiltration and fixation of the soft parts on the right side. The entire larynx is distinctly congested. The arytænoids are not enlarged.

Examination of the lungs by Dr. Halls Dally on November 24 showed that the percussion note was impaired above the left clavicle in front, and in the left supraspinous fossa behind, accompanied by moist sounds, which however were difficult to differentiate from the sounds caused by the presence of the tracheotomy tube. There is considerable difficulty in coming to any definite conclusion as to the condition of the lungs. Examination of sputum is negative.

(*December* 7, 1917.)

Removal of Foreign Body Impacted in the Œsophagus.

By G. C. Cathcart, M.D.

Mrs. S., aged 45. She came to me on October 27 and gave the following history. At lunch she had just taken a piece of duck when someone spoke to her. She hastily gulped it down, found it too big, and tried to bring it back into the mouth but failed; she felt choked and tried to drink some water; she then sent for a doctor who passed a rubber tube. She was apparently relieved for a time but as the day wore on began to get more and more uncomfortable. When I saw her at 8.30 p.m. she complained of great pain in the centre of the breastbone, with increasing difficulty of breathing and inability to swallow anything. She was pale and haggard-looking and in a cold sweat and evidently in great distress. As she was certain there was no bone in the piece she swallowed, I did not have her X-rayed but sent her at once into a nursing home. Under a general anæsthetic I first passed a tube into the larynx and found it clear, then I passed the œsophagoscope and at a distance of 8 in. a large dark greenish mass was seen. With the forceps I had, I only succeeded in bringing away pieces of the meat. As I did not have a pair of Dr. Irwin Moore's forceps, I asked him to come and bring his. He then passed them and at once brought up a large mass with the bone attached. Fortunately the sharp point of the bone was turned down, otherwise it would have scratched the œsophageal wall as it was coming up, but the patient suffered no ill-effects.

This case shows the advisability of always having an X-ray photograph taken, and also the need of every œsophagoscopist having a pair of Irwin Moore's forceps as an essential part of his armamentarium, as one never knows when they may not be wanted. The bone and a drawing of the pieces of duck are shown. The bone measured 30 mm. (1⅜ in. in length) by 13 mm. (½ in. in breadth), and the weight of the meat attached to it was ¼ oz.

(*December* 7, 1917.)

Case for Diagnosis.

By G. C. Cathcart, M.D.

Mr. J. S. was brought to me on October 27, 1917, by Dr. N. Turner, complaining of persistent hoarseness. He is aged 67. He was staying at Ramsgate in April, 1916, when an explosion took place at an ice factory. He went to assist the firemen, and in a short time had a choking feeling and had a pain in his throat and chest, owing to the escape of ammonia. The pain continued for a few days after his return to London. In December he had a bad cold and a return of the same pain that he had had when helping after the explosion; this lasted for a few days. Some months later he found he was getting hoarse and had difficulty in speaking.

On laryngoscopical examination the whole larynx was seen to be inflamed, and there was a great deal of swelling in the interarytænoid space, over the arytænoids and on the cords, and there was what looked like heaped-up necrotic tissue on both cords and in the arytænoid space. There was evidently some perichondritis of the arytænoid cartilages, as they were partly fixed, and there was only a small space left for breathing, which caused him to have stridor at night.

I removed a piece of the uvula, which was causing him considerable irritation and was very much elongated. Two days later I did a low tracheotomy. The patient has now been wearing the tube for a fortnight, and during that time has not used his voice at all. He has still a great deal of swelling in the interarytænoid space and over the arytænoid cartilages, but no necrotic tissue is to be seen.

I have never seen a case quite like this before, and should be very glad to have the opinion of the members in regard to it.

DISCUSSION.

Mr. Tilley: I think it is a tuberculous gland. There is much œdema of one glosso-epiglottic fossa, and there is much ulceration on the laryngeal aspect of the epiglottis. The laryngeal appearances are in favour of tuberculosis also, although there are no physical signs in his chest. I think there may be an old tubercular focus in the apex of one or both lungs, and that the laryngeal mucous membrane has been infected.

Sir StClair Thomson: There is no doubt about it being tuberculosis, but it is worthy of notice that laryngeal tuberculosis in the elderly is a very protean affair, and is apt to deceive people—that is to say, it does not follow the routine symptoms. It often simulates syphilis, or malignant disease, and it is commonly found in the elderly in what is regarded as a primary form. It is also apt to be acute and to kill its victims quickly. That seems rather a contradiction, because in middle age tubercle of the larynx is often a mild condition, but tubercle of the larynx in the senile is apt to be very severe.

P.S.—The patient died six weeks after the meeting.

(*December* 7, 1917.)

Case of Tuberculosis of Larynx.

By W. Jobson Horne, M.D.

Patient, a discharged soldier, aged 22. He joined up in March, 1916, and was in France in December, 1916. He was invalided out in May, 1917. He has had sore throat and dysphagia for several months, but not before the War. There is no family history of tuberculosis. No sputum is available. The thorax has not at present been examined. His weight is 9 st. 7 lb. Larynx: Considerable œdema and infiltration of epiglottis and arytænoid region.

In the first place treatment by scarification is proposed. Discussion on treatment is invited.

CORRIGENDUM. In the last number of the *Proceedings* remarks made by Dr. Watson-Williams upon Dr. Irwin Moore's case (pp. 39-45) by an unfortunate accident got out of place and appeared under Dr. Kelson's case (p. 38).

Section of Laryngology.

President—Dr. A. Brown Kelly.

Demonstration on Rhinoplasty.[1]

By H. Delf Gillies, F.R.C.S.

Of all the plastic operations of the face, rhinoplasty is the most difficult, except perhaps otoplasty, of which I have had no experience. The reason why these two sections of the work are so difficult is because they deal with the repair of "end organs."

In approaching the subject of the demonstration, the problem is to be solved from the point of view of the three main elements of the normal nose—that is to say: (1) The outside covering, the skin; (2) bony and cartilaginous framework, and (3) the mucous membrane lining.

Whatever element of the nose has been destroyed has to be replaced. In certain classes of cases—for instance, in burns of the nose, it is necessary to provide the skin covering only.

In another large class of case in which the skin covering and the mucous lining are more or less intact, the provision of the supporting element will suffice to restore the organ, whereas in the majority of gunshot injuries there is loss of all three elements. This loss may be partial or complete, and for the purposes of discussion I have divided the "loss of nose" into various groups, according to the portion of nose lost.

In discussing these classes of cases, I am illustrating the various methods employed by myself and my colleagues, whether the results have been satisfactory or indifferent. The first class of case is that of loss of portion of the tip, or of the alæ.

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[1] At a meeting of the Section, held February 1, 1918.

The photographs of six different methods of repair were shown by means of the epidiascope. For the larger loss of the tip the Keegan-Smith Indian method was preferred. The six cases include an excellent result (by Captain G. Seccombe Hett, R.A.M.C.) with a "Y" shaped cheek flap to form one ala and columella; and one successful case of a skin-grafting method to form an ala—by Lieutenant-Colonel Newland, D.S.O., of the A.A.M.C.

The next category demonstrated the loss of the upper quarter of the nasal bridge. The lining mucous membrane is of no importance in this situation, and both cases illustrated were treated by the same method. An osteo-periosteal flap was turned down from the frontal region to give support to the skin flap, which was advanced from the forehead.

For the next class of injury, loss of the upper half of the bridge of the nose, with two cases of mine and one of Captain Hett's, three methods were illustrated.

In the first, cheek flaps were advanced over a septal swing, as a preliminary operation, with the late embedding of a cartilage graft.

In the second, the support was supplied by osteo-periosteal graft from the tibia, and was not so satisfactory.

The third, Captain Hett's, had the cartilage support embedded in the flap in the frontal region, the under surface of which was epithelialized before it was brought into position.

Losses of the lower third of the nose were next demonstrated, and two main methods were advocated—one a modification of the Keegan Smith operation, and the other an original design of my own. A series of mechanical supports were also illustrated, showing the methods adopted in the early stages of nasal injuries. Three cases of superior maxilla fractures, with loss of the supporting structure, were treated by implantations of cartilage.

The pug nose type was the next group defined, and its solution solved in the following manner: Cartilage is embedded under the skin of the upper part of the nose, which is subsequently swung down as a combined skin and cartilage flap, the skin lining the new nose, and the cartilage is tucked in behind the remains of the tip to retain the latter in its correct position. The operation is completed by frontal flap in the usual manner.

The importance of taking a spare piece of cartilage and embedding it under the skin at the time of the rib operation was illustrated. This method (cartilage carriers) saves a double rib operation in many a case.

When the entire nose has been destroyed, the difficulties are correspondingly increased, and several cases of partial failure were shown. The successful ones were those in which provision of a mucous or skin lining to the new nose had been adequately made.

Captain J. L. Aymard's attempt to make a nose by a long chest flap was illustrated, as well as a case of severe burn of the face, in which I had grafted a large chest flap on to the face, covering the nose.

The following are details of a Case of Subtotal Rhinoplasty also demonstrated at this Meeting and that of November 2.

1719 PRIVATE D., Royal Fusiliers, aged 19. Wounded, France, February 17, 1916; admitted, May 10, 1916.

Condition: There was complete loss of all the tissues of the nose, bone, skin, and cartilage, except the left nostril, portion of the tip, and a small piece of the anterior end of the septum—about ¾ in. triangular.

Methods employed: (1) Building up the bridge, and providing a mucous membrane lining for the new nose by turbinate grafts. Operation, June 20, 1916: Left middle turbinate detached from behind, and stitched to the remains of the septum. Operation, August 9, 1916: Right inferior turbinate detached from behind, leaving its anterior end attached and superimposed upon the freshened surface of the left middle turbinate (new position). Operation, September 19, 1916 (local anæsthesia): The attached pedicle of the right inferior turbinate was cut through and stitched on to the freshened surface of the anterior portion of septum remaining.

(2) Rhinoplasty (Indian method): Operation, October 11, 1916. Return of pedicle: Operation, October 28, 1916.

(3) Supports used for this frontal flap: (*a*) Celluloid, October 11, 1916: A perforated celluloid plate was stitched into position to form an additional support to the bridge between the turbinate and the frontal flap. Similarly, a small bent celluloid strip was stitched into position to form a support for the right ala. This was unsatisfactory and had to be removed on January 29, 1917. (*b*) April 17, 1917: Implantation of cartilage (heterogeneous). This was unsatisfactory owing to its becoming absorbed. About the third week after the operation the nose became swollen, and about a drachm of pus was

evacuated. A few drops only of pus could be squeezed out for the next few days, and the further healing was uninterrupted. On examination, after two months, the cartilage had become absorbed. (*c*) September 18, 1917: Implantation of cartilage (autogenous). This cartilage was taken from the seventh costal on the right side—length about 1¾ in.; it was split longitudinally but not quite through, so that it was, when inserted, straddled as an inverted **V**.

(4) Small corrections have been made of the right ala, but this part is still incomplete.

Case of Total Loss of Hard Palate and Exposure of the Antral Cavities in the Mouth.[1]

By W. KELSEY FRY, M.C., and G. S. HETT, F.R.C.S.

AS a result of an aeroplane fall this officer, Lieutenant W., R.F.C., was struck in the mouth by the handle of his machine gun. He was operated on as an emergency case in France, the operation consisting in detaching pieces of the hard palate and alveolar pieces of the jaw, with their contained teeth, which were literally hanging by a thread. On examination, it will be seen that the whole floor of the nose is absent, that the outer wall is also gone on the left side, exposing the cavity of the antrum, which is occupied by a single polypus. On the right side there is an opening into the antrum below the inferior turbinal. Both antra are suppurating.

The case is shown from two points of view: (1) as a double traumatic antral suppuration, with the loss of the floor of the nose; (2) from the dental point of view.

Captain Fry, M.C., R.A.M.C., surgeon in charge of the dental department at the Hospital for Facial Injuries, Sidcup, is exhibiting models and diagrams, showing the method of fitting a denture, and making use of the floor of the antrum and the remains of a nasal crest as means of attachment for an artificial palate.

A pastel drawing of the condition, by Professor Tonks, F.R.C.S., is also exhibited.

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[1] The cases described on pp. 90-98 were demonstrated at the meeting of the Section held November 2, 1918.

Dental Notes on Lieutenant W., R.F.C., by

W. Kelsey Fry, M.C.

This case shows loss of the entire hard palate, together with the alveolus on both sides. The whole of the soft palate is present. It is of interest as it demonstrates the difficulty of supporting the prosthetic appliance necessary. At present the undercuts to which we look for support are inefficient. It also shows how important it is for the oral and dental surgeons to work in conjunction.

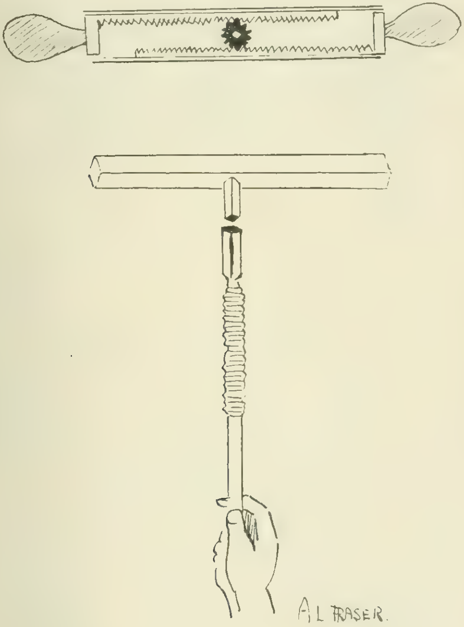

Fig. 1.

Shows mechanical movements of processes for retaining mould in position.

The supports to be used are: (1) laterally, floors of the maxillary antrum; (2) anteriorly, a ridge consisting of the anterior nasal spine and upper part of the pre-maxilla.

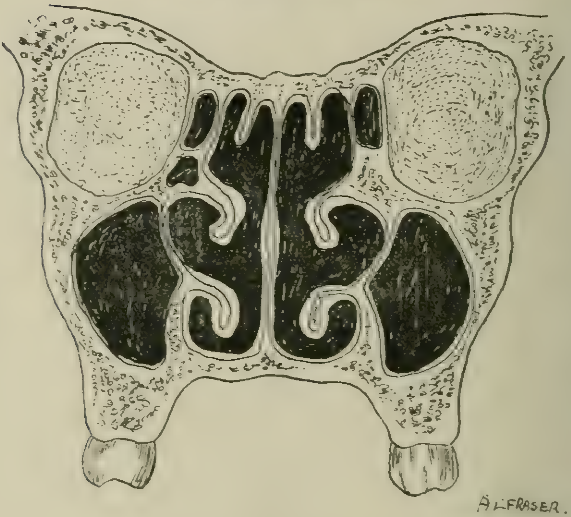

Fig. 2.
Coronal section of nasal cavities.

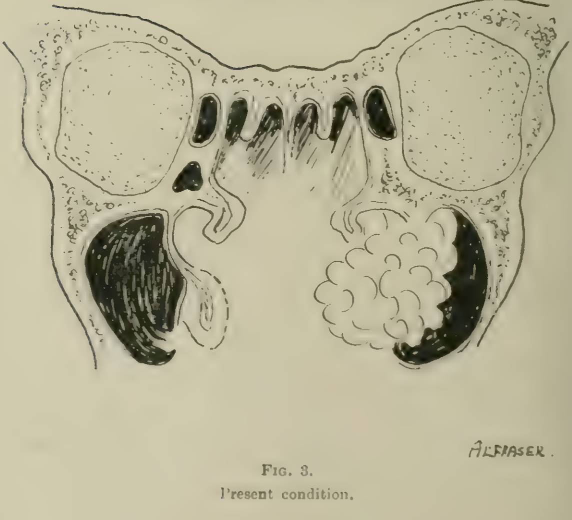

Fig. 3.
Present condition.

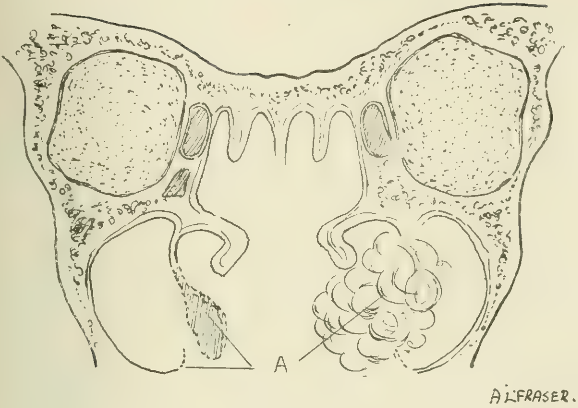

FIG. 4.

Condition after operation; **A**, parts removed.

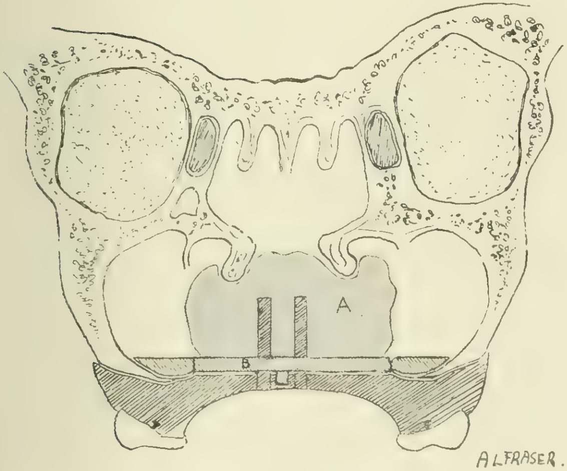

FIG. 5.

A, **B**, condition, with prosthetic apparatus in position, showing mould with denture attached by split pins.

The patient is now wearing a hollow box made to fit the cavity, the shape of which has been obtained from a plaster of Paris impression. The permanent apparatus is to be made as follows: A mould will be made to fit into the cavity, as shown in the figure. It will carry three movable processes, one for each opening into the maxillary antra, and one for placing above the anterior ridge. The patient will be able to place the mould in position and then turn the processes outwards, thus preventing the mould dropping. A denture will be attached to the mould by two split pins, which will be received into two slots provided for these pins in the thickened posterior part of the mould.

Sectional diagrams have been drawn by Lieutenant A. L. Fraser, R.A.M.C., dental surgeon; skiagrams by Lieutenant Johnston, R.A.M.C.

Case of Total Loss of Nose, illustrating the Method of Advancement of the Turbinals.

By G. S. Hett, F.R.C.S.

Lieutenant S. This is a case of total loss of nose due to gunshot wound. There is considerable sinking in and added loss of tissue of the left side due to loss of the nasal process of the left superior maxilla. In this case Major Gillies has placed the costal cartilage graft on the forehead. The forehead flap is to be brought down shortly to form the new nose.

The case shows an attempt to build up the inside of the nose and to make use of the turbinals and septum to diminish the undue size of the cavity and to form a mucous lining to the new nose.

All four turbinals have been advanced and the upper septal swing has also been made use of.

Operation, September 1, 1917 (Lieutenant Hett): Intranasal operation preparatory to plastic operation for complete new nose. The left middle turbinal was detached from its connexions posteriorly and swung forward, so that its posterior end was brought in contact with and attached to the anterior end of the left inferior turbinal. The right inferior turbinal was similarly treated and its posterior end swung up and attached to the root of the nose. The cartilaginous septum was detached from the floor of the nose and also from the vomer, the ethmoid remaining attached by a pedicle at the root of the nose. It was rotated and placed laterally so that it bridged across the space between the right inferior turbinal and the left middle, to both of which it was attached by catgut suture.

October 2, 1917: Following last operation there is now a mass of tissue filling up the cavity to the left of the middle line.

Operation, October 2, 1917 (Lieutenant Hett): The posterior end of the right middle turbinal was found to be firmly attached to the septum. Its anterior attachment was now separated and swung downwards to floor of the nose and attached there.

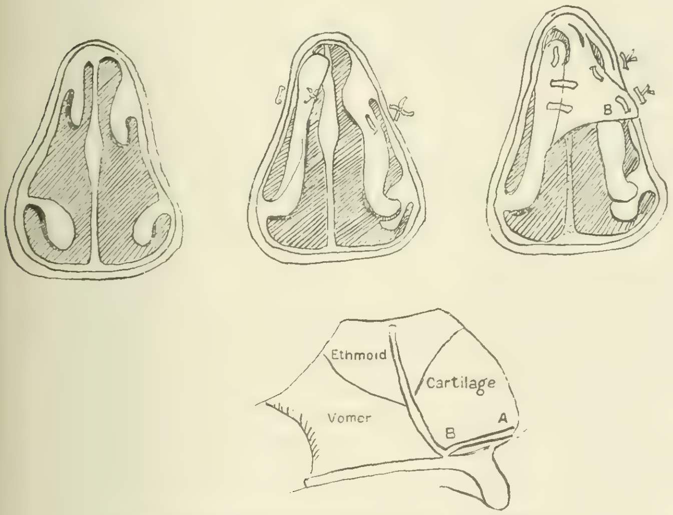

Diagram illustrating turbinal shifts and upper septal swing.

Shrapnel Wound of the Left Side of the Face.

By G. S. Hett, F.R.C.S.

Rifleman T. H., aged 32. Wounded, May 27, 1917. Admitted, July 11, 1917. Transferred to Queen's Hospital, August 21, 1917. Shrapnel wound left side of face. Left half of nose absent. Antrum open on face. Opening from floor of antrum through gingivo-labial fold.

Operation, September 27, 1917 (Lieutenant Hett): The opening between the antral cavity and the mouth was closed by raising the antral mucous membrane round the opening and closing the edges by silkworm gut sutures. The upper lid was everted and the mucous membrane of the alveolus and inner surface of upper lip was also sutured together by horsehair sutures. The left inferior turbinal was detached posteriorly, twisted forwards, and its posterior end attached to the anterior portion of the floor of the nose so as to form a support for a subsequent plastic operation on the nose. The right ala nasi was adherent to the septum by adhesions. The ala was partly detached below and the adhesions removed and a tube inserted into the right nasal cavity, thus establishing the patency of the right nasal passage.

Case of Sunken-in Nose, illustrating the Employment of an Autogenous Costal Cartilage Graft for Septal Support.

By G. S. Hett, F.R.C.S.

Private C. This patient received damage to the face from a kick of a horse while serving in France. As a result of this the septal support of the nose sloughed. The nasal bones remained. As a result of the injury the nose drooped and fell in from the middle of the bridge to the tip, causing a marked prize-fighter type of face. It was found on examination that the whole of the septal cartilage and part of the vomer were missing, and the soft parts of the septum had healed and were intact.

Operation: An incision was made down the mid-line of the columella from the tip of the nose down to the junction of the columella with the upper lip. This was deepened and the soft tissues of the septum were separated backwards until the remains of the vomer were met with. The division was continued upwards and forwards under the skin of the bridge and tip. The two sides were separated down to the floor of the nose in the same manner. By proceeding carefully it was possible to complete the separation without making a perforation. Meanwhile Captain Aymard had exposed the eighth costal cartilage on the right side. A quadrilateral piece of cartilage, rather larger than the normal cartilage of the septum and about the same thickness, was removed and cut into shape to fit tightly between the septal flaps. The usual submucous resection plugs were then inserted and kept for twenty-four hours.

Tracings from photographs show the condition before and after operation.

Notes on a Case of Post-traumatic Laryngeal Stenosis treated by Homologous Cartilage Graft.

By G. S. HETT, F.R.C.S.

PRIVATE R. was admitted to University College Hospital suffering from loss of voice and stridor. The patient had a high tracheotomy wound. He was wearing a tracheotomy tube, but even with this he had some stridor, especially at night. He had a history of having taken poison by mistake, and then cutting his throat in desperation. An emergency tracheotomy had been done, and it had not been possible to dispense with the tube six months after the date when first seen (October 23, 1917). The tracheotomy wound had touched the lower border of the thyroid cartilage, traversed the cricothyroid membrane, the cricoid cartilage, and first ring of the trachea.

Operation: A low tracheotomy was done. The upper tracheotomy wound was opened and very marked stenosis was found. This was due to the fact that the anterior wall of the respiratory passage in this situation consisted of nothing but scar tissue. There was a very narrow chink between the lower border of the thyroid and the cricoid. The narrowed passage was blocked above the tracheotomy opening by polypi. The polypi were removed, scar tissue dissected away, and an india-rubber tube inserted well up into the larynx and down into the trachea.

The patient managed without the low tracheotomy tube for a time, but it had to be reinserted at intervals. After another month it was found impossible to retain the lumen of the passage owing to contraction and want of support of the anterior wall. A plastic operation was therefore decided on.

Second operation: The thyroid cartilage was exposed. The flap had its base on the right, and was turned across the neck to the side. The scar of the cut throat, which lay above the flap, was excised. The thyroid cartilage was split, and some scar tissue at its lower border was excised. The cords and rest of the larynx were normal. The scar tissue over the cricoid and tracheal region was freely cut away. Dr. Butterfield had meanwhile exposed the larynx and upper part of the trachea of a dog in the annexe to the operating theatre. He excised and brought me the portion after washing it in normal saline solution. I had intended to use the dog's thyroid cartilage, but found that the anterior half of the trachea fitted better into the gap. The graft contained five rings of cartilage. The mucous membrane of the inner surface was retained. The graft was sutured by catgut into position, and the infrahyoid muscles were detached above and wrapped round it. Lastly, the skin flap was slid downwards and formed a covering to the whole.

The patient did well; the low tracheotomy was allowed to close on the fourth day. Subsequently two half rings of the graft appeared on the surface in a local abscess and were removed. The wound then healed, and the patient had a good airway, lost his stridor, and developed a deep voice, whereas before he only had a hoarse whisper.

Four months later he was brought into the hospital with acute abdominal symptoms. He died in a few hours. At the post-mortem the cause of death was found to be a strangulated diaphragmatic hernia.

This case is interesting as showing that the homologous graft persisted and formed a support and new anterior wall to the trachea.

The specimen is shown, and also a microscopic section by Dr. Butterfield, through the region of the homologous graft.

The microscopic sections were taken from a small thin piece removed from the left side of the median incision. These show small islands of cartilage, some of which are in intimate relationship to the lower end of the thyroid cartilage. These contain practically all the characters of normal cartilage, except that there is some fibrillation of the matrix.

Rhinoplastic Case.

By J. L. Aymard.

(Introduced by H. Delf Gillies, F.R.C.S.)

There is complete loss of nose, right eye, and partial loss of upper lip, following a gunshot wound. The case presents many of the difficulties attendant upon nasal reconstruction.

Mr. Aymard: A paper of mine on "Some Points in the Anatomy of the Septum" has been reprinted in the *Journal of Laryngology* for last month,[1] and I hoped to demonstrate the circulation in the cartilage of the septum I have here. It requires to be taken out of the spirit ten minutes, and then returned to the spirit, and removed: it will then show all the ramifications of the vascular system in the septal cartilage, which is only a prelude to the general circulation in the cartilage. I stated that operators often thought they had performed a subperichondrial resection, whereas, what was done was only a submucous resection.

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[1] *Journ. Laryngol.*, October, 1917, p. 308.

Turbinal Shifts and Septal Swings.[1]

By G. S. Hett, F.R.C.S.

By the former I mean advancement of the middle, or inferior turbinals, and have found this manœuvre of great use in gunshot wounds of the nose, in helping to fill up gaps, in acting as supporting structure to a new nose, or portion of a nose, which has to be made. They also help to form a new mucous lining for the new nasal cavity.

Case I.—Private F. This case illustrates the advancement of the right middle turbinal.

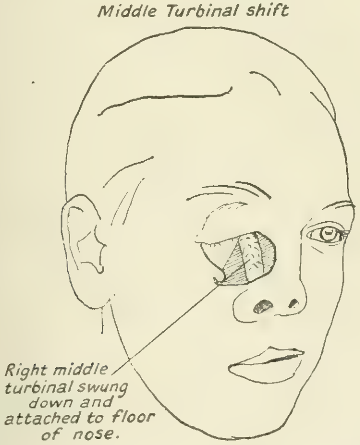
Middle Turbinal shift
Right middle turbinal swung down and attached to floor of nose.

Fig. 1 (Case I).

Case II: Septal Swing.—Lieutenant S. This shows a somewhat complicated mass of turbinals brought forward to help fill up the gap, in a case of Major Gillies, of total loss of the nose. (This case has already been shown by Major Gillies, *re* Rhinoplasty.) The diagram explains the shift of all four turbinals and in addition the upper septal swing. In this case the detached septum was swung laterally at Major Gillies' suggestion to try to compensate to some extent for the loss of the nasal process of the left superior maxilla.

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[1] At a meeting of the Section held February 3, 1918.

Case III.—Private M. A combined case of Major Gillies and my own, where an L-shaped septal swing was used to form a support for the bridge of the nose, and tip of the new nose.

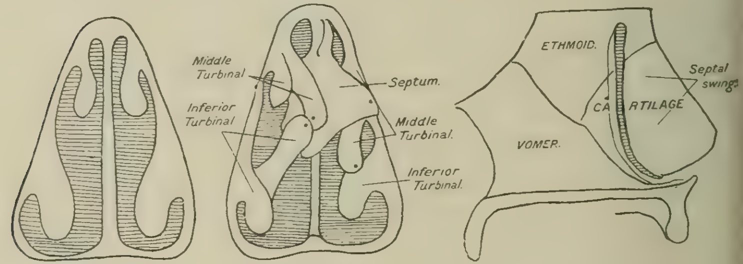


Fig 2 (Case II).

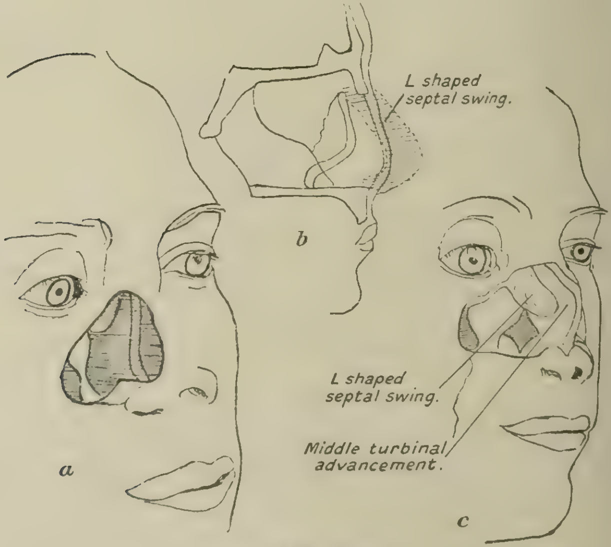


Fig. 3 (Case III).

Case IV: Nasal Supports.—Private C. This illustrates the method of septal support by making a new cartilaginous septum. This method is only applicable where the soft tissues remain, but may have a very useful application in cases of sunken bridge in civil as well as military cases.

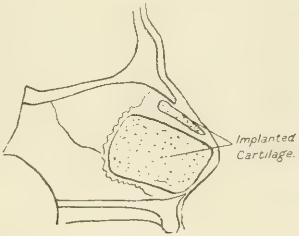


FIG. 4 (Case IV).

Case V.—Private B. This illustrates the same condition. The nose was held down by a scarred right ala, so that the latter had to be cut free before the tip could be advanced. In cases where septal support is needed but the soft tissues of the septum are lacking, two pieces of cartilage are introduced, one along the bridge through a tenotomy wound and the other by splitting the columella as in Lieutenant Hastings's case.

Case VI.—Private H. In this case I used a modified septal swing to form a new columella, and later implanted an L-shaped piece of cartilage to give prominence to the bridge, and support the columella.

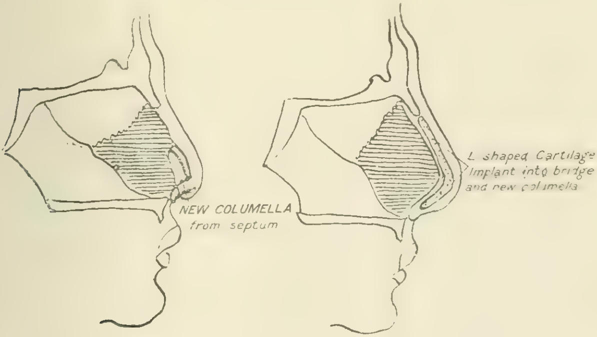


FIG. 5 (Case VI).

Case VII.—Private C. In cases of bringing down a nose from the forehead, the supports for the bridge, columella, and ala are usually planted in the nose before bringing down. The most difficult problem in my mind in rhinoplasty, is to get sufficient prominence for the tip, so that all additional means are useful. In the case now shown I have arranged several accessory supports which I trust may be added safeguards. At the time of the implantation of the cartilage in the forehead, I buried two rods of cartilage under the mucous membrane of the floor of the nose. They are now living and have a mucous membrane covering. When bringing down the nose I intend to swing them forwards and join their raw surfaces together and so have a median mucous membrane covered rod to act as a prop and replace the original septum.

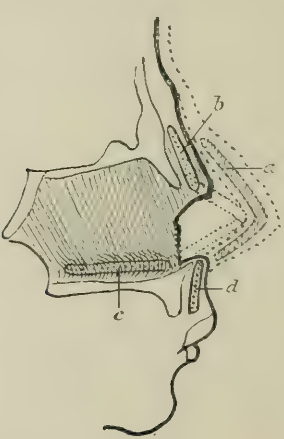


Fig. 6 (Case VII).

Cartilage grafts : *a*, in forehead flap ; *b*, in bridge of nose ; *c*, in floor of nose ; *d*, to form nasal spine.

I have also endeavoured to make a new nasal spine. In making a new nose, besides the tendency of the tip to sink, there is the bad appearance of the upper lip, and its junction with the columella. This is due to the frequency with which the nasal spine and crest are lost. I endeavoured to replace this with a new cartilaginous spine introduced through the mucous membrane of the upper lip. The forehead columella is to be attached directly to this and the cartilages joined. Further, I have placed a rod of cartilage under the skin of the bridge, which can also be swung downwards in order to form a still further support for the bridge and tip.

THE MAKING OF A NEW ALA.

Where one ala is destroyed, a vertical flap from the junction of the nose and cheek may be turned down to form a new ala. This is a simple method, but unless the ala is supported and the raw surface of the new vestibule is covered with epithelium, it will inevitably shrink, so that the contour of the ala is destroyed and nasal obstruction ensues. In order to obviate this contraction in one case (Private H., No. VIII), where the right ala had been destroyed together with the anterior end of the right inferior turbinal, I removed the anterior half of the left inferior turbinal and used this as a free gift to support the right ala. The turbinal was implanted in the position of the right ala. It established a living connexion with the tissues. A fortnight afterwards the mucous membrane from its superficial surface was removed and the usual skin flap was brought down to cover it. In this case, therefore, the right vestibule was lined with mucous membrane instead of skin. A serviceable but somewhat rigid ala was produced. In subsequent cases where possible, I have turned down a semilunar flap of skin previously epithelialized on its under surface by von Esser's method (Private W., No. IX).

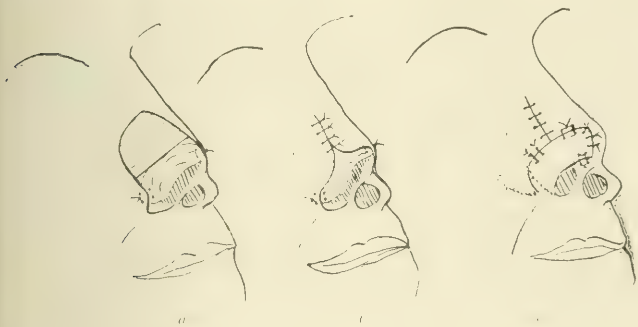


FIG. 7 (Case VIII).

A useful amplification of this method is to include a piece of cartilage between the skin lining of the vestibule and the external flap. Where alæ, tip and columella have to be made (Private R., No. X), I have found the **Y**-shaped flap of service. The flap must be cut thick,

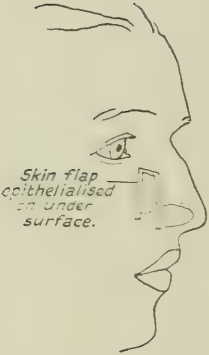


Fig. 8 (Case IX).

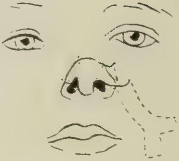


Fig. 9 (Case X).

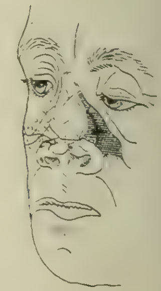


Fig. 10 (Case XI).

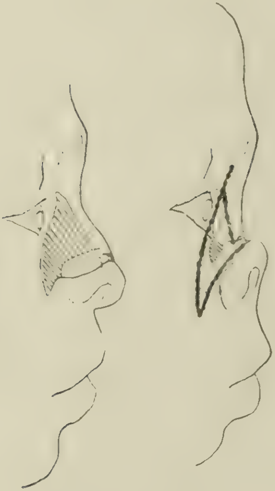
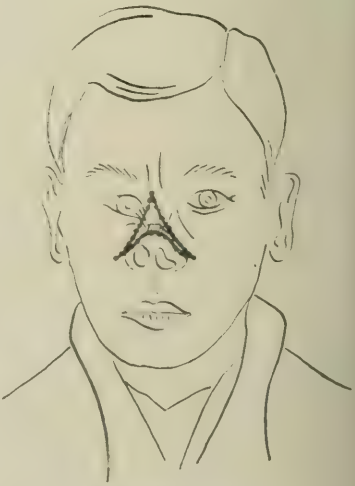


Fig. 11 (Case XI

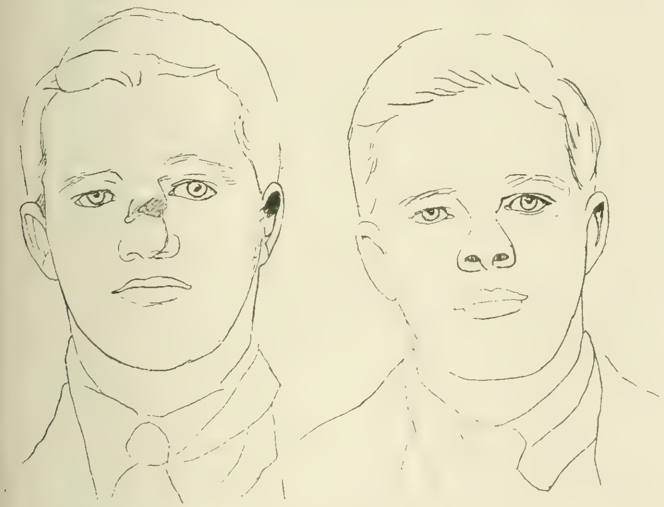


Front view of Fig. 11.

Fig. 12 (Case XII).

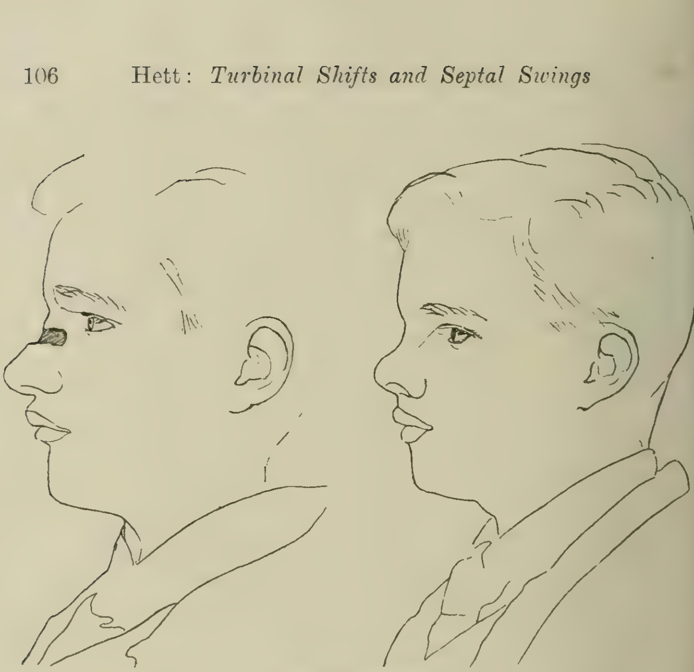
especially over the tip and columella, as otherwise the tip will contract and not be sufficiently prominent.

The result of the operation for restoration of ala, tip and columella was satisfactory but the left vestibule became somewhat contracted. In order to obviate this I employed the following manœuvre: A Stent was moulded round an india-rubber tube. The contracted skin and scar tissue of the vestibule were dissected away and the Stent introduced while still soft, so that it formed an exact impression of the cavity. When cooled and hardened it was removed and covered with Thiersch skin-graft. It was then re-introduced and stitched firmly into position. The drainage tube, which was passed through it, allowed free vent of mucus from the nose. Eight days afterwards the Stent was removed and the vestibule was found to be lined with healthy epithelium. This established a patent nostril and has prevented subsequent contraction.



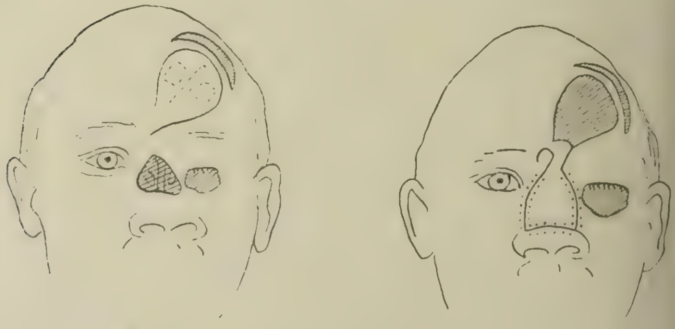
a Fig. 13 (Case XIV). *b*

Front view before operation. Front view after operation.



a FIG. 14 (Case XIV). *b*

Side view before operation. Side view after operation.



a *b*

FIG. 15 (Case XV). Front view.

THE PUG NOSE TYPE.

In cases where the lower portion of the nasal support—e.g., the septum—has been destroyed, the alæ and tip tend to sink in and take up the vertical position (Private C., No. XI). In these cases where possible the lower septal swing is adopted and the tip and alæ are brought down from the vertical to the horizontal. A new nose is then brought down from the forehead and either joined to the tip and alæ in their new position or placed over the tip and alæ, the latter surface having been previously rawed. In another case (Private R., No. XII), the new nose was merely brought down and joined to the alæ and tip, whereas in the last case the alæ and tip were overlapped by the new nose.

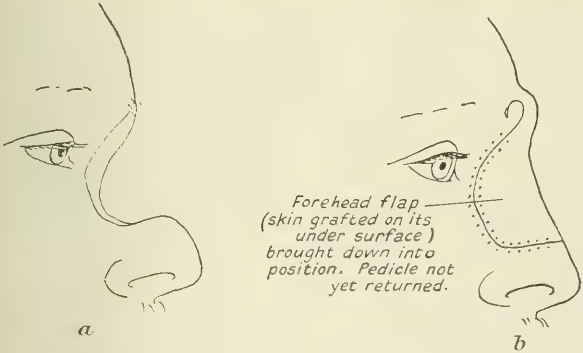


FIG. 16 (Case XV). Side view.

THE BIRD BEAK TYPE.

In these cases the upper portion of the nasal support—i.e., the nasal bones, have been damaged or destroyed. In the first case of this type shown (Private C., No. XIII), the external wound had closed but there was considerable nasal obstruction. Intranasal adhesions were removed and a submucous resection was performed which was carried far forwards so as to depress the tip. The removed cartilage was cut into four strips and these were introduced superimposed through a tenotomy opening so as to elevate the bridge.

In the next similar case (Private S., No. XIV), the upper portions of the nasal cavities were open on the surface. The middle turbinals were advanced so as to fill up the gap, and one vertical and two lateral flaps, as shown in the diagram, were used as a skin covering. In this case also there was considerable nasal obstruction. At a subsequent operation a month later the intranasal adhesions were cleared and a submucous resection was performed. The removed piece of cartilage was reinserted, with its long axis vertical so as to form a support for the upper portion of the bridge of the nose. A very fair result was obtained but the nose remained somewhat tip-tilted. The patient then came under Major Waldron, the head of the Canadian section at Sidcup. In order to improve the appearance still further we introduced a rod of cartilage from the eighth rib under the skin of the bridge.

A third case of the same kind (Private H., No. XV). Three pieces of cartilage have been introduced into the forehead: one to give prominence to the bridge, and two lateral ones to reproduce the nasal bones. After epithelialization of the under surface of the forehead flap, the latter has been brought down into position.

DISCUSSION.

The PRESIDENT: Besides the satisfaction of the surgical successes, it must be a source of gratification to those engaged in rhinoplastic work to reclaim for ordinary society men whose appallingly repulsive injuries would otherwise have condemned them to a life of isolation. When the war is over, and the cases requiring restorative treatment are rare, I hope that the experiences gained in this work will be utilized in forwarding a branch of our specialty which has been neglected by most of us. I refer to what has been termed "corrective rhinoplasty," which deals with noses that are healthy but either by their abnormal size or shape, cause disfigurement. At first, this subject might seem too unimportant to be worth serious attention, but on glancing at the literature one cannot but be struck with the variety of conditions requiring treatment, and the excellent results obtainable.

Sir STCLAIR THOMSON: Are these instruments inside the nose for keeping it up successful, or have they, in some cases, proved failures? We might have to try them for accidents to the nose arising in the football field or at boxing matches. In reference to the lining of the nose, I think it was said that if one does not get epithelial lining the nose contracts. Apart from that, does much difficulty arise from the absence of ciliated epithelium? Do these noses which are lined with ordinary mucous membrane—i.e., grafts of mucous membrane, keep moist? Or do the patients get rhinitis sicca, or ozæna, or crusts inside the nose? In cases in which I have had to remove the turbinals entirely for malignant disease of the ethmoid region, as I have reported here before, I have been astonished that the patients have remained free from ozæna or atrophic rhinitis, a condition we used to anticipate in those cases. That experience has made me think that atrophic rhinitis is much more a pyogenic result than one of difficulty or defect of turbinal tissue and ciliated epithelium.

Mr. HERBERT TILLEY: In the case of a slight dent or depression following a submucous resection, do the exhibitors recommend the insertion of a portion of septal cartilage from another patient, or the removal of a small portion of rib cartilage from the same patient?

Mr. H. D. GILLIES (in reply): In reply to Sir StClair Thomson, I have not seen any case of atrophic rhinitis resulting from these injuries of turbinals or the measures taken for the repair of the nose. One point in connexion with complete rhinoplasty is interesting from the voice-production point of view. One of the officers I showed lost a good part of his nose, as well as a portion of his superior maxilla, and it did not matter whether he had no nose, or whether Mr. Hett had moved his turbinals, or whether I had remade his nose, he had the same nasal resonance, and his smell was normal from the beginning. I have not seen any case of atrophic rhinitis among these, and there is a possibility, if other methods are not available, that skin grafting in the nose might be a means of curing atrophic rhinitis. Skin in the nose used as mucous membrane, as also in the mouth, and in the eye-sockets, does admirably, in my experience: it causes no trouble of any character. What it becomes histologically is not, I think, yet established, but from a practical point of view it is very satisfactory.

Mr. G. SECCOMBE HETT (in reply): With regard to Mr. Tilley's question as to autogenous *versus* heterogeneous grafts in the nose, I think he spoke entirely of the septal cartilage graft. Both have been tried, the septal cartilage in the autogenous and the heterogeneous manner, that is to say, grafting one person's septum into his own nose, and also into that of another patient, and that, so far, has proved satisfactory. With regard to heterogeneous costal cartilage grafts, Mr. Gillies knows about those better than I do. I have thought there was not so much vitality about the heterogeneous as about the autogenous grafts. With regard to Sir StClair Thomson's question, I do not know whether he includes unhealthy conditions, such as sinus disease or atrophic rhinitis, when speaking of the interior of the nose, but I appreciate the extreme necessity of making the cavity healthy before putting on a new nose; for instance, in one case, a man had a chronic antral condition, a new nose was brought down, and had to be lifted off again and the antra cleared out because the nasal cavities were full of putty-like material, and both antra had foul pus in them.

New Laryngotomy and Tracheotomy Tube.

By A. L. Abel.

(Introduced by H. J. Banks-Davis, M.B.)

This is a modification of Fuller's bivalve tracheotomy tube. The difference is that one limb of the valve has been lengthened $\frac{3}{16}$ to $\frac{4}{16}$ in., and the concave border of that prolongation has a scalpel edge. The object is to render the time-interval between the tracheal incision and the insertion of the tube *nil*. The method of use has been to make a small skin incision first, transversely or vertically, and, without anything further being done, the instrument is held between the thumb and finger, and by a slight rotatory movement the tube is carried into the trachea or larynx. After the tip of the smaller valve has passed the obstruction, which it does easily, the patient is at once heard to breathe, and the inner tube is then inserted. With this guarding the point, the instrument is driven home. The instrument fits its incision exactly, and there is no escape of blood or air. There is no need to drive it home, neither does it need tying. So far, it has been used in fifteen or twenty cases of laryngotomy: for tracheotomy, only on the dead body. It has been employed in connexion with operations on the jaw, throat, and tongue. It has been said that it might cause sepsis by healing up so rapidly; but the wound heals by first intention and is closed almost at once, and leaves no scar. No sepsis has been produced inside the trachea, and it has given gratifying results.

Epithelioma of Soft Palate and Uvula.

By Andrew Wylie, M.B.

Male, aged 38, complained in October, 1917, of pain in the throat, and later of difficulty in swallowing, with slight cough and spit. Dr. Rothwell, Maidstone, saw him in November, and at once administered potassium iodide and mercury, after tuberculosis had been excluded.

Patient attended Central Throat Hospital in December and there was marked symmetrical ulceration of the soft palate, extending to the tonsils. The uvula was also affected. The Wassermann reaction was *positive* and the appearance was similar to specific disease, but even large doses of anti-specific remedies had no effect. Chest normal; no tubercle bacilli found. Enlarged gland has appeared recently on the right side. A piece was removed, which Dr. Wingrave reports as typical epithelioma.

Microscopic specimen shown.

I intend to treat the case by diathermy.

Case of Frontal Sinus Suppuration; Suppurative Meningitis for Fourteen Days; Operation; Recovery.

By W. M. MOLLISON, M.C.

B. S., AGED 29, was admitted to Guy's Hospital in July, 1917, on account of acute frontal sinus suppuration in the site of a previous operation. The man had previously had over thirty operations of the nose and both frontal sinuses, all in a provincial hospital except the last, which was in Guy's in December, 1916, when a thorough operation was performed on the left sinus; during his recovery from this, pain along the course of the left supra-orbital nerve was a prominent symptom. On admission in July, 1917, there was a red tender swelling in the left supra-orbital region and considerable discharge of pus from the nose; operation was performed and pus found in the sinus; drainage into the nose was established and a rubber tube inserted; at the same time, as the nose was narrow, a submucous resection of the septum was performed. The man made a good recovery except that he complained still of pain along the supra-orbital nerve; he was discharged in August.

On September 27 he was re-admitted on account of continued pain and nasal discharge. On October 1, the left sinus was again opened and more bone removed from the limits of the sinus and granulations gently curetted; a very thin fronto-ethmoidal cell was found far back over the orbital roof containing granulations; again a tube was inserted along the passage from the operation cavity to the nose and kept in place for ten days. After operation the patient's temperature at once rose to 100° F., and remained between 100° and 102° F. all through the illness. He was ill, complaining of slight headache, his mouth and tongue were dry and he was somewhat delirious at times. On the twelfth day after operation (October 12) he had a rigor and his headache became severe. Lumbar puncture showed cloudy cerebro-spinal fluid under increased pressure; the puncture was repeated daily for the next fourteen days, the needle being left in place each day for about two hours; it was impossible to keep it in longer as the man had so much pain in the back and legs.

Six days later (October 18) further operation was undertaken, as the meningitis did not appear to be getting worse and no organisms were found in the fluid, though it was crowded with leucocytes. The previous supra-orbital incisions were re-opened and joined by an incision across the bridge of the nose, and from the inner end of the left supra-orbital incision one was carried up on to the forehead for 2 or 3 in. Bone was removed to expose the dura mater over a considerable area; the bone showed small spaces containing granulations towards the root of the nose and the intersinus region; these granulations led the operation as it were to the right side, which was found to contain beads of pus. When the bone had been removed between the sinuses just above the root of the nose, a small vessel was seen standing out as a strand between the dura mater and the nose; this vessel was taken to be the most anterior part of the superior longitudinal sinus and as it had been surrounded by septic bone it was considered to be a possible source of the meningitis; it was therefore ligatured in two places and the intermediate piece removed for section; microscopical examination showed it to be a vessel with an organizing thrombus (this is shown). The dura mater over the whole area was normal in appearance, and the superior longitudinal sinus showed no sign of thrombosis higher up.

After operation the patient at once improved, the headaches ceased, and the temperature came down, and the cerebro-spinal fluid soon became normal. Finally on November 20 the skin flaps were adjusted.

DISCUSSION.

Dr. DAN MCKENZIE: The case is interesting because of the question which arises as to how the meningitis was produced, presumably by this thrombosis. I think it is the first case in which a thrombus has been discovered in the operation for frontal sinus trouble. I would like to enter a caution against the use of the term "suppurative meningitis," which is conventionally limited to cases in which bacteria are found in the cerebro-spinal fluid. In this case I do not think they were found: consequently it belongs, strictly speaking, to what is called "serous meningitis." The difference in the prognosis between these forms is very great, and that is where the nomenclature is important. This case may come into the category of milder meningitis cases, but all the same, the result of the treatment is certainly a matter for congratulation.

Mr. MOLLISON (in reply): The little vein in the section shows thrombosis in the lumen, and outside shows chronic inflammation. I think the inflammation may have been going on in this vein since the submucous resection was performed, three months before the man developed the meningitis.

Calculus of Tonsil.

By C. H. HAYTON, F.R.C.S.Ed.

D. C., AGED 35, an Italian patient, was sent to the Throat Department of the Royal Chest Hospital by Dr. S. Greenwood, of Goswell Road, who suspected a calculus in the tonsil. The patient complained of irritation at the back of the tongue, with a constant desire to swallow. An examination of the mouth revealed an enlarged red bulging left tonsil quite in contrast to its fellow on the opposite side. At first sight it simulated a peritonsillar abscess, but the readiness with which the patient opened his mouth and the absence of pain precluded the possibility of a quinsy. A small white spot was noticed on the tonsil. A probe elicited the stony hardness of a calculus. With local anæsthesia the stone was readily removed. It is a pyramidal-shaped stone, the base measuring 2·7 cm., altitude 2·2 cm., and thickness 1·8 cm., and the weight 7·9 grm. No examination was made for the *Leptothrix buccalis*. From appearances it is composed of phosphate and calcium carbonate.

Stone exhibited.

The PRESIDENT: I suppose that Mr. Hayton, in mentioning leptothrix, has in mind the view that these calculi may develop around leptothrix filaments in crypts, but I think such an occurrence extremely unlikely. Leptothrix is abundantly present in the mouth, and the caseous masses in lacunar tonsillitis swarm with them, yet calculi of the tonsils are very rare. Many years ago, when trying to show that what was then termed "mycosis leptothricia" was a keratosis, I planted an excrescence of keratosis from a patient in one of my tonsillar crypts to see if a mycosis would develop, but with negative result. Long afterwards pus began to come from my tonsil and one day while squeezing this out a tiny calculus came away. There is little doubt as to its having formed around the excrescence. A foreign body probably in most cases forms the nucleus of a tonsillolith.

Tumour of the Epiglottis.

By W. Jobson Horne, M.D.

The patient was present at the meeting of the Section on December 7, 1917. The tumour was then described as a cyst of the epiglottis. Subsequently it was further examined and found to be a solid tumour. A portion was removed for microscopic examination and reported to be a round-cell sarcoma. The use of radium has produced a most satisfactory result.

Dr. Jobson Horne (in reply to a remark by the President): I would like to show the microscope specimen at a future meeting, with a view of obtaining opinions upon the nature of the growth. The term "sarcoma" appears to me to be too comprehensive.

Case of Laryngo-fissure.

By A. L. Macleod, M.B.

W. B., aged 55, engineer. First seen on May 16, 1917. Complained of hoarseness, lasting eighteen months; no cough; no difficulty in swallowing; abstainer, moderate smoker. On examination a nodular growth was found on the anterior third of the right cord, which was fixed.

Patient was seen on May 21 by Sir StClair Thomson, who advised operation, with reservation on account of (*a*) length of case, (*b*) fixation of cord, and (*c*) possible invasion of cartilage.

On May 25 a Wassermann test proved negative.

On June 6 laryngo-fissure was performed, Dr. Irwin Moore kindly assisting. Recovery was rapid and uneventful.

Pathologist's Report.—"Section shows a typical extensive epithelioma."

Sir StClair Thomson: I have a drawing which will show the extent of the growth in this case before it was operated upon. It should be one of our objects to show how these epitheliomata can be treated, for, unfortunately, in the Provinces and elsewhere, they have hitherto been left in the hands of the general surgeon, with disastrous results. To-day I received a letter from America, showing that they suffer over there in the same way.

Case of Orbital Tumour.

By J. GAY FRENCH, M.S.

THE patient, a widow, aged 58, by occupation a dressmaker, stated that about nine years ago she first noticed a bulging of the left eye. This condition continued until about eighteen months ago, when the eye began to water, and she then felt some pain in the eyeball.

She first came under my care on November 1, 1917, when it was noticed that the left eye was displaced, and that there was a definite bulge above the inner canthus. There was no diplopia, and an examination of that eye with an ophthalmoscope revealed nothing abnormal. The swelling was soft and fluctuating, but on deep pressure a definite bony mass could be felt. An examination of the nose revealed nothing abnormal beyond the fact that the left middle turbinal was slightly enlarged.

An X-ray plate was taken, and it shows definite thickening on the upper and inner side of the orbit. This extends also into the ethmoidal region. Transillumination showed the left frontal sinus dark.

Within the last month the growth has definitely increased in size.

DISCUSSION.

Mr. HOWARTH: This is a mucocele of the ethmoid and left frontal sinus. I have seen about eight cases. In some the ethmoid alone is involved; in some the frontal sinus alone; in one both frontal sinuses were affected and one ethmoid, so that one could get fluctuation from the right frontal sinus to the left ethmoid. The duration of this case, nine years, is not unusual. One of mine was of seventeen years' duration. I think if Mr. French were to cut down on that, he would find the bony tumour was involving the ethmoid and possibly the left frontal sinus, and that if he drained it into the nose, the eye would go back, and the patient would be cured. The contents in these cases vary a good deal: sometimes there is thick mucus, sometimes inspissated pus. Usually the contents are sterile, but sometimes they contain bacteria.

Mr. O'MALLEY: I think this is a mucocele, but it is difficult to say whether it is connected with the ethmoid or with the frontal sinus. If I were called upon to deal with it surgically, I should not think of attacking it merely from the front aspect where it is presenting above the inner canthus, because, as has been pointed out, sometimes these mucoceles are connected with the frontal sinus back wall, the sinus may be absorbed, and thus one gets sepsis and meningitis. I think this should be opened over the point where it is presenting, and then be connected freely with the ethmoidal region, after taking away the middle turbinal, so that there is a safe way into the nose for free drainage.

Sir StClair Thomson: In my text-book there is a photograph of a case very like this, but more exaggerated. That patient was a medical man, and the frontal sinus was full of the gelatinous sort of material which Mr. Howarth described. Dr. Logan Turner has pointed out the non-necessity, indeed the danger, of being too operative in these cases. As Mr. O'Malley says, you may only stir up sepsis, and sufficient has been done so long as you secure good drainage into the nose.

Mr. Gay French (in reply): I am going to operate on the case next week, and shall try to effect drainage into the nose. I hope to report the result to the next meeting.

Three Cases of Symmetrical Nodules of the Vocal Cords.

By E. D. D. Davis, F.R.C.S.

Case I.—A doctor's widow, aged 38, of no occupation, complained of hoarseness of six weeks' duration. She was neither a singer nor voice user, and had neither cough nor any other symptom apart from hoarseness. The larynx shows typical, symmetrical, pale, small "singers' nodules" on the anterior third of both vocal cords. No adequate cause for the condition can be found.

Case II.—A female munition worker, aged 26, attended the hospital for hoarseness of twelve months' duration. Symmetrical reddish nodules, about the size of a small peppercorn, can be seen on the anterior third of both cords. The patient has a dry cough, but is not a voice user. The nose is normal. When she was first seen there was slight pharyngitis.

Case III.—A stage-hand, aged 32, first seen in December, 1916, suffering from left frontal headache, with nasal catarrh and chronic laryngitis. There was considerable oral sepsis, and after dental care, the œdematous left middle turbinal was removed, and the septal deformity corrected by a submucous resection. There was no sinus suppuration. The nose improved, but the chronic laryngitis remained. The patient had frequent mild attacks of bronchitis with persistent cough, but repeated examination of the sputum and chest did not reveal pulmonary tubercle. Alcohol was forbidden. In April, 1917, symmetrical œdematous projections, resembling singers' nodules, developed on the anterior third of both vocal cords, and have not disappeared in spite of treatment.

Symmetrical Ulceration of the Vocal Cords.

By E. D. D. DAVIS, F.R.C.S.

A WOMAN, aged 47, was first seen in April, 1916, and complained of gradual loss of voice of three months' duration. An examination showed symmetrical ulceration, with formation of granulation tissue on the anterior third of both vocal cords. Repeated examinations of the sputum and chest were negative. Weight constant; Wassermann reaction negative, and there is no history of syphilis. The condition has remained unaltered in spite of varied treatment.

DISCUSSION.

Mr. HERBERT TILLEY: Some of these patients do not make any particular use of their cords, and it is just possible we may have to give up the idea that these vocal cord nodules are all caused by excessive use of the voice. I have seen cases in young children who have not been particularly vociferous either indoors or in the playground, and I have seen them in adults who, even in reply to leading questions, could not admit there had been undue use of voice. I presume Mr. Davis will treat them in the usual way, that is, if they do not get well after prolonged rest, that he will apply the point of the galvano-cautery, and by the indirect method.

Sir STCLAIR THOMSON: These are very interesting cases, because I have been puzzled by singers' nodules occurring in people who are neither singers, nor orators, nor shouters. Some I have found had a deaf mother or other relative, and the shouting needed may have been the cause of it. With regard to the case of symmetrical ulceration of the vocal cords, I have watched such a case three or four years and tried to get at the cause whether syphilis, tubercle, nose trouble, tobacco, alcohol—but found nothing to account for it.

Dr. JOBSON HORNE: During the past year I have seen several similar cases. With regard to treatment, I am not in favour of an application of the galvano-cautery to the vocal cords straight away; they seem to get all right with rest of voice and a general improvement in health. Hospital patients recover robust health more slowly and find it more difficult to rest the voice; this probably accounts for the persistence of the condition in some cases. In the last case I could not see ulceration of the vocal cords, the condition seemed to be similar to that in Cases II and III, but in a more acute stage. It would be instructive to receive a further report upon this group of cases.

Mr. FRANK A. ROSE: One feature common to the first two cases of symmetrical nodules of the vocal cords is the high degree of laryngitis which is present: there is swelling and redness of the mucous membrane and of most of the interior of the larynx. That is much more intense and conspicuous than is usual in cases which one calls singers' nodules. I do not recall a case of the latter, in a schoolmistress, for example, in whom there was this degree of laryngitis.

The PRESIDENT: The interesting question about nodules is their ætiology. I agree with Mr. Tilley that while they usually occur in those over-using or abusing the voice, such as school teachers, freak singers, &c., they are also found in young children. The cause probably lies in the manner in which the cords are pressed together, possibly owing to the action of the external thyro-arytænoids. These nodules develop where maximum contact takes place, in the middle of the ligamentous part of the cord. They are usually epithelial hypertrophies, sometimes glandular. I think there is no better way of treating the very small nodules than by lightly touching them with the cautery and then drawing it away. Some members of the Section are very averse to this method.

Mr. E. D. D. DAVIS (in reply): I am much indebted to members for their suggestions as to the pathology and treatment of these conditions. I am sorry the first case did not come, but it was a typical case of symmetrical nodules on the vocal cords, known as singers' nodules, but which I have called "symmetrical nodules." The patient in the second case has a recent cold, and the laryngitis now present has developed since I saw her last. When I first saw her the larynx showed singers' nodules, but the nodules were red and inflamed. The last case has altered recently too, but there was no doubt about the presence of ulceration. I have treated it by removing the granulation tissue and applying tincture of iodine, and the ulceration is now healing. One occasionally sees such a case as this where there is no evidence of either tuberculosis or syphilis and which does not respond to treatment. She has been attending since 1916, and is only just beginning to improve.

Notes on a Case of a Tumour of the Nasopharynx.

By W. STUART-LOW, F.R.C.S.

A MAN who for two years has suffered from nasopharyngeal obstruction. In the nasopharynx there is a large, red, firm, and pendulous swelling which has a broad base and is attached to the roof of the cavity. It is very firm to the touch, and has occasionally given rise to hæmorrhagic discharge, although not profusely. It is proposed to remove this tumour, and I shall be obliged if members will give their opinion of their experience of the best method of doing so.

DISCUSSION.

Mr. TILLEY: My impression of this case, after a cursory examination, is that it is a choanal polypus, with a congested lower border seen below the edge of the soft palate. By watching the tumour while the patient moves his palate, one sees the growth move very freely, and I do not think it would do this if it were a fibroma attached to the base of the skull. On looking into the right nasal cavity, one sees what appears to be a polypus. Furthermore, by digital examination I found there was a smooth mass passing into the posterior choana.

The PRESIDENT: If I had seen only the growth in the nasopharynx I should have hesitated to offer an opinion, having been deceived several times with such. But the polypoid condition in the right nostril indicated the nature of the growth. There seems to be no doubt about its being a naso-antral polypus. A good specimen can be obtained by using a bent snare, passing the loop up behind the soft palate, gripping the growth and tearing it out, when the naso-pharyngeal and nasal portions together with a cyst from the antrum will come away *en masse*. Smaller naso-antral polypi are easiest removed by Lange's hook.

Mr. W. STUART-LOW (in reply): I have not seen the case for two months, and when I last saw it, it seemed to have the characters which I have described here. It is a very unusual case. The whole thing is a mass of redness, and it is different from any choanal polypus I have seen or felt.

Shrapnel Embedded in the Neck.

By W. Stuart-Low, F.R.C.S.

A soldier wounded in France four months ago. Subsequent treatment by partial closure of the glottis and trachea, which necessitated tracheotomy and a re-insertion of this tube some weeks after its removal and still demands its retention. There are still small pieces of shrapnel lying in the tissues, embedded deeply beside and behind the larynx and trachea, as seen in the skiagram shown, and I am anxious for an expression of opinion from those having practical experience of such cases, as to whether or not I should cut down and remove these fragments.

The man complains of occasional pains in the neck.

Case of Double Lachrymal Stenosis; Right Side successfully treated by Internal Dacryocystotomy (West's operation).

By James Donelan, M.B.

The patient, a boy, aged 11, had suffered from epiphora for several years owing to lachrymal stenosis. For this two external operations, of which the history is unknown, had been tried over two years ago. The result was apparently not good, as the skin over the lachrymal sac on both sides was approximately in the same condition as may still be seen on the left, when he was sent to my department at the Italian Hospital by our oculist, Mr. Stanford Morton, with the suggestion that I should attempt the internal operation. This was done on November 8, 1917, the details of West's operation being carried out as nearly as possible according to the description in Sir StClair Thomson's book. The result appears to be so satisfactory that it is now intended to attempt the other side in the hope that if a lachrymal sac is still present, which there is some reason to doubt, a similar result may be obtained. The great difficulty of the procedure in so small a space was in making sure that one had got hold of the lachrymal sac before using the scissors.

Sections from the portion of lachrymal sac removed in this case by Dr. Rodriguez, Resident Medical Officer, Italian Hospital, are exhibited.

Laryngeal Case for Diagnosis.

By Andrew Wylie, M.D.

I saw this case once before. Besides the swelling in the inter-arytænoid space, several members pointed out to me a little growth on the anterior end of the left vocal cord. I therefore intend, in the first place, to remove that growth, and examine it for tubercular or malignant disease.

Case of Thyrofissure and Removal of Right Cord six years after Operation.[1]

By G. C. Cathcart, M.D.

Mr. W. L. was sent to me in September, 1911, by Dr. Beach, of Hurstpierpoint, with the following history. He was aged 63, and complained of fatigue and hoarseness of the voice. He had had this for three years on and off and said it first came on after a long and tiring motor drive; for the last six weeks it had been continuous. On examination there was a small pedunculated tumour growing from the junction of the anterior and middle two-thirds of the right vocal cord. I removed this with Mackenzie's forceps and sent it for microscopic examination; the report was as follows: "This small pedunculated tumour is a papilloma composed of thickened masses of squamous epithelium in a stroma of areolar tissue. The surface of the tumour is necrotic, and there is some inflammatory reaction at the base. There is no evidence of any tendency to infiltrate the pedicle, which is quite free from growth. We see no reason to regard it as malignant, but the case should be carefully watched."

His voice got much better for a week or two, but in November he complained of more difficulty in speaking. On examination the papilloma seemed to be growing again, and there was a little depression on the cord, as if it were being infiltrated. Dr. Greville MacDonald and

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[1] Reported at a meeting of the Section held December 7, 1917 (patient not present).

Mr. Charters Symonds saw it with me in consultation, and we came to the conclusion that it was malignant. On December 25 I performed a thyrofissure, after doing a low tracheotomy. On opening the larynx, the growth, as generally happens, was found to be much more extensive than when examined by the laryngoscope. The whole of the cord and arytænoid cartilage was removed, the wound was closed up and the tracheotomy tube removed after a week. The patient did very well and left the home in three weeks.

The report on the growth removed was as follows: "The tumour is a squamous-celled carcinoma; the section shows the tumour mass invading the submucous structures, but there is no erosion of cartilage; the limits of the specimen are free from growth."

The peculiarity about the case was that six months before he came to me he had seen two other throat specialists who said he was only suffering from laryngitis, so that the papilloma must have grown very quickly. The patient has had lessons in voice production, and his voice is now very fair. Two years ago he felt so well that he married again.

Section of Laryngology.

President—Dr. A. Brown Kelly.

Pedunculated Carcinoma of Pharynx (Specimen and Slide).[1]

By A. Brown Kelly, M.D. (President).

The patient, a man aged 40, was found by chance to have a considerable mass attached by a pedicle to the upper part of the right tonsil. He was unaware of the presence of the growth and had experienced no difficulty in speaking or eating. The tumour was as large as a walnut, the surface smooth, the colour that of the adjacent normal mucous membrane, and the consistence hard. It was removed by snaring. Subsequent examinations—the last, nine months after operation—revealed nothing abnormal. The patient died from an accident about seven years later without having had any further trouble from his throat.

Dr. John Anderson, pathologist, reports: "The growth is pedunculated and covered with mucous membrane showing no evidence of ulceration. On section, a denser area and another of looser fibrous appearance are seen. Microscopic examination shows a covering of stratified epithelium externally, with areas of round-celled increase beneath. Normal glandular tissue and congested vessels are situated more deeply. Beyond these and separated from them by fibro-muscular tissue is the principal feature of the specimen—namely, a fibrous stroma with cancer areas of varying size scattered through it. The arrangement and appearance of the cells are those of an epithelioma derived from the prickle Malpighian layers: a few cell nests are present. Beyond the cancerous zone is a large area of poorly cellular fibrous tissue."

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[1] At a meeting of the Section, held March 1, 1918.

The unusual features in this case of pharyngeal cancer are the pedunculated form of the tumour, its intact surface, the healthy state of the neighbouring mucous membrane, the absence of glandular involvement, and the non-recurrence after operation. The malignant elements were present in the substance of the growth and the mucous membrane showed no epithelial indippings.

Was this tumour originally a simple one, perhaps a fibroma, with a malignant nest beneath the mucous membrane?

DISCUSSION.

Mr. FRANK A. ROSE: I think the growth is an endothelioma, and closely allied to, or identical with, a parotid tumour. Neoplasms similar to parotid tumours grow near the tonsil and in the palate: this is an example.

Mr. TILLEY: I have recorded a case comparable, but not identical.[1] The patient, a boy, had a sarcoma; being pedunculated it was regarded as non-malignant. The growth recurred, the tonsil swelled, and glands appeared in the neck. When the pedunculated growth was microscoped we were told it was probably malignant. Later developments proved this to be so.

Mr. NORMAN PATTERSON: This case resembles one operated on fifteen years ago. The patient twelve years after reported with a pedunculated tumour growing from the left side of the nasopharynx; it was diagnosed as a fibroma. I removed the tumour, and the pathologist stated it to be an epithelioma. Rapid recurrence took place without ulceration. Mr. Lack split the palate and applied diathermy. It recurred. Radium and X-rays were applied, the latter proving more beneficial. The pathologist pronounced it to be an endothelioma.

Dr. D. R. PATERSON: I saw a case of a pedunculated mass of lymphoid tissue projecting from one tonsil. When first seen it was inflamed, but became gangrenous, and was definitely lymphoid. It must have always existed in the pharynx, though unnoticed. It was the size of a walnut, and close to the stalk where it was snipped off was normal tissue. The case shown may have been a lymphoid nodule which later developed malignant disease.

The PRESIDENT (in reply): The following points are against the growth being an endothelioma, (1) the cells are darker, as the chromatin present is relatively more and the protoplasm correspondingly less; (2) the blood spaces are not sufficiently distinctive; (3) epithelial cell nests exist, though few, and the arrangement of the cells around the nests; (4) some of the tumour cells are of the prickle type. I know of only two or three cases of a similar kind, in which the growth sprang from the lateral wall of the pharynx or the epiglottis.

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[1] *Trans. Laryng. Soc.*, 1902-3, x, p. 140.

Photographs of Two Children with Congenital Appendages (Teratoid Tumours) of the Nasal Septum.

By A. Brown Kelly, M.D.

Case I.—Female infant, aged 2 months, presented an outgrowth filling the right nostril and projecting ¼ in. beyond it. The growth was of fleshy consistence and covered with skin which was continuous with that of the columella and upper lip. It measured antero-posteriorly 16 mm. and transversely 15 mm. Rhinoscopic examination revealed a rounded band extending a short distance upwards beneath the mucous membrane on the right side of the septum. In the vestibule the band widened out and passed into columella and half way down the upper lip. The growth was removed but inadvertently destroyed before a histological examination was made. The nostrils are now asymmetrical, the right measuring 18 mm. antero-posteriorly, the left 12 mm. The child presents no other malformation.

Case II.—Male infant, aged 2 months, presented considerable broadening and flattening of the lower part of the nose and a vertical median furrow at the tip. The columella was very broad and from the upper part of its right edge a rounded process covered with skin projected and occupied the anterior angle of the vestibule, while from its base outside the nostril a smaller one projected upwards and outwards.

Specimens of Naso-antral Polypi with Antral Attachment.

By A. Brown Kelly, M.D.

These specimens were obtained by opening the antrum, loosening the cystic lining membrane from the walls of the cavity and drawing the growth down behind the palate. One may recognize the large choanal portion, the small nasal portion, and, coming off at right angles from the place of union of these two, the antral cyst constricted at its base where it was encircled by the edge of the accessory opening. The above operation is not recommended as a routine procedure.

DISCUSSION.

Mr. Tilley: Some growths removed from the post-nasal space do not recur, while others do. We should give the patient the chance of avoiding a major operation by removing the polypus at least once or twice by way of the nose or of the post-nasal space. This morning I saw a patient from whom I removed a large naso-antral polypus early last October. She came with some indefinite sensation interpreted as a recurrence of the tumour. This proved not to be so.

Dr. W. HILL: Yesterday I operated upon a case. The polypus had become enormous since removal eighteen months ago. As it was a recurrence, perhaps I ought to have opened the antrum.

Mr. MARK HOVELL: Many of these cases do not recur.

Dr. IRWIN MOORE: These cases confirm that the chief origin of choanal polypi is the maxillary antrum. Professor Kubo (Japan) reports that out of thirty-six cases, twenty-seven originated in the maxillary antrum, three in the sphenoidal sinus, and six were of uncertain origin. The three sphenoidal cases were reported by Moure (Bordeaux) and Massei. I have added a fourth case of spheno-choanal polypi.[1] Kubo suggests naming these according to their origin and extension. Dr. Syme holds that an accessory ostium exists in only a few cases; the polypus enlarges and finds its way through the normal ostium. The specimens exhibited to-day show that the lining membranes of the antra surrounding the pedicles were degenerated, and cysts were present. In the cases investigated by Kubo 73 per cent. of the antra contained cysts and polypi. Many of these growths recur after removal. Kubo, Syme and others advise radical removal of the growth and its surrounding unhealthy tissue.

Dr. JOBSON HORNE: I reported a case of a large antro-choanal polypus I removed from the post-nasal space without opening the antrum. Some urged the opening of the antrum as a routine practice.

The PRESIDENT (in reply): When you get a single large polypus in the posterior part of one nasal fossa or hanging into the nasopharynx and there is a copious watery discharge on snaring, you may conclude it is from the antrum. In all the cases I have examined, an accessory ostium has been present. I have referred to the association of accessory ostia and cysts in the antrum. These polypi often develop from cysts. A cyst fills the antrum, a portion prolapses through the large ostium accessorium, and increases quickly from œdema.

Intrinsic Cancer of the Larynx; Laryngo-fissure and Semi-demi-laryngectomy, one year after Operation.

By Sir StClair Thomson, M.D.

THE patient was aged 49 when he consulted me in March, 1917. His voice had been failing for a year and a half, and had been quite hoarse for fourteen months. The right vocal cord, with the exception of the vocal process, was replaced by an irregular, rough, white, slightly cauliflower infiltration. It extended right up to the anterior commissure, and there was a subglottic extension. The cord was almost immobile.

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[1] *Proc. Roy. Soc. Med.*, 1917, x (Sect. Laryng.), p. 65.

Laryngo-fissure on March 9, 1917. No preliminary administration of alkaloids. General anæsthesia by chloroform. Duration of operation one and a half hours. On raising the soft tissue, with the perichondrium, from the inner surface of the right thyroid ala, it was found that the thyroid cartilage was so soft and eroded that the detacher slipped through it and was felt under the outer perichondrium. The right thyroid ala was therefore removed.

Sections of the growth show a typical squamous epithelioma penetrating deeply into the small muscles. The removed thyroid ala was also examined histologically, and the pathologist reports: "No actual cancer cells are to be seen in any of the cartilage sections, and it appears to be eroded in advance of the actual extension of the growth."

Since August last the patient has been in active Government service. He can bicycle 53 miles a day, feels in good general condition, and has a better voice than he had just before operation. There is a new cicatricial right cord and a very roomy glottis.

Extrinsic Cancer of the Larynx, four years after Operation by Mr. Wilfred Trotter through the side of the Neck.

By Sir StClair Thomson, M.D.

G. H., now aged 59, was shown before the Section on February 2, 1917.[1] He had a malignant growth of the left aryepiglottic fold. The glands on the left side of the neck were removed by Mr. Trotter on March 31, 1914, and the growth, together with the left ala of the thyroid cartilage, was removed on June 19, 1914.

The patient is only shown again to demonstrate how well he keeps after three and a half years, in spite of chronic bronchitis and abundant tobacco.

DISCUSSION.

Dr. Dan McKenzie: Recently I had a case of post-cricoid cancer which came almost into view. A tube of radium was inserted, and it did harm.

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[1] *Proceedings*, 1917, x, p. 53.

Mr. E. D. D. DAVIS: In Mr. Trotter's operation access to the pyriform fossa is good, the whole of the ala of the thyroid cartilage is easily exposed, and removed by division near the mid-line. It is an excellent operation for the excision of the growth in early cases.

Mr. G. W. DAWSON: These growths when pronounced to be extrinsic are left alone, but the two cases described show that more should be attempted, and one would like to know more about Mr. Trotter's operation, and when the condition in the pyriform fossa negatives operation.

Mr. LAMBERT LACK: For these cases of extrinsic disease diathermy is better than any cutting method. It is easier to get at the growth and to remove it thoroughly.

Dr. W. HILL: I treated a case by diathermy three years ago. The growth is pharyngeal, not laryngeal. Unless it be very superficial the pyriform fossa is involved, and there must be removal of half the larynx. At times there is a good deal of swelling in the vestibule of the larynx, and that may need tracheotomy. My small experience leads me to support diathermy. Radium acts capriciously. Radium should never be given to a patient who is going downhill.

Mr. W. STUART-LOW: My experience has been against radium in favour of diathermy, especially in early cases. The patient can be anæsthetized orally, and with a diathermy knife there is no bleeding. In some of my cases there was no recurrence for months, and patients can take food better than after other treatment.

Mr. E. D. D. DAVIS: Have any members treated a post-cricoid epithelioma by diathermy in late cases when the upper edge of the ulcer is seen in the pharynx and behind the arytænoids? I have had a number of these cases of post-cricoid growth; after excision of the growth follows tracheotomy or gastrostomy. They finally die of broncho-pneumonia.

Mr. LAMBERT LACK: My experience with diathermy has been bad in post-cricoid cases. I have done three, but I shall not try it again, for it is impossible to eradicate the growths by this method.

Mr. DOUGLAS HARMER: At St. Bartholomew's we have a special electrode about the size of a penholder, with a strip of metal exposed on one side only. This, before the current is turned on, is passed behind the cricoid. Afterwards, by rotating the instrument first to one side and then to the other, the growth can be slowly destroyed, and a free passage made for a large bougie to pass without resistance. Some of my patients swallowed better for a time; the relief was transient. Extensive burning causes œdema, and is followed by septic perichondritis, which does not recover. Although diathermy is a first-class treatment for tumours of the pharynx, especially those that can be completely enucleated, it must be employed with the greatest caution near the cartilages of the larynx.

Sir **StClair Thomson** (in reply): In the case of intrinsic cancer, I had to remove the ala of the thyroid cartilage as it was destroyed. I left a little ledge of it at the back. The case of extrinsic cancer is Mr. Trotter's, and I brought it again so that he might have it discussed. Among selected cases it is an admirable operation. Recently I passed on to Mr. Trotter a case which had been refused operation, both in Glasgow and Edinburgh, as inoperable. The result is yet to be seen, but the patient has at present a very good voice, can swallow well, and has returned to his home.

Foreign Body impacted in the Trachea (part of the shell of a Brazil nut).

By F. A. Rose, F.R.C.S.

A male, aged 37, was submitted to tracheotomy on account of urgent dyspnœa. The foreign body exhibited was found impacted in the upper part of the trachea. An attempt to remove it through the larynx failed. It could not be moved although the forceps gripped it. It was then broken into two pieces and extracted through the tracheotomy wound.

Attention is directed to the large size of the fragment (measuring over an inch in length) which became firmly impacted in the trachea after it had passed through the larynx.

DISCUSSION.

Dr. **Irwin Moore**: The tracheotomy was needed in this case on account of the urgent dyspnœa, also because this aspirated foreign body was unusually large, sharp, and irregular and impacted below the larynx. Any attempt at removal without tracheotomy might have caused serious damage to the larynx.

Dr. **H. J. Banks Davis**: If an attempt had been first made to fracture the nutshell, a piece might have dropped lower down.

Mr. **Frank A. Rose** (in reply): The patient inhaled the nutshell when dining. It is a little over 1 in. in length, $\frac{3}{8}$ in. in breadth, and $\frac{1}{4}$ in. thick.

Extreme Alar Collapse.

By William Hill, M.D.

Female, with saddle nose (of luetic origin), together with potential occlusion of nasal vestibule, the anterior nares appearing as L-shaped slits (symmetrical). The left limen is also narrowed by a falciform cicatricial band above. Injection of wax temporarily relieved the alar collapse, but there has been a relapse. Advice will be welcomed.

DISCUSSION.

Dr. DUNDAS GRANT: A simple and sometimes effective operation in alar collapse employed when the nose is very narrow is to detach the ala, cutting away a semilunar piece of skin external to it, and stitching the ala at the outer margin of the raw surface.

Mr. NORMAN PATTERSON: This case can be benefited by inserting a thin layer of cartilage.

Mr. O'MALLEY: This case is the worst type, because the alar cartilage has kinked inwards, and if we attempted to put in cartilage to act as a prop, there would be nothing to cover it, the cartilage being thin and poor. When the alar cartilage was perpendicular, I have inserted a piece of cartilage and widened the nostril to give it a wider base and excellent respiration.

Dr. D. R. PATERSON: In similar cases I have removed a wedge-shaped piece from the alar cartilage, the vestibular side, with very good results. In syphilitic subjects one can never be certain after operating that there will not be subsequent retraction, even when the syphilis appears obsolescent.

Dr. W. HILL (in reply): As there is a good deal of collapse of the columella, to stiffen the columella as suggested is desirable, but I appreciate the danger of operating on a columella in a syphilitic subject. It is better to use a piece of cartilage from someone else. After I had put in wax on the vestibular side the alar collapse disappeared, but the wax has now disappeared. As a general rule, when one puts wax into the nose it remains, and the case is pronounced cured. A month ago a boy came to me to have wax put into his nose. I had used wax eight years ago on him. He has now got very little wax left there.

Carcinoma of Fauces treated by per-Oral Excision, followed by Diathermy.

By WILLIAM HILL, M.D., and NORMAN PATTERSON, F.R.C.S.

PATIENT, aged 73. The operation was carried out under pharyngeal suspension. The external carotid was first ligated. The case is shown to raise the question of the advisability of this as a routine measure in a case where the pillars and tonsil are involved, and where there is also slight extension to the base of the tongue.

DISCUSSION.

Dr. W. Hill: It was a question if we should ligature the carotid. Mr. Patterson had a death from secondary hæmorrhage in such a case, and others have occurred. Ligature of the carotid starves the growth, and there is less likelihood of reactionary hæmorrhage.

Mr. Norman Patterson: I know of five cases which have died from hæmorrhage. Preliminary ligature of the external carotid, and ascending pharyngeal artery may be considered under the following conditions: (1) Cases where the blood-pressure is high. (2) When the arteries are thickened and degenerated. (3) Extensive growths involving the neighbourhood of the tonsils, soft palate and pillars, or the base of the tongue and epiglottis. (4) When removal of glands and treatment of the primary growth are undertaken at the same time. The advantages are: (1) Little or no hæmorrhage at the time of operation. (2) No hæmorrhage when the sloughs separate. (3) More extensive sloughing, and therefore more thorough destruction of the tumour. (4) When time is an important consideration possibility of rapid excision followed by diathermy.

Dr. Irwin Moore: I show water-colour drawings of a patient aged 75, suffering from epithelioma of the tonsil and tongue. The case was treated by diathermy after ligature of the external carotid. A large cavity is seen after the removal of the growth, and the drawing made one month later shows healthy tissue without cicatrices.

Mr. E. D. D. Davis: I have seen two cases of fatal secondary hæmorrhage from the internal carotid artery following excision of a large growth in the faucial region. In both cases there was an erosion of the internal carotid artery. Tying the common carotid may cause hemiplegia. I had one case of malignant glands in the neck in which the common carotid was tied, and the patient died with hemiplegia. The severe hæmorrhage in such cases is from the internal carotid. Ligature of the external carotid in cases of removal of the upper jaw makes no difference to the hæmorrhage.

Dr. James Donelan: The common carotid should be tied. Some years ago I enucleated the left tonsil in a man suffering from unsuspected Banti's disease. Pressure with Spencer Wells forceps controlled the bleeding until the forceps was removed. It was not possible to ligature the bleeding vessel so the common carotid was tied; the bleeding stopped.

Dr. D. R. Paterson: In all extensive operations about the tonsils the external carotid should be tied. Anyone who has tried both ways will not hesitate. In cases of large tumours in the antrum, in which much hæmorrhage is likely, I frequently ligature that vessel. This procedure gives a clean field, and removes fear of secondary hæmorrhage. I advocate it in extensive diathermy operations.

Mr. W. D. **Harmer**: Preliminary ligature of the external carotid must be considered in every case of an extensive operation in the pharynx, in the region of the tonsil or lower down. Tying either the common or the internal carotid is useless because if the growth involves either of these vessels it is inoperable. Operations on small growths in the pharynx do not require preliminary ligature of the external carotid artery. Pressure can be applied and if necessary the vessel ligatured should hæmorrhage occur.

Dr. H. J. **Banks-Davis**: Unless anæsthetized the patient will not allow pressure from the inside of the mouth.

Mr. **Norman Patterson** (in reply): Some of the cases of hæmorrhage are very troublesome. The last case I was unable to deal with (he was blue in the face, bleeding severely, and would not keep still) until I did a preliminary laryngotomy. One can never tell beforehand what cases will bleed. I agree that ligature should not be done in cases of small growths.

Case of Papilloma of the Nose.

By G. W. Dawson, F.R.C.S.I.

Man, aged 36, came under my care last November with a history of bleeding from the nose every two or three days for the previous six months. A mulberry dark red mass was seen anteriorly and posteriorly, filling up the left posterior part of the nose. It was attached to the septum close to its posterior margin, and was firm to the touch. The growth was removed under a general anæsthetic, an attempt being made to strip off the mucous membrane from which it grew. The bleeding was free and the bulk of the tumour much larger than one expected, being sufficient to fill a wine-glass.

Pathological report: "Section shows structure of papilloma. No evidence of malignancy."

DISCUSSION.

The **President**: Nasal papillomata are apt to be confused with papillary hypertrophies unless microscoped. To the naked eye the specimen is a true papilloma.

Mr. **Dawson** replied.

Perichondritis of the Larynx.

By G. W. Dawson, F.R.C.S.I.

MAN, aged 65, first seen last June, suffering from a large foul ulcer affecting the right side of pharynx and the outer surface of arytænoid region. It was at first thought to be malignant, but the ulcer rapidly improved on 20-gr. doses of pot. iod. At the end of August the swelling was noticed externally on the right side of the neck and under the chin, causing some difficulty of breathing. The right side of larynx and arytænoid cartilage had become very swollen and œdematous looking. With a guarded McKenzie's lancet I punctured the arytænoid and inner surface of larynx, giving exit to a few drops of pus. On October 2 the breathing became very difficult and I was obliged to open his trachea in a hurry. He stopped breathing before the airway was opened, but revived under artificial respiration. Pus appeared and kept discharging from the upper portion of the tracheotomy wound, and a probe passed into a track upwards and outwards 2 in. The swelling on the right side of neck increased into a hard brawny mass.

On February 1 last swallowing became difficult and the neck became more swollen. He preferred to allow the abscess to open naturally, which it did in about a week. The swallowing is still difficult.

DISCUSSION.

Sir STCLAIR THOMSON: These prolonged cases of perichondritis of the larynx are inevitably fatal. One or two of my cases were evidently started by a tracheotomy done too high and the larynx became infected, and even after a low tracheotomy was done and the other opening closed the result was bad. One case, a man aged under 40, seemed to be going downhill, but we ultimately found the disease was subglottic and malignant. We do not know whether Mr. Dawson's case is malignant disease, or whether it is syphilis, or the two combined: I believe nothing will stop it. Dr. Segura, of the Argentine, told me such cases were common there, owing to tracheotomies being done too high up, and that to stop the perichondritis they excised the larynx—a rather drastic remedy.

Dr. JOBSON HORNE: In one of two similar cases I have had, a man died suddenly from gumma rupturing into his larynx. In the second the patient died from malignant endocarditis.

Mr. Dawson (in reply): When I did the tracheotomy I found the cartilage much ossified, so perhaps a better name for the case would have been periostitis. When seen, he had a large foul ulcer at the base of the tongue, extending to the arytænoid and the pyriform fossa. He improved rapidly, the ulcer healing under iodide: it was extrinsic to the larynx. As it was about to heal, he got this swelling limited to half the larynx. There is inflammatory thickening, and an abscess forms occasionally.

Tumour of Pharynx.

By G. W. Dawson, F.R.C.S.I.

Woman, aged 38. Pain and difficulty in swallowing for several years; worse for the last three weeks. There is a well-defined tumour apparently attached to the posterior surface of the pharynx on a level with the arytænoids, the right one of which it overhangs and obscures. The tumour is about the size of a split tangerine orange and soft to the touch.

Tumour of the Palate in a Child, aged 8 months, for Diagnosis.

By James Donelan, Ch.M., M.B.

Suggestions as to treatment desired. The swelling causes some interference with suckling. This appears to be the youngest case reported of torus palatinus, if that diagnosis is accepted.

Dr. Donelan also exhibited a specimen of negro skull with large torus.

DISCUSSION.

Mr. Tilley: I thought the swelling was rather soft mucous membrane covering a very hard torus palatinus.

Dr. Donelan: I thought it was a torus palatinus. It occurs in the fœtus. Korner, in the *Archiv für Laryngologie*, brings the literature up to 1912. He groups the ages of children under 10 years together showing that 7·5 per cent. occur in boys and 8 per cent. in girls. The skull, that of a negress, lent by Professor Thane, shows a torus extending the entire length of the interpalatine and intermaxillary sutures. This is rare in negroes but common amongst Peruvians and Eskimos. I believe it starts most commonly at the intersection of the two sutures. It may remain there as in most of the sketches in Sir Rickman Godlee's paper,[1] or it may extend along the intermaxillary suture, or in a double form representing the hypertrophied edges of that fissure.

The President: This appears to be a tumour and not the torus palatinus. I have seen the latter in a baby. The ages given in the statistics are probably those at which the torus was noticed, not of its origin.

Case for Diagnosis.

By James Donelan, M.B.

Woman, aged 55, with chronic recurrent aphonia and subglottic thickening. Has been hoarse since beginning of December. There is a slight roughening of edge of the anterior fourth of right cord.

Repair of Nose by Paraffin Injection.

By James Donelan, M.B.

Woman, aged 22, shown here about five years ago. Saddle-back nose from congenital syphilis. Deformity removed in great measure by paraffin injection. The patient's breathing space has been greatly improved incidentally, and she is quite pleased with the "creation."

DISCUSSION.

Mr. O'Malley: The condition would be improved by putting a small piece of cartilage in the columella.

Mr. W. Stuart-Low: It is a very good result. It has stood well for five years, and the patient's appearance is greatly improved.

Dr. D. R. Paterson: In a former discussion a member of the Section related cases in which paraffin had travelled from the nose to the cheek, producing a ridge, and from the mammary region to the groin.

Dr. Dan McKenzie: I have tried to dissect wandering paraffin from a man's nose, injected ten years before. The paraffin was broken up into tiny globules, and disseminated among the tissues. I could only remove small pieces of tissue containing the globules, but improved his appearance.

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[1] *Proceedings*, 1909, ii (Sect. Surg.), p. 175.

Mr. WYLIE: I had a case in which the patient went on board ship as a stoker after having had paraffin injected. The paraffin melted and was found in the cheek.

Mr. W. STUART-LOW: Dr. Grant will remember a similar case of a stoker on board ship. At first his appearance was very much improved, but later he returned looking much worse.

Mr. TILLEY: When this subject was first mooted and paraffin was being used, the late Sir Henry Butlin said the only experience he had had of paraffin injections was in dissecting them out. I have had similar experience to Dr. McKenzie's, spending two hours dissecting out paraffin from a lady's face which had been injected by a Bond Street "beauty specialist." I think the use of strips of septal cartilage will frequently take the place of paraffin.

Dr. IRWIN MOORE: I have had considerable experience of injecting paraffin wax into the nose, but have never had any trouble from its wandering. I use solid wax—a preparation from Paris—and inject it with a powerful syringe.

Dr. DONELAN (in reply): In one of my five or six cases injected over ten years ago the nose retains a perfect shape though previously it was quite flat with only the bridge and tip slightly projecting. Another, done between five and six years ago, has remained unchanged. I use paraffin with a melting point of 108° F. It should not be fluid but just soft enough to move through the special warmed syringe. The injection is made from inside, and I support the remains of the septum afterwards with Adams's septal forceps, covering the puncture and retaining the paraffin while the nose is being moulded. An assistant grips the root of the nose and prevents any escape towards the forehead. It sets in about half a minute; an ice compress is applied the moment the desired form has been attained. Much depends upon the quality and melting point of the paraffin. In all the cases in which I have used it it was chosen on account of syphilis and it seems a useful method when other surgical measures are inapplicable.

Patient from whom a large Malignant Growth has been Removed from Right Antrum.

By HERBERT TILLEY, F.R.C.S.

THE case is shown to illustrate the advantage of approaching the field of operation by way of the canine fossa, including the removal of the ascending process of the maxillary bone, and thus avoiding the scar incident to lateral rhinotomy. It is an extension of Denker's operation, and affords a very complete view of the antrum and ethmoidal regions. General anæsthesia was administered by the "intratracheal ether" method.

DISCUSSION.

Mr. TILLEY: The case emphasizes that in malignant growths of the antrum it may be wiser to choose this mode of access, because you get a wider field, less hæmorrhage, and no deformity.

Mr. W. STUART-LOW: I have operated on a number of cases of malignant disease in this way. You can retract the cheek freely, and remove the whole anterior wall and the ascending process, and the patient being placed higher than the operator, you can get good exposures and control.

Intranasal Frontal Sinus Operation under Nitrous Oxide Anæsthesia.

By DAN MCKENZIE, M.D.

THE operation was performed for the relief of pain from which the patient, a woman, had been suffering for several weeks. The anterior end of the middle turbinal having been removed, the infundibulum was enlarged with Dr. Watson Williams' burrs, the smaller and the medium size only being used. The operation was comfortably completed after a single administration of nitrous oxide. Immediate relief from pain followed, and at the last washing out ten days ago no pus was obtained.

DISCUSSION.

Mr. TILLEY: In many cases the success does not only depend on boring a larger fronto-nasal canal, but also on removing infected anterior ethmoidal cells. Disaster will surely attend any attempts at "brilliancy" in operating in these dangerous regions.

Dr. MCKENZIE (in reply): I should not treat an ordinary frontal sinus case by this method. This girl had an acute frontal sinus, with much pain: I did not start with the intention of entering her frontal sinus, but it was easy to enlarge the opening.

Tonsil Artery Forceps.

Shown by DAN MCKENZIE, M.D.

BLEEDING from the tonsil area should be stopped before the patient leaves the operating table. In some cases, this necessitates the ligature of bleeding points. To pass a ligature over ordinary pressure forceps, even of a large size, is difficult, and to facilitate the task the forceps now shown were devised. They are a modification of another type used for pelvic hæmorrhage.

Dr. Jobson Horne: I keep by me nasal polypus forceps, an old type, very similar to the instrument exhibited, for the same purpose.

A Case of Proptosis of the Left Eye.

By E. D. D. Davis, F.R.C.S.

A woman, aged 60, was sent on November 20, 1917, by Mr. McMullen, for an examination of the nose. The left eye had been prominent and the sight had been lost for twelve months. There was no pain and there were no nasal symptoms. The movements of the eyeball were limited and optic atrophy was present. Inspection of the nose was negative. Transillumination showed an opaque left antrum; X-ray, opacity of left orbit and antrum. Puncture of the left antrum was negative. Wassermann reaction negative.

The cause of the proptosis is obscure. The condition has remained unaltered since November.

Mr. E. D. D. Davis (in reply): I think the cause of the proptosis is in the orbit and not in the antrum. The eyeball is pushed forward and not upwards as in the latter case.

Fixation of both Vocal Cords; Tracheotomy.

By E. D. D. Davis, F.R.C.S.

A widow, aged 44, first attended the hospital on October 9 last. Tracheotomy had been performed by Dr. Basil Hood six weeks previously for laryngeal obstruction. The patient then stated that fourteen months ago she had an attack of influenza followed by loss of voice and difficulty in breathing. On examination, the right vocal cord was fixed in the mid-line and the left cord was obscured by swelling of the left ventricular band. Both arytænoids were immobile. The thyroid alæ were normal. Chest presented no physical signs. X-ray of chest negative. Wassermann reaction positive. Injections of salvarsan and administration of mercury and potassium iodide produced slight improvement.

A Microscopic Section from the Case of a Tumour of the Epiglottis.[1]

By W. Jobson Horne, M.D.

The case, it will be remembered, was one of most unusual occurrence. It will also be remembered that the tumour melted away in the most remarkable manner under the influence of radium. The microscopic section of the tumour is exhibited in the hope of eliciting expressions of opinion about the nature of the case.

Papilloma of the Larynx.

By Andrew Wylie, M.D.

Patient, a male, aged 20, has had hoarseness for eight months. A large papilloma is seen growing from the anterior end of the right vocal cord.

Transplantation of Cartilage into the Septum.

By J. F. O'Malley, F.R.C.S.

The septum was completely resected to free the airway, but the tip of the nose had no support and the ridge was much depressed. Portions of the septal cartilage were then implanted in the tissues of the columella and to restore the ridge.

Columella: A strip $\frac{1}{5}$ in. wide and 1 in. long (this varies with the individual nose) was taken and after making an ample bed for it with a tenotomy knife and blunt dissector, it was inserted and the wound carefully stitched.

Ridge of nose: The tenotomy knife was inserted vertically to a depth of $\frac{1}{5}$ in. and $\frac{1}{4}$ in. from tip of nose and then pushed up to nasal bones or upper limit of depression. It was then withdrawn and pushed about $\frac{1}{8}$ in. towards tip of nose. An ample bed was made with blunt dissector. The piece of cartilage was inserted, pushed upwards and then lower end drawn downwards towards nasal tip. This small manœuvre obviates the need for stitching the skin incision and prevents the lower end of the cartillage slipping into the wound. The incisions are then painted over with collodion.

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[1] Shown at the previous meeting.

DISCUSSION.

Mr. TILLEY: One little defect would be remedied by the insertion of a piece of cartilage.

Mr. HARMER: I find that thin pieces of cartilage are often insufficient to repair a badly depressed nasal bridge. In such cases a large piece of rib cartilage is required.

Tertiary Syphilis of the Pharynx, clinically resembling Tuberculosis of a Lupoid Type.

By IRWIN MOORE, M.B.

PATIENT, a female, aged 44, upon whom an urgent trachectomy had been performed in May, 1917, for specific stenosis of the larynx, was shown on June 1, 1917,[1] and on November 2, 1917, to demonstrate the result, in one month, of a single injection of galyl. This cleared up the infiltration of the pharynx,[2] and re-opened the larynx so that the patient breathed freely through the larynx with the tracheotomy tube corked. Patient had been lost sight of until recently, and was found to have dispensed with her tube two weeks ago, and the neck wound had healed.

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[1] In the report of this case (see *Proceedings*, 1917, xi (Sect. Laryng.), p. 4) the date admitted to hospital should read May 23, 1917—not 1916.

[2] *Proc. Roy. Soc. Med.*, 1918, xi (Sect. Laryng.), figs. 1 and 2, p. 47.

Section of Laryngology.

President—Dr. A. Brown Kelly.

Brain from a Patient who presented Nystagmoid Movements in the Pharynx and Larynx.[1]

By A. Brown Kelly, M.D.

The patient, aged 63 when first seen, complained of occipital pain which he attributed to his nose. This was sunken owing to tertiary ulceration. He had taken alcohol freely. Nine months previously he had had a slight shock.

On examining the pharynx nystagmoid movements of (*a*) the posterior wall, (*b*) left tonsillar region, and (*c*) left half of the soft palate were observed. The "short phase" of (*a*) was to the left, and of (*b*) to the right, so that the twitchings seemed to converge to the lateral wall. The movements of (*c*) were barely discernible.

In the larynx similar twitchings were present during quiet respiration, but on phonation were lost sight of, the cords being approximated normally. The nystagmoid movements took place during both inspiration and expiration, and were greatest at the left vocal process and left arytænoid. They were more noticeable during expiration when the vocal cord was swung inwards by a series of four or five to-and-fro jerks, which, as in the pharynx, were unequal and arrhythmical. The right half of the larynx was affected, but to a much less degree. Over 130 to-and-fro movements were executed per minute.

The man was examined occasionally during the next two years, but no change was noted in the appearances above described. Shortly after his last visit to me he was sent to Hawkhead Asylum, where he died two months later from apoplexy. The superintendent, Dr. James H. Macdonald, made a detailed post-mortem examination of the brain, especially of the vessels at its base. A résumé of his conclusions follows.

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[1] At a meeting of the Section, held May 3, 1918.

The post-mortem examination shows that the patient's mental state was due to cerebral softening associated with cerebral arteriosclerosis, and that death was brought about by hæmorrhage into the cerebellum, with pressure on the pons and medulla. Owing to these gross changes which were long subsequent to the onset of the nystagmus of the vocal cords, it is very difficult definitely to relate this rare condition to a particular lesion of the nervous system. The changes mentioned are, however, to be attributed without doubt to disease of the vascular system represented by endarteritis obliterans and thrombosis of the larger cerebral vessels, and fibrosis of the capillaries and small arterioles; these lesions were widespread and essentially syphilitic in character.

It would appear legitimate to conclude that the nystagmus of the vocal cords was a comparatively early external manifestation of disturbed nervous function resulting from interference with the blood supply to the nervous mechanism. The site of this circulatory disturbance was probably in the cerebellum. Capillary fibrosis was particularly marked in that organ, and there, too, the final disturbance took place which terminated in death. Further, it is especially deserving of note, that the circulation through the superior cerebellar arteries must have been deficient for some considerable time. At the points of origin of these vessels from the basilar artery the latter vessel was almost occluded by a thrombus which gave evidence of age in well advanced organization. The thrombus encroached on the lumen of both cerebellar arteries, but more especially on that of the right side. Both vessels were at the same time affected by endarteritis. Deficiency of circulation in these arteries would be compensated to a greater or less extent through the anastomosis which exists between the superior and inferior cerebellar arteries. In point of fact, the inferior cerebellar arteries were noted to be injected and prominent, especially on the right side, whilst the branches of the superior cerebellar artery on each side were small and empty in comparison. Of the two inferior cerebellar arteries, the posterior, which is derived from the vertebral artery, was especially well filled with blood. Compensation, established in this manner, which might well have been ample enough to prevent extensive softening of the cerebellum, such as might have betrayed itself by gross signs (vertigo, ataxia, vomiting, &c.), was yet sufficiently incomplete or irregular to give rise to minor functional disturbance.

DISCUSSION.

Mr. TILLEY: One of my clinical assistants recently mentioned that a friend of his from whom the late Sir Victor Horsley removed a cerebellar tumour some years ago, and who is now doing good work in France, had nystagmoid movements of his pharynx since the operation. The patient was not conscious of the movements, which were evident to anyone looking into the throat. At the old Laryngological Society I showed an adult with similar nystagmoid movements of the pharynx, and I think it was Sir Felix Semon who quoted Schlesinger's views on the subject, and suggested that most of these cases develop general paralysis of the insane. At any rate, my patient died four years afterwards of that disease.

Mr. LAMBERT LACK: I have published a paper on this subject reporting two of my own cases and many collected from the literature. The cases were divided into two main groups. Most of them developed, sooner or later, gross cerebral disease, generally cerebellar, of which they died, but in a large number of cases the movements ultimately passed off. The movements in both cases were very similar, and in the latter class they were usually associated with some irritation of the throat, such as pharyngitis sicca. Nearly all the cases were in young people.

The PRESIDENT (in reply): These cases are very rare, and we are glad to have the record of any post-mortem examinations on them. At present I cannot give the consensus of opinion as to how the nystagmus is produced.

Simple Method of Recording Diagrammatically Movements of the Vocal Cords; with special reference to Tremors. (Epidiascope demonstration.)

By A. BROWN KELLY, M.D.

IN order to record diagrammatically tremors and ataxic movements of the vocal cords, a chart is required on which the normal excursions can be registered. For this purpose it is necessary to know the length of excursion of the cords, or how far they are apart in their different phases of activity, also the amount of movement they execute in a given time.

The width of the glottis has been carefully investigated by Sir Felix Semon.[1] He found in the adult male that this on an average amounted to 5 mm. when the cords were in the cavaderic position, and 13·5 mm. when in the position of quiet respiration. Accordingly, a vertical dotted line is drawn to denote the median position, and at distances of 2·5 and 6·75 mm. lines are drawn on each side parallel to it to represent the cadaveric and quiet respiratory positions (fig. 1). The position of deep inspiration is not indicated because it is identical with that of quiet respiration, excepting possibly in a small percentage of individuals to be referred to later; an allowance, however, of 2·5 mm. is made in the chart for special cases, the outside lines are thus 16 mm. apart. Assuming the frequency of respiration to be normal—viz., 18 per minute or 3 in ten seconds, and the ratio of inspiration to expiration as 10 : 12 (Foster), the movements of the cords in relation to time are registered by measuring off distances of 10 mm. and 12 mm. alternately at the side of the chart to represent inspiration and expiration respectively.

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[1] *Proc. Roy. Soc. of Lond.*, 1891, xlviii, p. 403.

A chart based on statements found in the text-books would differ somewhat from that just described. Measurements are not given by their authors, but it is stated that the cadaveric position is mid-way between those of adduction and quiet respiration and that quiet respiration is midway between adduction and deep inspiration. Again, assuming the glottis to be 5 mm. wide when the cords are in the cadaveric position, it will measure 10 mm. and 20 mm., when the cords are in the positions of quiet respiration and deep inspiration respectively (fig. 2).

It is surprising that unanimity does not exist as to so obvious a matter as the state of the cords during quiet respiration. While some authors, especially Semon, state that they are motionless or almost so, others describe the glottis as enlarging and diminishing with each inspiration and expiration. The careful examination of the larynx of a number of tolerant subjects soon convinces one that, as a rule, the vocal cords are at rest or almost so during quiet respiration, and that only exceptionally do they make extensive excursions synchronous with inspiration and expiration. On deep inspiration further abduction does not take place, the distance between the vocal processes remaining unaltered, although the edges of the cords may occasionally be drawn outwards.

A straight or slightly wavy line, coinciding more or less with that of quiet respiration therefore represents the cord during quiet respiration and deep inspiration in the great majority of persons (fig. 3). In the exceptional individuals, on the other hand, in whom the cords swing markedly inwards and outwards, it will be seen (fig. 4) that this pendulum movement does not go on uniformly, but that inspiration begins with sharp abduction followed by a period of rest, after which gradual adduction takes place; further, in these persons deep inspiration may cause the cords to be abducted beyond the position of quiet

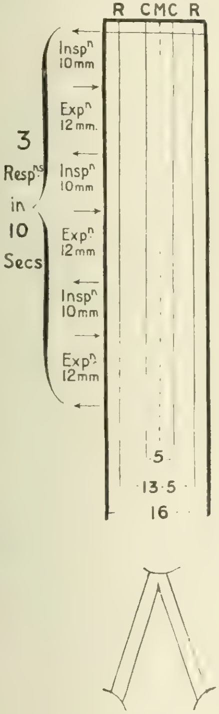

Fig. 1.

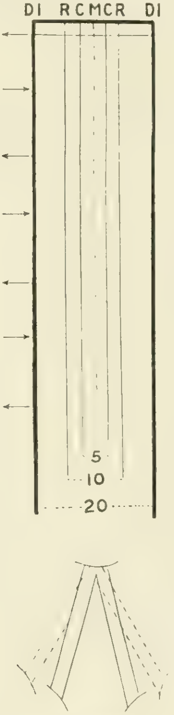

Fig. 2.

respiration, but on this point I cannot speak definitely. Phonation is represented by the immediate meeting of the cord tracings in the middle line, and the emission of voice is indicated by a thick stroke, a whisper by a thin one. A heavy arrow at the side denotes the moment at which the impulse to phonate is given.

From the above data it may be concluded that the chart based on Semon's measurements (figs. 1 and 3) corresponds most nearly to the normal conditions; this chart is therefore used. In order that the tracings of the movements of the cords may be distinct, the lines marking the cadaveric and respiratory positions are omitted, and if the movements of both cords are alike only those of one are represented.

In passing to the consideration of tremors of the cords it has first to be pointed out that these are manifested only when there is movement of the cords during quiet respiration; possibly when the cords are motionless the tremors are masked by the tonicity of the abductors. It must not be assumed, however, that swinging vocal cords are always associated with the diseases of which tremors are a manifestation; on the contrary, the vocal cords were found to be stationary and without tremors in some patients suffering from the same affections. My

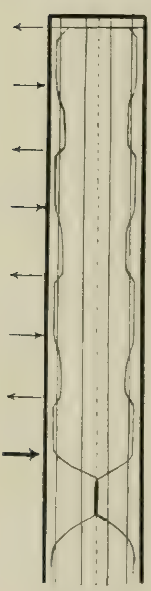

Fig. 3.

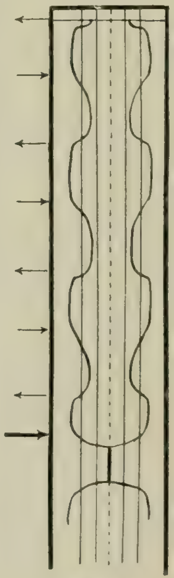

Fig. 4.

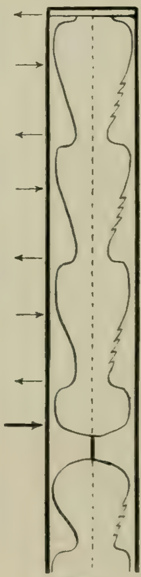

Fig. 5.

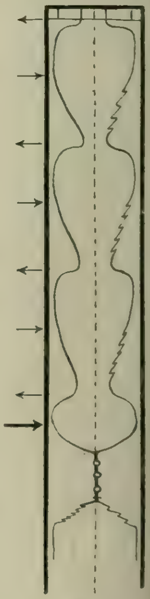

Fig. 6.

impression, however, is that the pendulum type of laryngeal respiration is relatively commoner in persons with central nervous disease than in normal individuals.

Physiological tremors may occasionally be seen in weak or nervous subjects. They are irregular and usually accompany expiration only.

In certain diseases of the central nervous system, especially insular sclerosis, tabes and paralysis agitans, tremors of the cords are not infrequently met with.

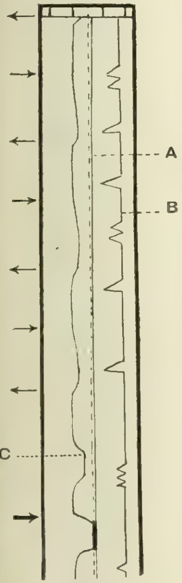

Fig. 7.

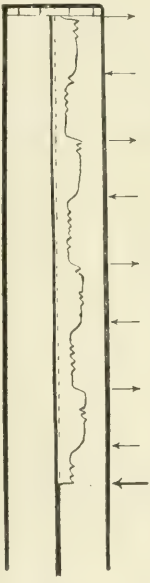

Fig. 8.

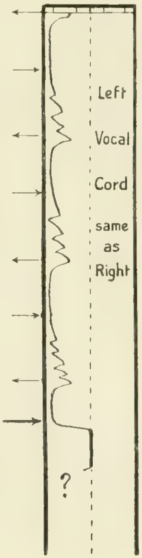

Fig. 9.

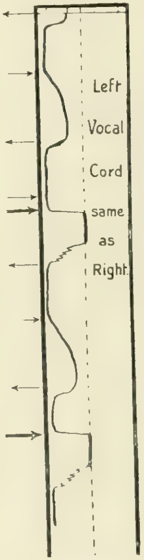

Fig. 10.

In insular sclerosis, in one case marked tremors of the left vocal cord were observed during expiration (fig. 5). In another, the left vocal cord showed tremors during expiration, phonation, and after phonation, while the right showed them only during and after phonation and to a much less degree (fig. 6). In a third patient, the left cord (fig. 7, **A**), was fixed in the middle line, while the right (**C**) swung around the cadaveric position, and marked irregular twitchings of the left arytænoid (**B**), involving the left ventricular band, and to a less degree the left cord, were noted.

In tabes, expiration was interrupted by tremors in several cases. In one patient the right cord was motionless in the middle line, while the left swung around the cadaveric position, inspiration being accompanied by two or three twitches and expiration by a number of fine tremors; occasionally involuntary sharp adduction to the middle line took place (fig. 8).

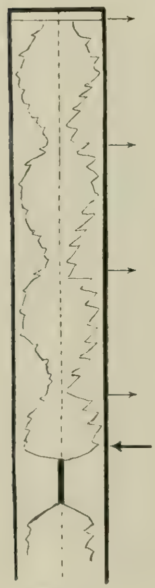

FIG. 11.

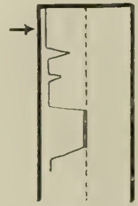

FIG. 12.

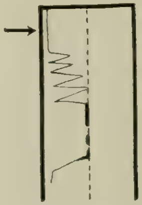

FIG. 13.

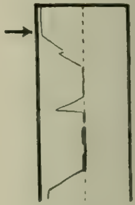

FIG. 14.

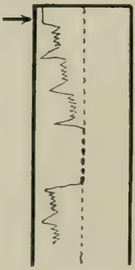

FIG. 15.

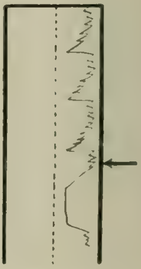

FIG. 16.

In paralysis agitans a detailed examination was impossible in some of the patients. In several, tremors accompanied expiration only (fig. 9); in others they appeared only during the relaxation of the cords after phonation (fig. 10). In all cases both cords were affected alike.

In a case of nystagmus of the vocal cords which was under my care twitchings took place during both inspiration and expiration but were more marked during the latter, when the cords were swung inwards by a series of four or five to-and-fro jerks which were unequal and arrhythmical and more distinct in the left cord (fig. 11).

A noticeable feature in all the cases referred to is that the tremors were most frequently observed during expiration; much less often when the cords were going apart after phonation; and still more rarely during phonation; further, that when present during both inspiration and expiration, as in nystagmus, they were more marked during the latter phase. This predominance during expiration is probably to be accounted for by the fact that expiration is a passive movement and more easily disturbed, while inspiration is usually strong enough to mask tremors. In support of this view it may be mentioned that in a case of insular sclerosis in which expiration was accompanied by tremors, when the patient produced blowing expirations, thus making expiration active and inspiration passive, the tremors were found to be present with the latter only.

The material at my disposal, although seven large hospitals have been laid under contribution, is too limited to allow of generalizations or attempts to associate certain kinds of tremors with certain diseases.

Charts may be constructed in the manner indicated also to represent the action of the vocal cords in shell shock stammering when phonation is attempted. Thus, from the instant when the effort is made to phonate (marked by an arrow) until the voice is produced a considerable interval may elapse during which the cords may be twitched once or twice towards the middle line (fig. 12), or they may swing to and fro and an interrupted voice result (fig. 13), or they may slowly approach the middle line until they meet but at first merely a whisper is produced (fig. 14). Again, the cords may present frequent fine tremors and only after prolonged effort tremulous phonation may result (fig. 15), or the patient may even be unable to approximate the cords completely (fig. 16).

Cases of Epithelioma of Larynx removed by "Window" Resection of the Thyroid Cartilage.

By H. LAMBERT LACK, F.R.C.S.

THE patient, Mr. A. G. W., aged 59, seen in March, 1917, had been hoarse for four months. There was a growth about the centre of the left vocal cord. A piece was removed for diagnosis and reported to be epithelioma. The operation in the London Hospital in the presence of three members of the Section, was performed by the method described in the *Lancet* of November 11, 1916. A transverse incision was made at the level of the cricothyroid membrane, the left ala of the thyroid exposed and a large square piece of it removed. The larynx was then opened and the growth, with the left vocal cord and the surrounding parts, freely removed. The wound was partly sewn up. No tracheotomy was performed. There was no trouble with bleeding. The patient made an uninterrupted recovery and in two months was back at his work as a commercial traveller, having a very fair voice.

The second patient, Mr. H. B. M., aged 72, first seen in June, 1917, complained of loss of voice since November, 1916. There was a large growth in the centre of the right cord, a piece of which being removed for diagnosis, was found to be epithelioma. The operation was performed in the same manner as in the last case on July 6 and the patient made a rapid recovery. In spite of his age he had no bad symptoms, but has since remained well and has a good voice.

These two cases are shown to illustrate the method which I advocated over a year ago and demonstrate, I think, all that was claimed for the operation.

DISCUSSION.

Dr. IRWIN MOORE: Mr. Lack's operation has restored a good voice to the patients though I do not admit it is better than in cases dealt with by thyrofissure. When Mr. Lack drew our attention to one of his cases in 1916 he told us he had carried out this modified procedure in his earlier thyrofissures—but subsequently adopted the "usual method." Dr. Kelson asked him why he had allowed such a long interval to elapse before reviving it. Mr. Lack claims no less than sixteen advantages for his modified operation over thyrofissure: I shall criticize these *seriatim*. The first advantage stated is that a better view and better access are obtained: but we can get all the access that is necessary by thyrofissure performed in the usual way, especially if we use a self-retaining graduated retractor. The second advantage is stated to be that there is no occasion to split the thyrohyoid membrane. I have been associated with my colleagues in over forty thyrofissures during the past twelve years, and have not known a case in which it had been necessary to split that membrane. Mr. Lack says "violently pulling apart the two halves of the larynx in thyrofissure causes discomfort and difficulty in swallowing:" but there is never any force used and no need to draw the thyroid alæ violently apart. The only occasion which I know of where difficulty in swallowing occurred was in a case where an extensive growth involved the arytænoid cartilage and necessitated removal of its greater portion. Mr. Lack says his modified operation is more rapid: but on a previous occasion when I saw him operate the incision across the neck was attended with so much bleeding, that it took considerable time to check it by compression forceps. All operators agree that there is no necessity for haste, and success in thyrofissure depends on a slow, careful and deliberate operation. Mr. Lack maintains that it is easier to control hæmorrhage by his method, but thyrofissure as now performed is practically a bloodless operation. Endolaryngeal hæmorrhage seldom occurs, but if there is a bleeding point it is generally on the outer side of the arytænoid after the growth is separated, and bleeding can be stopped by gauze or pressure forceps. The late Sir Henry Butlin stated that he had not known a case in which hæmorrhage gave him any anxiety. Mr. Lack says there is the advantage of a more thorough removal of the underlying cartilage. Why does he depart from the old-established rule of freely removing the whole of the growth without unnecessarily sacrificing healthy tissue? Butlin pointed out that the periosteum acts as such a strong resisting barrier between the growth and the cartilage that very rarely was the cartilage infiltrated, and if it were it sufficed to take a thin slice off the inner surface or to remove the portion of cartilage affected. Mr. Lack states that there is less trouble with the anæsthetic, and less danger of blood entering the lungs, I am sure any anæsthetist would prefer to give an anæsthetic for a laryngofissure with a tracheotomy tube in position than in an operation carried out by Mr. Lack's method; I have spoken to several anæsthetists on this question and they agree. In thyrofissure as usually carried out, if preceded by temporary tracheotomy, the risk of blood entering the lungs is avoided. Again, Mr. Lack says, if packing is necessary, it is easier to introduce and remove: but this cannot be admitted. The risk of cell-transplantation is said by Mr. Lack to be less by his method than in the usual thyrofissure. Now, Mr. Lack has published an important paper on the subject of cancer re-infection, yet in his method of performing the operation the serious and unnecessary risk of cell-transplantation is absolutely ignored. He suggests two modifications of the operation: (*a*) A preliminary thyrofissure followed by resection of a piece of the cartilage; but this has invariably been carried out in the past if the growth has infiltrated the cartilage. (*b*) A preliminary "window resection" of the cartilage. The objection to this, however, is that the extent of the disease cannot be ascertained as is possible by the usual thyrofissure, and in reflecting the cartilage off the soft parts, by undermining it with the dissector, one may break into the growth which may have extended through the perichondrium and infiltrated the cartilage and so cause the very cell infiltration which it is so important to avoid. Mr. Lack says no necrosis of cartilage occurs in his modified operation, but no necrosis of cartilage should occur in thyrofissure it has not occurred in any of the forty cases I have been concerned with, since the days when stripping the external chondrium off the cartilage was discontinued. In conclusion, there appear to be some advantages in the modified operation suggested by Mr. Lack, for I agree with him that it may be possible to remove a more extensive growth than by the usual method of thyrofissure.

but it can never be an alternative to, or take the place of the present day method of performing thyrofissure in those early cases in which thyrofissure is indicated. If carried out only in advanced cases of endolaryngeal cancer which have passed the ideal stage of thyrofissure, and if preceded by a preliminary thyrofissure, Mr. Lack's modified operation should prove of great value and take the place of hemilaryngectomy, because the framework of the larynx is left intact, thus avoiding the risks and disadvantages of hemilaryngectomy. Mr. Lack thinks that his suggestions make the operation of thyrofissure more easy, but thyrofissure is in itself a simple operation, the difficulties met with being mainly post-operative. Sir StClair Thomson recently remarked in connexion with thyrofissure that the simpler the operation the more perfect it is likely to be and that we have now got beyond the complications which were formerly experienced. I think that we have now overcome with our present day technique all the difficulties which were formerly associated with this operation.

Mr. TILLEY: I have no extensive experience of Mr. Lack's "window resection." Since the matter was described two years ago, I have employed it in two cases, and I did not see any particular advantage in it over the ordinary laryngofissure. I carefully followed Mr. Lack's directions. The cases did well, but no better than after simple splitting of the larynx. I agree with many of the points made by Dr. Irwin Moore in favour of laryngofissure. The retraction of the two halves of the larynx need not be violent, and the view obtained is as good as one could wish for. With regard to difficulty in swallowing, in a case I operated on three weeks ago, in which I had Dr. Irwin Moore's help, both vocal cords were extensively diseased, and both were removed, and there was great difficulty in swallowing for three days. Feeding was carried on *per rectum* for forty-eight hours, and afterwards the patient could swallow liquids by the mouth. Of eighteen or nineteen laryngofissures which I have performed, that was the only one in which there has been difficulty of swallowing. Mr. Lack's operation is practically a hemilaryngectomy, just leaving the upper and lower borders of the thyroid wing to support the parts afterwards, and when such an operation is necessary I agree that Mr. Lack's modification might be very useful, and also when one is not sure whether the disease has pierced the cartilage, because I feel that when there is even a tiny button of disease to be seen on the outer surface of the cartilage, the prognosis is not good and recurrence is probable unless free removal of the disease has been carried out.

Mr. W. STUART-LOW: Mr. Lack's results are very good, and I am particularly pleased to see he did not perform tracheotomy. When doing laryngofissure most operators still perform tracheotomy, but in many cases there is no necessity for it. The operation for laryngofissure is a comparatively simple and easy one, as, if the incision is kept in the middle line no vessels are encountered. Another point in favour of dispensing with tracheotomy is that the operation is quite quickly performed. I know of cases in which this operation of laryngofissure was prolonged for an hour, but I cannot see why this is at all necessary, especially if tracheotomy is dispensed with. More cases would submit to laryngofissure if these points were more generally recognized.

Dr. H. J. BANKS-DAVIS: If the operation is done with only local and not general anæsthesia, tracheotomy may be dispensed with. But if the patient is anæsthetized, it is safer to perform tracheotomy, because there may be bleeding when the growth is cut into. If the patient is anæsthetized, he cannot cough this blood up, whereas he could if under local anæsthesia.

Mr. E. D. D. DAVIS: My experience of Mr. Lack's method gives me the impression that a better exposure, particularly of the arytæno-epiglottidean fold is obtained, and it is in this soft tissue that a recurrence takes place: also in the position of the ventricular band. It may be necessary to divide the thyrohyoid membrane. Difficulty in swallowing after laryngofissure is due to removal of the arytænoid cartilage, and severe injury to the arytænoid region may result in having to feed the patient by tube. If the growth is divided with a sufficient margin, leaving the arytænoid cartilage more or less intact, the patient does better and makes a more rapid recovery.

Dr. J. W. BOND: The voices in Mr. Lack's cases are unusually good, better than we generally get in cases where laryngofissure has been done. One of these patients is young and so one would expect a good recovery. I have done laryngofissure without tracheotomy: the need for that depends on the case. I have tried local anæsthesia once or twice, but it has ended in a general anæsthetic having to be given and a tracheotomy done. Hæmorrhage in some cases is severe. In a case I operated on three years ago, I put eighteen ligatures on before I got into the larynx; I suppose the patient had some arteriofibrosis; he is alive yet. I think the great merit of Mr. Lack's operation is, that it enables the operator to obtain a good view of the parts. In laryngofissure my great difficulty has been to see the interior of the larynx sufficiently, and I cannot agree with a previous speaker that one readily gets a good view. I once did the operation on a woman who had an inch of fat over the larynx and it was very difficult to see the parts properly, but in Mr. Lack's procedure one can see through a large round opening. As to operating when the cartilage is affected I had a case of malignant disease perforating the cartilage: I did not do a hemilaryngectomy, I removed all the cartilage round tne growth as well as the growth, and the patient did very well and is still alive.

Dr. IRWIN MOORE: I would like to ask Mr. Lack how he decides which way he will do the operation in any particular case, whether by preliminary fissure and resection of cartilage, which is what we do now under thyrofissure, or by preliminary "window resection" of the cartilage without fissure.

Mr. LAMBERT LACK (in reply): I am very glad that this operation has met with criticism. I would do the operation in the way I have last described the first method, which I showed here two years ago, was the first step which led on to the second and better method. The first cases showed it was possible to do the operation and get a good result by removing the cartilage. That simplified the operation a good deal, but the treatment I now carry out simplifies it still more. The operation is simpler than thyrotomy; there is no need for tracheotomy and it takes only half the time. There is no question that by this method the operator does get a better view. With regard to its being a more complete operation, I think those who spoke rather admitted that when they said it might take the place of hemilaryngectomy. If that is so, and if it is suitable for cases which are too advanced for thyrotomy, it is admittedly a more complete and a better operation. I showed these cases not to discuss the operation, but to show that the after-results are as good as those obtained by any other method.

Thyroid Tumour at Base of Tongue.

By H. LAMBERT LACK, F.R.C.S.

THIS patient, an adult woman, shows a large smooth mass at the base of the tongue, just in front of the epiglottis. It is presumably an aberrant thyroid and the case is shown chiefly as a rarity. The tumour causes little inconvenience, but any suggestion as to diagnosis or treatment would be welcome.

DISCUSSION.

Mr. HOWARTH: I showed two similar cases before the Section in 1913, when I looked up all the recorded cases and found that they are not so rare as might be thought. I found that up to the end of 1913 there were records of eighty-six cases, but I have not been able to bring the statistics up to date because of the war. Both my cases suffered from difficulty in swallowing, and one case had severe dyspnœa when she bent down. In both some operation was needed. In view of operation it is possible that removal of the tumour will cause myxœdema or thyroid insufficiency, and this leads to the question as to whether it is the whole thyroid or only part that is aberrant. I think that it is usually only a part: that part which is developed from the median *anlage*, and it depends on how much thyroid is developed from the lateral *anlage* as to whether the patient has enough thyroid tissue to carry on with. In both my patients I was able to establish the fact that lateral lobes were present, as I did a preliminary laryngotomy, it being before the days of intratracheal ether. If some thyroid tissue is present in the neck and the symptoms justify it I think the tumour should be removed through the mouth. Usually there is a good deal of bleeding, especially in those that are not encapsuled. External incisions seem to me to be unnecessary. Operation in the present case does not seem essential, as the patient has no special symptoms. One of my cases was under observation for eight years before operation was thought justifiable. These tumours may be associated with other abnormalities of the thyroglossal tract. I recently saw a patient with a thyroglossal cyst, and the laryngeal mirror revealed a small tumour at the base of the tongue of which she was completely unaware.

Mr. W. STUART-LOW: I introduced the method of slitting the tongue from tip to base in the middle line, and so freely exposing the tumour. The tongue is then rapidly stitched up again, and healing takes place at once. I showed before this Section a case in which this had been done—viz., that of an adult female whom I saw two or three months after, when the operation had been in every way successful, there being no recurrence of the tumour. I described the method in 1909 in the *British Medical Journal* in my article "A Contribution to the Surgery of Lingual Thyroids."

Mr. H. L. WHALE: I had severe bleeding in a case of this nature. I used an ordinary needle-holder for curved needles, and broke four needles in succession in the substance of the tongue. I rescued them, and the end was satisfactory, though the patient was bleeding all the time.

Mr. HARMER: I saw the late Sir Henry Butlin operate on several of these cases as long ago as 1895, and I think that they are not very uncommon. He performed a preliminary laryngotomy, as suggested by Dr. Bond, and plugged the pharynx so that blood could not enter the air passages during the operation. For large tumours he split the tongue from the tip to the back, to secure a really free exposure. I think most of his cases did well, but he drew attention to the point referred to by Mr. Howarth. One of Butlin's cases developed myxœdema after operation, although there seemed to be a normal thyroid gland in the usual situation. As he remarks: "I have come to the conclusion that these tumours should not be treated by operation lightly. The mere cutting off the tumour at the base of the tongue in Seldowitch's case with a galvano cautery loop was followed by symptoms of myxœdema."[1]

Submaxillary Gland Suppuration.

By DAN MCKENZIE, M.D.

THE patient, a man aged 30, has had trouble in the left submaxillary region for several months, and he removed from the floor of the mouth, several weeks ago, what he supposed to be a displaced tooth, but what obviously must have been a calculus.

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[1] Burghard, "System of Operative Surgery," 1909, ii, "Operations on Tongue" (Butlin), p. 213.

When first seen a fortnight ago there were the usual signs of abscess involving the sublingual and submaxillary regions, and pus could be plentifully squeezed out of Wharton's duct in the floor of the mouth. Under cocaine, the abscess was freely opened from the mouth and has drained and dried up. But the whole of the gland remains indurated and tender. Calculus was looked for when the abscess was opened, but no débris could be discovered.

Suggestions as to further treatment would be welcome.

A Large Submaxillary Calculus.

By Dan McKenzie, M.D.

This stone, of a roughly spherical shape, measures 1·7 cm. by 1·3 cm. (or nearly ¾ in. by ½ in.), and weighs 1·85 grm. (or 29 gr.). It was removed for the relief of pain from the floor of the mouth of a man aged 56. A general anæsthetic was given and after the mucosa had been picked up with forceps and freely slit up, the calculus was removed from its bed by means of a Volkmann's spoon. It is entire save for one small fragment.[1]

DISCUSSION.

Mr. Tilley: I have had experience of these submaxillary gland suppurations in one of my immediate relatives. In this instance the patient had for many years suffered from the swelling of the submaxillary glands, which, during meals, extended downwards nearly to the clavicles, and at the same time pain and irritation were intense. Purulent discharge could be squeezed from Wharton's duct. There was no calculus. The operation consisted in removing the submaxillary glands entirely, from the outside. When cut into, they were of a greyish-black colour, and very fœtid. I do not think anything short of removal will be of any use in Dr. McKenzie's case. Since removal, my patient has not suffered from digestive troubles nor from a dry mouth. I presume the numerous other glands keep up the necessary secretion of mucus.

Dr. Jewell: Four months ago I removed a submaxillary gland for a similar condition. There was healing by primary union, and the patient has remained well ever since. Her pain was more acute than in Dr. McKenzie's case, being at times almost unbearable, and relieved by the occasional passage of pus from Wharton's duct into the mouth. No calculus was detectable by the probe, but the skiagram showed one in the gland very well. The gland was cut and looked fibrous to the naked eye, and contained a calculus of about the size of a quarter of a pea.

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[1] The calculus is now in the Royal College of Surgeons Museum.

Mr. HARMER: I agree that where there is suppuration in the submaxillary gland, which has persisted for some time, there is only one cure, namely, removal. It is easy to carry out. If the patient be a lady, one has to consider the disfigurement which may result.

Mr. O'MALLEY: In a parotid gland case, the patient was sent to me for a septic condition of the mouth and throat associated with a poor state of health. On examining the mouth, I found pus oozing into it on pressure on the parotid, the left parotid being enlarged. It was a difficult problem to do anything which offered a good result, but I suggested that the patient should get some chewing-gum, and chew it half an hour before a meal, so as to stimulate the activity of the gland, and then wash the mouth out with a solution of peroxide. After a few months the doctor reported that it had cleared up. It has not recurred, and that is over a year ago.

Mr. E. D. D. DAVIS: I have removed the whole submaxillary gland on four occasions: Two for suppuration, one for endothelioma, and a fourth for pain and inflammatory attacks following the impaction of calculi in Wharton's duct. The result was satisfactory in each case, and there was no disadvantage arising from the loss of the gland. In the case of a girl, in spite of the fact that the wound healed by first intention, there was some deformity and a depression owing to the loss of tissue.

Mr. H. L. WHALE: The difference between the treatment of a submaxillary gland case and a parotid gland case depends upon the fact that the first is very easy to operate on, whereas the other is formidable, because of the liability to buccal fistula.

Dr. IRWIN MOORE: I showed a similar case at the last meeting of this Section, together with a skiagram which showed a calculus in the submaxillary gland. When I operated, I found the stone was in Wharton's duct, and it was easily removed through an incision. This case now shown may have another small calculus in the gland which would be difficult to feel, and might account for the induration. In one case I had years ago, I felt sure there was no stone in the gland, but it turned out there was one. The present case has been improving since the operation and I think that if no further calculus is present ionization to the enlarged gland would cause a rapid subsidence of the swelling.

Dr. MCKENZIE (in reply): My personal experience has been the opposite to those of other speakers. Some said they start to operate on a gland in which they do not expect to find a calculus, and they find one: but I have started to operate on a calculus which I did not find. I spent an afternoon looking for a calculus which I had felt distinctly in the floor of the mouth. I put the patient under an anæsthetic, and made the usual incision, and, as I did not find the stone, I concluded that my knife had pushed the calculus farther and farther, until I found myself under the skin near the outside. I removed my knife, and took a bit of the gland, which was so hard that I thought it was carcinoma, but the microscope showed it to be simple fibrosis. The patient was put on potassium iodide, and did very well. It would seem therefore that fibrosis of the submaxillary gland may get well if a free incision through the substance of the gland be made. The salivary gland should not be removed unless it is absolutely necessary.

A Large Dental Cyst involving the Floor of the Nose.

By E. D. D. Davis, F.R.C.S.

A tailor, aged 30, attended the hospital for a discharging sinus in the position of the right upper central incisor. Six years ago a cyst had been opened and scraped at another hospital and packed with gauze for a long period, hence the sinus. On examination a large cyst was found occupying the right palatal process of the maxilla. The cyst extended from the first right bicuspid to the left central incisor, a width of 1 in., and its length or backward extension into the hard palate was more than $1\frac{1}{2}$ in. An opening had been made between the cyst and the nose, and the floor of the cyst bulged the hard palate downwards on the right side.

The exhibitor removed the cyst on April 18, 1918, and the operation performed was as follows: The right lateral incisor and canine were extracted because the roots of these teeth projected into the cyst. The greater part of the bony anterior wall was removed, and the cyst wall was carefully peeled out and detached, the bony floor of the cyst with the exception of the alveolus was then removed, and the mucoperiosteum of the palate pressed upwards to attempt to obliterate the cavity. The hole in the roof of the cyst communicating with the nose was enlarged and its edges shelved off. The right maxillary antrum was explored and found to be normal. A section of the cyst wall is shown.

Free criticism of the operation is invited.

The Result of the Removal of Large Dental Cysts.

By E. D. D. Davis, F.R.C.S.

Case I.—A woman, aged 26, had a large dental cyst occupying the left premolar and molar region from the left canine to the maxillary tuberosity. The floor of the antrum was pushed up, but did not communicate with the cyst. The bony anterior wall and floor of the cyst were completely removed, and the cyst peeled out.

Case II.—This case was practically identical in position and treatment as the above.

A Dental Cyst involving the Nose.

By E. D. D. Davis, F.R.C.S.

A TELEGRAPHIST, aged 32, complained of a swelling below the right ala of the nose of five years' duration. A cystic swelling occupied the right canine region and projected into the nose below the anterior end of the right inferior turbinal. The wall of the cyst within the nose was thin and translucent, and with cocaine anæsthesia on November 12, 1915, this portion of the cyst wall was removed with punch forceps to establish drainage into the nose. The cyst has now practically disappeared, with the exception of a dry sinus within the nostril.

DISCUSSION.

The **President**: On what grounds does Mr. Davis put down the last case as a dental cyst? We know that dental cysts may extend to the floor of the nose and cause an elevation there, but there is also a class of cases in which a cyst may develop in the soft tissues of the floor of the nose, and be unconnected with any tooth. Is not the last case such a cyst? With regard to treatment, I have peeled out many dental cysts, but it is unnecessary, as you can get good results by taking out a large segment of the cyst wall, after which the whole sac shrivels.

Dr. **Paterson** (Cardiff): I agree with Mr. Davis. I have come across cases which have been operated upon before, and I have had to deal with some of my own a second time too. I not only peel out the cyst completely, but remove a considerable part of the bone. Sometimes there is a large cavity to obliterate, and it may be impossible to get the cavity to shrink until one has dealt with the bony wall and removed the cyst completely.

Mr. O'MALLEY: My experience coincides with that of Mr. Davis and Dr. Paterson. During the last year I have had six of these cases, which were referred to me by dental surgeons as antral cases. I had some difficulty in persuading them that the antra were not involved, but was able to do so by having a skiagram taken, with a probe in the cyst cavity. Some came to me after the cavities had been opened and packed with gauze, and were in a filthy condition. Opening must be free. The outer wall should be taken away freely, so that one removes the projecting portion of distended bone. And where it starts from the upper part of the cyst, you can easily peel away the lining wall. If you attempt to dissect the lining wall of the cyst from below, it is more difficult.

Dr. MCKENZIE: These cases may be easy or difficult: perhaps the difference may be due to their size, as some are very large. In a patient of mine the cyst had become very large, filling the wall of the antrum, and causing great bulging forward of the cheek. It had been suspected to be malignant. The bony covering of the cyst was very thin, and all that could be done was to obliterate the cyst wall. But a large opening into the mouth was left. Later, I tried to close this fistula by a plastic operation, but it re-formed. The final result did not please me, although I cured the cyst. But only a pressing back into place of the greatly distended and distorted bone of the superior maxilla would have rendered the closing of the fistula possible.

Mr. LACK: Why are there differences of opinion concerning simple cases of this kind? I agree with the President that the majority of these cases do well after free opening: there is no need for dissection.

Dr. H. J. BANKS-DAVIS: I have had such cases come up to the out-patient department, for months after operation, with large holes, and I cannot close them up. The worst case I have seen was that of an old gentleman aged 80, who had had a large cyst of the upper jaw. I removed it freely, but I could not get the cavity to close, and he worried about it so much that he was removed to an asylum, where he remains.

Dr. KELSON: Patience is the great thing with these cysts. I find they take, after opening and removal of inner wall, three to six or nine months to fill up, but that the end result is satisfactory. What does Mr. Davis propose to do in his first case, as the man seems to be in a somewhat parlous condition, with a large hole leading from the mouth to the nose, which does not seem likely to close?

Dr. IRWIN MOORE: I showed a case of a nasal retention cyst at the first meeting of this Session. I made up my mind to do a sublabial rhinotomy and dissect out the cyst, but in the meantime the patient went to Charing Cross Hospital to have his teeth attended to. There the dental house surgeon incised the cyst, and treated it by gauze packings, which resulted in a cure.

Mr. E. D. D. DAVIS (in reply): The last case is doubtful: there was a septic canine tooth on that side, and the cyst was at the apex of the root. I have no further proof that it was a dental cyst. I have seen a considerable number of dental cysts which have been opened and packed or scraped and the result has been very unsatisfactory and the sinus has persisted for years. The first patient, the man I showed to-day, was a difficult case and the cyst had been packed for six years. When I saw him there was a discharging sinus and a large cavity to obliterate and his operation was done only fifteen days ago. I removed the floor of the cyst so that the mucoperiosteum may form granulation tissue to fill the cavity. I agree with Mr. O'Malley if one removes all the bony anterior wall, the cyst is easily peeled out, but if you attempt to peel out the cyst through a small opening the dissection is difficult. In the cases of cysts in the molar region, the cavities healed completely in two months. In my experience, opening and scraping without removing the cyst wall has been decidedly unsatisfactory.

A Colony of Actinomyces in the Crypt of a Tonsil.

By W. DOUGLAS HARMER, M.C.Cantab., and
A. C. STEVENSON, M.B.

PATIENT was a boy, aged 9, who had suffered from chronic tonsillitis for six months. There had been a crypt in the left tonsil from which pus was constantly oozing, and the tonsillar lymphatic glands were swollen, the largest being an inch in length. The tonsils were enucleated and sent to Dr. A. C. Stevenson, who found no evidence of tubercle, but in one of the tonsils, lying in a deep crypt, there was a colony of actinomyces. The section was shown to Professor Shattock, who reported: "A colony of actinomyces. In a Gram-stained preparation a few normal branching filaments occur and large numbers of coccus-like forms, well stained, which may be taken as spores. The periphery of the colony is regularly fringed with clubs, but these are all finely granular. In a logwood-eosine preparation very little is stained, indicating that the colony is largely degenerate. The crypt in which the colony lies presents no signs of invasion, but the epithelium is here and there desquamating and at one spot the colony is invested with an exudate containing polymorphs."

One week after operation the glands had almost disappeared.

The specimen is shown in order to obtain information as to the rarity of such conditions.

DISCUSSION.

Dr. H. J. BANKS-DAVIS : Six months ago I took a case to Mr. Tyrrell Gray. The patient, a woman aged 50, had a peculiar condition of the tonsil and enormous glands in the neck, extending to the clavicle. I asked him whether it was malignant, and he said that he thought it was actinomycosis. His impression proved correct. After excision it cleared up. The pus contained actinomyces. The glands had got infected through the tonsil. The patient quite recovered.

Dr. IRWIN MOORE : These cases are very rare, but I have seen a few cases reported in American literature. I will look up the references.

Postscript.—Arthur Cheatle and W. D'Este Emery[1] in 1904 were the first to report and describe a case of actinomycosis of the tonsil in this country. Butlin,[2] in the discussion on this case, referred to one case he had seen some years previously. Jonathan Wright[3] (New York), in 1904, was the first to describe a case in America. He refers to one case reported by Lesin[4] in 1895, also to four cases described by Ruge[5] in 1896. T. K. Hamilton[6] (Melbourne), in 1910, reports one case he had seen.

Mr. HARMER (in reply): This was discovered accidentally. The tonsil was cut for tubercle, but no tuberculosis was discovered. Dr. Davis, a pathologist in Chicago, has investigated this question. He claims that in examining 130 pairs of tonsils from children, actinomyces were found in 30 cases, and he quotes articles by others who also declare the condition to be common. I bring it forward because although I have had a number of tonsils cut, mostly for tubercle, I have not previously had a report of actinomyces, and I wanted to hear the experience of others.

Dr. A. C. STEVENSON (in reply): My chief difficulty from the pathological point of view is what type of actinomycosis we have here. No smear preparations were made, nor was it possible under the circumstances to attempt cultivation or to inoculate into test animals. In some ways, the type resembles the bovine, and in others the human variety, the latter having few, the former many clubs. The peculiar sporulating appearance at the edge of the tumour has only once before been described to my knowledge in the article by Dr. Davis, which Mr. Harmer quoted. Davis came to the conclusion that the nodules in the tonsil were not actinomycotic in nature, as he only got the *Bacillus fusiformis*, spirilla, and cocci, from cultures and inoculation experiments. It is probable that even after washing the nodule as he described, he would get these organisms, common elements of the flora of the mouth. After inoculation of guinea-pigs with the nodules, Lord, of America, found typical actinomycotic lesions present.

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[1] "Specimen of Actinomycosis of the Tonsil," *Proc. Laryng. Soc. Lond.*, 1904, xii, p. 5; *Journ. Laryng., Rhinol., and Otol.*, 1904, xix, p. 679.

[2] Loc. cit.

[3] "Actinomycosis of the Tonsil," *Amer. Journ. Med. Sci.*, 1904, cxxviii, p. 74.

[4] *Wratsch*, St. Petersburg. Abstract in *Centralbl. f. Laryng.*, 1895, xi, p. 901.

[5] *Zeitschr. f. klin. Med.*, 1896, xxx, p. 529.

[6] "The Faucial Tonsils and their Relation to Various Local and General Diseased Conditions," *Austral. Med. Journ.*, Melbourne, 1910, xv, p. 329.

Paralysis of the Arytænoideus in a Woman, aged 24.

By H. J. Banks-Davis, M.B.

The cords meet in the anterior two-thirds, and gape in the posterior third. Loss of voice is complete. Paralysis of the arytænoideus is very rarely functional. In this case the thyroid is enlarged, and is probably the cause of the paralysis. This is a very difficult condition to cure.

DISCUSSION.

Dr. Smurthwaite: I think this case is purely psychic in origin and can be cured by psychological methods. In trying to phonate, she brings the false cords together, showing a big effort is being made. It is like what I see in many shell shock cases: the anterior two-thirds of the cords are brought together. If the patient can be got to breathe deeply and overcome the spasm, the voice will return. Dr. Banks-Davis says paralysis of the arytænoideus is very rarely functional. If he means the crico-arytænoideus posticus this is so: an abductor paralysis is not functional. This patient has no abductor paresis, for the cords can be approximated. The crico-arytænoidei, thyro-arytænoidei and interarytænoideus are supplied by the recurrent laryngeal. If the thyroid disease were the cause of the patient's loss of voice we should expect both recurrent laryngeal nerves to be involved, and consequently all the above muscles.

Mr. Mark Hovell: I have only seen one case of paralysis of the arytænoideus. The patient was under the care of Sir Morell Mackenzie at the Throat Hospital, Golden Square, and is mentioned in his work on "Diseases of the Throat." The affection had existed for several years before I saw her, and she was under my observation for upwards of forty years, during which time there was no alteration in the symptoms. She died in January last, aged 88. I think the present case is not one of arytænoideus paralysis.

Mr. O'MALLEY: I agree that the important factor in this case is a functional one; there is considerable straining and tightening of the larynx, as is often seen in soldiers who have shell shock with laryngeal trouble—cases of functional trouble grafted on a laryngeal affection, such as catarrh. I regard this case as of that type. My plan of treatment is to carry on the examination, and use friction with the mirror on the pharyngeal wall until a good deal of secretion drops, and then the glottis will close as a protective effort, bringing the cords into firm apposition. The next deep expiration will produce phonation, which will at once convince the patient there is a voice and, as a rule, there is no further difficulty in getting him to speak distinctly instead of whispering.

Dr. DONELAN: I think this is a case of functional paresis of the transversus in a hysterical subject. It is specially interesting in that the oblique fibres of the arytænoideus which bring the apices of the arytænoid bodies together are not affected, as shown by the approximation of the cartilages of Santorini. While the cartilaginous glottis remains open in characteristic triangular form from inaction of the transverse fibres, the ligamentous glottis is very slack from paresis of the internal thyro-arytænoid. I have looked up the Continental literature on this subject and I find there is general agreement that the arytænoideus is very rarely affected except functionally or as a myositis. It is also in favour of a general neurosis that the patient has had previous attacks of loss of voice, has irregular menstruation, and is very emotional.

Dr. JOBSON HORNE: It is just twenty years since I first wrote on this subject. In 1898[1] at the annual meeting of the British Medical Association at Edinburgh I pointed out that a certain group of cases of so-called functional aphonia were myopathic in origin. Under the microscope I demonstrated a myositis in the intrinsic muscles of the larynx, and in particular in the interarytænoid muscle. The specimens were obtained from patients who had died from tuberculosis of the lungs, and in whom the larynx presented perhaps no gross or naked-eye evidence of disease. I further pointed out the danger of overlooking pulmonary tuberculosis at a time when one could be most useful to the patient, through labelling a case of loss of voice as "functional" aphonia without taking a wider view and ascertaining the cause of the "functional." The deduction was in all cases, male or female, of loss of voice so unfortunately described as functional, to eliminate as far as possible the presence of pulmonary tuberculosis.[2]

Mr. WHALE: I have seen many of these cases in the last year or two in France, and I endorse Mr. O'Malley's opinion that they are functional aphonia grafted on to a subacute laryngitis. They are common in France, in men who come from the line. These glottides are more common than the diamond-shaped glottis of hysteria. I have seen them in men who are recovering from gassing, in whom there is no other sign of congestion or inflammation. I should be very sorry to think that all these people are going to have tubercle. Our treatment for them is mental therapy: persuading them that they will get well.

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[1] *Lancet*, 1898, ii, pp. 419, 518; *Journ. Laryng., Rhinol., and Otol.*, 1898, xiii, pp. 481-486.

[2] This patient lately developed acute pulmonary tuberculosis and is now in a sanatorium; this is interesting in view of the remarks made by Dr. Jobson Horne.—H. J. BANKS-DAVIS.

Mr. LACK: These cases are very rare in civil life. Mr. Hovell has seen only one, I have seen the same case and another. The first case lived to 80, and did not develop tuberculosis. In neither case was there the least attempt to close the posterior part of the glottis on phonation. The arytænoideus showed no trace of movement. These cases may be functional, but I have never seen any benefit from any kind of treatment. On the other hand it is difficult to believe that the paralysis can be due to pressure on the recurrent laryngeal nerve, as pressure on that nerve always affects the abductors first.

The PRESIDENT: I agree with those members of the R.A.M.C. who have spoken of the frequency of this condition: in fact, in the functional aphonia of soldiers this transversus muscle is the most frequently affected of all the laryngeal muscles, and the most intractable. I hope that in our next month's discussion on warfare neuroses someone will throw light on the subject, because it is very striking how paresis of so small a muscle causes so much vocal disturbance. I have not always been fortunate in the treatment of these cases.

Dr. PATERSON: I also have seen these cases from time to time. One sees them from France suffering from aphonia, some of them after gas. After undergoing local and other treatment for months with varying success, one has had not infrequently to discharge them with little voice to a Command Depot, where they may be under regulated discipline.

Mr. HARMER: The condition is common in soldiers, the number I have seen probably running into double figures. The prognosis is bad, but functional aphonia in soldiers is very difficult to cure. I have also seen several officers with this condition: they have been sent to Mr. McMahon for voice training, and their voices have recovered.

Case of Trichotillomania with Delusions of Nasal Origin.

By H. J. Banks-Davis, M.B.

This patient, a woman, aged 37, was transferred to my department by Dr. Pernet, whose notes on the case are appended.

When I saw her first six months ago she was perfectly reasonable, but was convinced that her nose was infested with maggots: that at times they crawled up through her nose and brain on to her scalp, where they commenced devouring her hair, and she pointed to the condition there as the result of this invasion. The nose is narrow, but nothing abnormal exists. Unfortunately a septal resection was performed some years ago, and in her mind it is firmly fixed that this operation was performed for troubles then which she alleges she suffers from now.

I have seen her in an attack, produced by "tickling" the septal tubercle with a probe. This produces intense irritation of the scalp. Her anxiety becomes extreme and she rubs and plucks at her hair in a manner quite absurd. The sensation then passes off and she points to the damage done by herself as that done by phantom maggots grazing on her scalp.

Light cauterization of the septal tubercle has relieved these attacks which, presumably, are entirely of reflex nasal origin.

Dr. Pernet's notes (abstract) are as follows: "The patient came first to my skin clinic for loss of hair and strange symptoms about the scalp which she attributed to 'live things': she felt them start from her nose and crawl up inside her head. The hair in patches was short, with broken ends, and under the microscope brush-like, evidently the result of breaking them off by rubbing. On account of her nasal symptoms I sent her to Dr. Banks-Davis. Later she saw me again and brought some maggots with her (larvæ) which she said had come out of her nose. This was in June. I sent her to Dr. Grainger Stewart to deal with the mental condition (désagrégation physiologique) and the delusions. On January 16 she again insisted on 'live animals' crawling through her nose on to her scalp, but on questioning her she stated that no maggots had 'come out' since she had brought me those in June ('there are none at this time of the year')."—G. Pernet.

Dr. JOBSON HORNE: The case, I think, affords a further argument for a stock-taking of, and a discussion on the *end*-results of, submucous resection of the nasal septum.

(?) Thyro-lingual Cyst in a Girl, aged 20.

By H. J. BANKS-DAVIS, M.B.

WHILE awaiting admission for operation for a thyro-lingual cyst, which produced symptoms interfering with deglutition, I advised the patient to paint the cyst twice a day with tincture of iodine. In a fortnight the swelling disappeared, rendering no further treatment necessary.

Double Perforation of the Septum Nasi with Peculiar Appearance of Soft Palate, in a Man, aged 46.

By H. J. BANKS-DAVIS, M.B.

THE cause of the condition is obvious. Apart from the double septal perforation the appearance of the soft palate is unusual. The perforation in the soft palate, which is much attenuated, is a large one, but the point of interest is the extreme delicacy of the thread of tissue which supports the palate and uvula on the left side. It is so fine and slender that it is surprising that it has not been torn across in deglutition. Should this occur, would the palate fall over to the right side of the pharynx? and if it did, what would be the treatment?

Left Recurrent Laryngeal Nerve Paralysis in a Patient suffering from Tuberculosis.

By IRWIN MOORE, M.B.

Final Notes, together with Post-mortem Report and Specimen showing the Nerve embedded in a Mass of Caseating Glands.

PATIENT, a male, aged 45, was exhibited at a meeting of the Section on November 2, 1917,[1] under the title of "Complete Paralysis of the Left Vocal Cord due to a Mediastinal Growth."

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[1] *Proc. Roy. Soc. Med.*, 1918, xi (Sect. Laryng.), p. 49.

He was first seen on September 18, 1917, when he complained of slight hoarseness for three weeks, following the impaction of a piece of meat in the laryngo-pharynx, and since then he had only been able to swallow semisolid food.

Examination of the larynx at this time was obscured by œdema of the arytænoids caused by impaction of the foreign body. Two weeks later, on subsidence of the œdema, complete paralysis of the left vocal cord was observed (cadaveric position). An imperfect cough, bovine in character, and imperfect phonation following the exertion of speaking, were present.

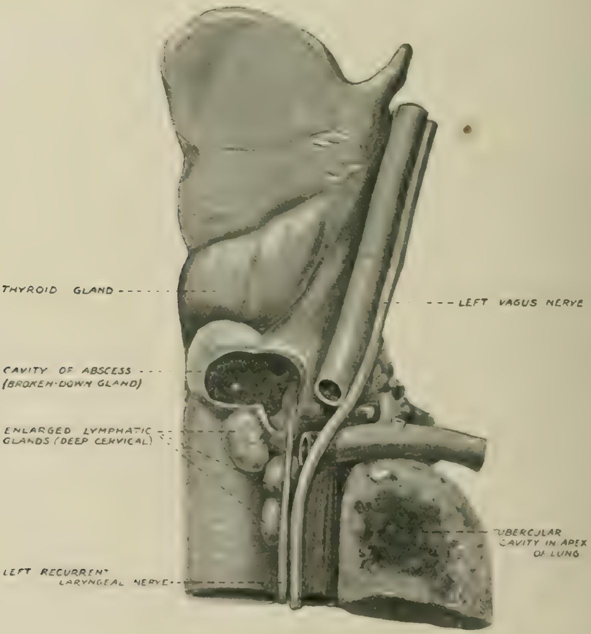
THYROID GLAND
LEFT VAGUS NERVE
CAVITY OF ABSCESS (BROKEN-DOWN GLAND)
ENLARGED LYMPHATIC GLANDS (DEEP CERVICAL)
TUBERCULAR CAVITY IN APEX OF LUNG
LEFT RECURRENT LARYNGEAL NERVE

Fig. 1.

Left recurrent laryngeal paralysis due to involvement of the nerve in the abscess cavity of a broken-down peritracheal gland. (Dissected by Professor Shattock, F.R.S., and now in the Museum of the Royal College of Surgeons.)

After November 2 patient's condition gradually deteriorated, emaciation became more marked, with high temperature, rapid pulse and respiration, also spasmodic cough and increased amount of sputum, in which tubercle bacilli were found.

In view of the opinion that a mediastinal growth was encroaching on the left lung and was probably of a malignant nature, he was treated between November 8 and December 11 with injections of colloidal copper (cuprase 5 c.c.)—six in number, at weekly intervals—preceded by 5 c.c. quinine urea hydrochloride to prevent the pain of the injection. Each injection greatly relieved the respiratory distress, reduced the temperature and pulse-rate and improved for the time the general condition of the patient. Death occurred on January 18, 1918, nearly five months after the onset of the first symptoms.

I am indebted to Dr. Kelson for performing the post-mortem and dissecting the specimen now shown.

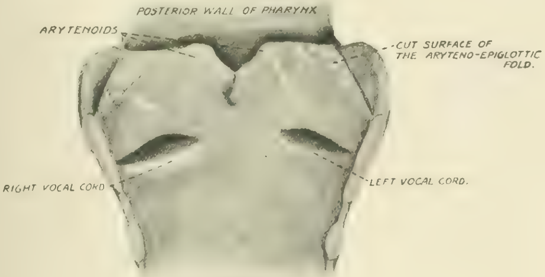
POSTERIOR WALL OF PHARYNX
ARYTENOIDS
CUT SURFACE OF THE ARYTENO-EPIGLOTTIC FOLD.
RIGHT VOCAL CORD
LEFT VOCAL CORD.

FIG. 2.

Left recurrent laryngeal paralysis. The larynx has been split open from the front in the middle line and the thyroid alæ stretched apart; shows very clearly the atrophy of the left vocal cord.

POST-MORTEM REPORT.[1]

The body was much emaciated.

Larynx.—Both vocal cords were in the cadaveric position, the left thinner than the right; on neither side was there ankylosis of the crico-arytænoid joint. The lower deep cervical and upper mediastinal (posterior) glands were enlarged and caseating, some having quite broken down. The left recurrent laryngeal nerve passed up to and became embedded in the mass.

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[1] This specimen is now in the Museum of the Royal College of Surgeons.

Lungs.—Both were consolidated at the apices and showed caseating material. On the left side was a cavity as large as a billiard ball. The pleura on both sides was thickened and adherent, but did not involve the left recurrent nerve (W. H. Kelson).

The case is of interest in that:—

(1) The first danger signal of serious disease within the chest wall was the sudden impaction of a foreign body in the laryngopharynx, for which patient first sought advice.

(2) No history could be obtained of any early symptoms of pressure on the recurrent laryngeal nerve, such as paroxysmal dyspnœa, laryngeal stridor, or paroxysmal cough.

(3) The symptoms, physical signs and X-ray examination, were compatible with, and first suggested, a mediastinal growth encroaching on the left lung, and it was not till some time later, when tubercle bacilli were discovered in the sputum, that tuberculosis was suspected.

Dr. KELSON: It is evident from the dissection that it was an actual case of paralysis from recurrent nerve involvement in a mass of tuberculous glands, some caseating, some broken down. Post-mortem records of these cases are very numerous, and there has been a good deal of difference of opinion on the subject.

Case for Diagnosis.

By JAMES DONELAN, M.B.

WOMAN, aged 62. Seen first on February 14, 1918. She had been slightly hoarse since Christmas, perhaps longer. About a month before she presented herself she had an attack of acute infectious catarrh which, judging by its traces had involved the nose, nasopharynx, larynx and trachea. She had previously attended the hospital for chronic atrophic rhinitis and ethmoidal suppuration. The hoarseness has become much more marked during the last six weeks. She was shown here on March 1, but as no definite opinion was reached she is again brought forward as requested. She had then general subglottic thickening of both vocal cords with general thickening and irregular outline of the anterior fourth of the right cord and irregularity of that part of its marginal outline. The arytænoid bodies were much swollen. This swelling has considerably lessened. When seen again on May 1 by two other medical men and the exhibitor, she had distinct lagging of the right vocal cord. The appearance of the cord near the commissure is unchanged.

DISCUSSION.

Dr. H. J. BANKS-DAVIS: There was a small nodule on the right cord, but I do not know what it was.

Mr. MARK HOVELL: To me it looked like chronic laryngitis, but it may be an early stage of something else.

Crossing of Arytænoids.

By W. DOUGLAS HARMER, M.C.Cantab.

SERGEANT B., April, 1915: Pneumonia and bronchitis, followed by hoarseness. Tubercle suspected, but not proved. March, 1918: "Gassed" in France (? mustard); voice lost completely for a few days, now steadily improving. Still has troublesome cough. No tubercle bacilli in sputum. Chest: no physical signs. Larynx shows some general redness and overlapping of arytænoid cartilages.

Dr. PATERSON: This is an unusual degree of the condition: it is not uncommon to see some degree of crossing in a normal larynx.

Case of Double Dacryocystitis.

By W. DOUGLAS HARMER M.C.Cantab.

SIX years persistent discharge from both lachrymal sacs; seventeen abscesses.

Five years ago double West's operation. Afterwards less muco-pus for a time.

April 9, 1918: West's operation repeated. Sacs are now shrinking, but still has severe epiphora and muco-pus.

Case illustrates the failure of West's operation where there is eversion of punctum from conjunctival sac. This is now being treated by cautery to invert the puncta. Sacs will gradually shrink when normal position of puncta is restored.

N.B.—Patient also has tubercular glands in neck.

Dr. PATERSON: This indicates and illustrates a condition which sometimes seriously interferes with the success of the operation. I was interested in seeing that the treatment was the cauterizing and shrinking of the mucosa, with the view of bringing the punctum into proper line.

Section of Laryngology.

President—Dr. A. Brown Kelly.

DEMONSTRATION OF SPECIMENS AND CASES OF WARFARE INJURIES OF THE LARYNX.[1]

Epidiascope Demonstration of Specimens from Cases of Warfare Injuries of the Larynx.

By W. Douglas Harmer, M.C.Cantab.

Specimen 2410: Larynx. Wound through anterior part of thyroid cartilage causing comminuted fracture, granulations and œdema of glottis. Tracheotomy. Death from septic bronchitis, one day.

Specimen 2653: Larynx. Shrapnel wound shattering anterior part of larynx. Course: Left side of neck at level of cords, and out through right scapula, after wounding pleura. Tracheotomy. Cellulitis of neck, hæmorrhage into mediastinum. Broncho-pneumonia. Death four days.

Specimen 2516: Larynx. Wound causing fracture of cricoid and upper tracheal rings. Track foul and septic. Tracheotomy. Death, double pneumonia, three days.

Specimen 1895: Larynx and pharynx. Shell wound causing fracture of hyoid bone and perforation of pharynx. Death, broncho-pneumonia, ten days.

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[1] At a meeting of the Section, held June 7, 1918.

Specimen 794: Larynx. Bullet wound through right aryepiglottidean fold, causing acute laryngeal congestion. Death, bronchopneumonia, seven days.

Specimen 1652: Pharynx and larynx. Shell wound across pharynx at level of cricoid cartilage. Entrance behind left sternomastoid muscle. Course behind vessels lacerating left common carotid to right lobe of thyroid gland, where fragment was found. Death one day.

Specimen 446: Larynx. Wound causing acute inflammation and ulceration of right aryepiglottidean fold. Death, œdema of glottis, one day.

Specimen 1743: Larynx. Wound causing diffuse hæmatoma of extralaryngeal tissues and obstruction of glottis; also irritation of sympathetic nerve with unilateral exophthalmos; cerebral embolus. Death three days.

Specimen 1948: Larynx. Wound causing diffuse hæmatoma of extralaryngeal tissues and laryngeal obstruction; lesion of bifurcation of right carotid and right sympathetic nerve. Death one day.

Specimen 2556: Larynx. Wound causing destruction of anterior part of thyroid cartilage; no swelling or sepsis. Death in one minute from violent hæmorrhage from carotid artery.

Specimen 1015: Larynx. Shell wound. Entry, middle line in front; course, through larynx and œsophagus as far as vertebral column. Death, cellulitis of neck, one day.

Specimen 472: Pharynx, larynx and spinal cord. Track of bullet from left through aryepiglottidean fold, right wall of pharynx into spinal canal; injury to cord and meningitis. Tracheotomy.

Specimen 688: Larynx. Bullet wound through right ala of thyroid cartilage, left arytænoid, left sinus pyriformis. Caused extensive emphysema of neck and chest; left lung collapsed and congested; spleen and kidneys congested.

Specimen 2409: Larynx. Wound through thyroid cartilage causing sepsis and cellulitis of neck, inflammation of trachea: great dyspnœa, hæmoptysis and emphysema. Tracheotomy. Death, bronchopneumonia, three days.

Specimen 2146: Larynx and pharynx. Wound above larynx transversely across pharynx. Entry small; exit much larger and more ragged. Lungs congested and œdematous. Death three days.

Specimen 2406: Larynx. Wound in middle line of thyroid cartilage through larynx at level of vocal cords. Tracheotomy. Mucosa of larynx extensively undermined by suppuration with fenestræ due to necrosis. Death, broncho-pneumonia, three days.

Specimen 2620: Pharynx and larynx. Wound through pharynx causing hæmatoma of neck due to injury of internal jugular vein, dysphagia, aphonia, and hæmorrhage from throat (one quart). Death, hæmorrhage, three days.

Specimen 414: Pharynx and larynx. Bullet wound through œsophagus opposite cricoid cartilage. Demonstrates small amount of injury caused by many bullet wounds. Caused extensive inflammation of retropharyngeal tissues and mediastinitis. Lungs showed evidences of old pleurisy and fibrosis.

Specimen 1545: Larynx and œsophagus. Bullet wound causing dysphagia. Death, one day. "Had condition allowed, gastrostomy would have been performed."

Specimen 448: Larynx and trachea. Bullet wound through neck into chest, injuring apex of right lung. All great vessels escaped. Death, hæmothorax infected with "gas organisms," five days.

Specimen 1043: Larynx and spine. To show hæmatoma of right half of larynx. No notes available.

Specimen 272 illustrates death from hæmorrhage without injury to any large vessel.

Specimen 29 illustrates death from septic pneumonia after twenty days.

Specimen 1379 illustrates impaction of shell fragment in posterior part of larynx, causing œdema of glottis and fracture of cricoid. Death after tracheotomy.

Specimen 2 illustrates death from hæmorrhage into air-passages from wound in thyroid vessels.

The above specimens have been lent by the Royal College of Surgeons by the courtesy of the Curator.

Cases of Gunshot Wound of Larynx.

By W. DOUGLAS HARMER, M.C.Cantab.

Case I.—Gunshot wound (shrapnel) through anterior part of larynx. Wound received April, 1917. Tracheotomy May, 1917, till February, 1918. Advised laryngostomy in July and August by two laryngologists. Now has adequate glottis but very husky voice. Marked scarring externally and internally, under treatment with radium. Note reaction after fifteen hours' radium.

Case II.—Gunshot wound (shrapnel), penetrating entry left of sixth cervical vertebra, causing paralysis of left vocal cord. Shown to illustrate keloid in operation scar, much improved by twenty-four hours' treatment with radium; also improvement of voice after treatment by radium and voice production (MacMahon).

Case of Gunshot Wound of Larynx.

By HUNTER TOD, F.R.C.S.

GUNSHOT wound penetrating larynx and resulting in no injury beyond thickening of ventricular bands and huskiness of voice. Shown to illustrate the absence of scarring after wounds of larynx.

Case of Gunshot Wound of Larynx.

By J. GAY FRENCH, F.R.C.S.

GUNSHOT wound. Bullet through larynx, causing extensive damage to right half of thyroid cartilage, fixation of right vocal cord and inadequate glottis. Wounded April, 1917. Tracheotomy, "high," May; "low," October. Stricture divided October; later dilated with bougies for three months. Still has stridor and wears tube; voice remarkably strong.

Case of Shrapnel Wound of Trachea.

By W. S. Syme, M.D.

Private S. Shrapnel entered about 2 in. above the right clavicle in front of the sternomastoid on October 30, 1917. Blood poured from his mouth. For two weeks he had difficulty in swallowing and his voice was lost. His breathing was also difficult. For some weeks he was unable to raise his head from the pillow, though he was able to nod (depress the chin). Ability to swallow returned. After two weeks voice returned but was again lost after another period of about three weeks. When seen on December 19 he could speak: there was no laryngeal paralysis. There was the appearance of a cicatrix on the right side of the posterior wall of the trachea. He still had difficulty in raising his head. He has at times had return of some difficulty in breathing. (I should have said that X-ray shows a foreign body below the junction of the outer and middle third of the left clavicle.) He made a practically complete recovery.

My view of the case is that the shrapnel passed between the trachea and œsophagus and that the pressure of the hæmorrhage, which probably recurred to a slight extent at times, caused the difficulty in breathing and loss of voice and, by interfering with the action of the muscles in front of the spine, caused the difficulty in raising the head.

Cases of Gunshot Wound of Larynx.

By E. A. Peters, M.D.

Case I.—Sapper B., gunshot wound larynx and lung. Cartilage graft. June 20, 1917: Fragment of shell entered the left side of the larynx in region of cricoid and rested on apex of lung (according to skiagram). Tracheotomy performed at Casualty Clearing Station and an attempt made to remove the fragment; this was coughed up four days later. July 9: At Winchester developed right basal broncho-pneumonia. December 17: Admitted to R. V. H., Netley, with almost complete stenosis of larynx, and using a tracheotomy tube. The left cord is swollen, while the right cord moves slightly, but is infiltrated. He can speak with a hoarse voice. There is, besides the obstructing infiltration œdema, a cicatricial band at the level of the cricoid occluding the larynx. A Durham's tube was inserted and a small intubation tube passed into the larynx from below. The œdema diminished rapidly. April 3, 1918: A segment of rib cartilage was inserted by turning down a flap. The tracheotomy wound closed somewhat rapidly. He can now speak and breathe, though there is still some stridor on deep respiration.

Case II.—Private L., aged 38, gunshot wound larynx. Destruction of anterior segment of cricoid. Cartilage graft. September 8: Bullet entered above right sterno-clavicular articulation and passed out to left of middle line, destroying the anterior segment of the cricoid cartilage. September 12: Tracheotomy. October 12: Condition when seen at Netley: Complete stenosis above tracheotomy wound, except to probe. Bronchitis; Durham's tracheotomy tube inserted. The laryngoscope shows the glottis margin is swollen and there is no lumen. January 8, 1917: Bronchitis had subsided. Colonel Symonds and I incised the wound upwards and inserted a T-shaped split laryngeal tube. A few days later the upper half of the tube was retained as in Lack's method, and a Durham's tube worn at the same time. A band of cicatricial tissue appeared to form above the laryngeal tube, and the glottic opening was still infiltrated and closed. Accordingly an inverted intubation tube was inserted from below to the level of the arytænoids, and a Durham's tracheotomy tube employed. He was encouraged to attempt to speak while he closed the tube with the fingers. This treatment was alternated every three days with the use of a Parker's tube, in which an upper perforation had been made. The glottis is now slightly open. April 12, 1918: A segment of rib cartilage was inserted beneath the sterno-thyroid, near the margin of the tracheotomy wound. The lumen of the larynx has now increased and the cartilage has been drawn over the upper part of the tracheotomy wound; the wound has been still further diminished by a plastic operation. At present a laryngoscopic examination shows a somewhat contracted and infiltrated glottis. The rib cartilage segment can be felt at the upper margin of the tracheotomy wound. He can breathe through the larynx and speak if the opening is occluded.

Case III.—Private T., aged 19. Gunshot wound: bullet passed through larynx and fractured first rib. April 5, 1918: A bullet entered on right side of larynx and passed through thyroid cartilages, fractured the first rib, and passed out in left supra-scapular region. April 16, 1918: The left cord is swollen and immobile in cadaveric position. The brachial plexus is affected as shown by "riding glove" anæsthesia, and functional paralysis of extensor communis digitorum, extensor brevis pollicis, flexor sublimis digitorum, and flexor profundus digitorum, indicating damage to C. 7 and 8. Captain Cooper diagnosed functional paresis. The usual eye symptoms of irritated sympathetic C. 8 were present. Present condition: The left cord moves and discloses a pucker in anterior part of larynx. The patient considers that he was hit in the back. A skiagram shows fracture of the neck of the first rib (left), and no foreign body to be present. The eye symptoms are those of paralysed sympathetic.

Case of Shrapnel Wound of Neck and Larynx.

By W. Stuart-Low, F.R.C.S.

A soldier wounded in France on October 1, 1917, shrapnel having lodged in the neck and larynx. Shown at a recent meeting of the Section with skiagrams indicating the position of the shrapnel. The question was if the shrapnel should be removed or not. Shrapnel was not removed, and the inflammatory areas subsided and the pain lessened, showing the wise course of leaving the shrapnel embedded and untouched.

Tracheotomy was performed on October 30, 1917, at Chatham. The tube having been removed the patient suddenly became very ill with œdema of the larynx, and he was urgently admitted into the in-patient department of the Central London Throat, Nose, and Ear Hospital on November 17, when the tracheotomy tube had again to be inserted.

The case is shown to emphasize the following points:—

(1) The advisability of leaving pieces of shrapnel at rest in the tissues, even when in close proximity to the larynx and trachea.

(2) The importance of recognizing the fact that tracheotomy should be performed at as low a point in the trachea as possible in cases of this nature.

(3) The tracheotomy tube ought never to be prematurely removed.

DISCUSSION ON WARFARE INJURIES AND NEUROSES OF THE LARYNX.

Mr. W. Douglas Harmer.

(ABSTRACT.)

THE statements made are limited to a description of gunshot wounds of the larynx, and based on investigations of the histories of 245 patients. (Group I, 108 cases: Personal observations and results of letters of inquiry addressed to eighty laryngologists. Group II: 110 cases treated in home hospitals, 1914-15. Group III: twenty-three post-mortem specimens from the Royal College of Surgeons, and four fatal cases from notes supplied by Mr. Lawson Whale.)

Wounds of the larynx are quite rare compared with injuries of the jaw. The commonest place of entry is the anterior triangle of the neck; transverse wounds are more common than oblique: entry wounds in the middle line in front are very rare. Injuries of the larynx between the level of the vocal cords and cricoid are the most serious. Tracheal wounds are rare. The pharynx or œsophagus is often included. Extralaryngeal wounds are very common.

Perforating are more common than penetrating wounds. In 108 cases the wounds were stated to have been caused by bullets in 58, shrapnel in 20, shell fragments in 16, and by bayonet in 1; not stated in 13 cases. Bullet wounds appear to cause lighter injuries than ragged fragments of shell. The course of the wounds was from left to right in 26 cases, from right to left in 18, right only in 13, left only in 12; middle line 5.

There were many instances of passage of bullets through the neck without important injuries resulting. The healing of wounds in the larynx is generally satisfactory.

In many of the cases the classical symptoms were absent. The principal occurring were aphonia, hæmoptysis, hæmorrhage and dysphagia.

Injuries to the framework of the larynx are difficult to determine: when apparently slight they may prove serious. Laryngeal injuries comprise those of the epiglottis, fractures of the hyoid bone and of the cartilage. The larynx is sometimes shot away. The cricoid may

be fractured or perforated, with fixation of the crico-arytænoid joint. Perichondritis supervenes in nearly all wounds of the cartilages. The vocal cords are often injured, and the ventricular bands, if wounded, may become so swollen as to obstruct the lumen of the larynx.

Paralysis of the vocal cords after gunshot wounds of the neck is noticeable, generally abductor in type—left abductor more frequently than right.

Inflammatory stenosis is common in the early stages, due to inflammation of the mucosa, œdema, abscess or hæmatoma. Brown Kelly has reported a case of dilated glottis.

Laryngeal injuries are often complicated by wounds of the pharynx, fatal in severe cases; the cervical portion of the œsophagus is often perforated; foreign bodies may traverse the trachea; definite injury to the carotid arteries has been recorded in three cases; wounds to the large vessels generally terminate fatally.

Treatment.—In the early stages the patient must be prevented from choking. Tracheotomy is necessary in doubtful cases. Nearly one-third of the cases reported required a tube at some stage. Crico-tracheotomy is inadvisable; high tracheotomy is less dangerous than low. Prevention of sepsis in wounds is best treated by excision of the lacerated tissues. Suturing together of air and food passages should be undertaken on the lines prescribed for cut throat. Partial or total extirpation of a shattered larynx may be necessary. Tracheotomy may be necessary for stenosis of any kind; a method needs devising which will both dilate the stricture and absorb the scar-tissue. Moure has had good results with laryngotomy or laryngo-tracheotomy.

The lumen of the air-passages must be kept patent by bougies, intubation-tubes, or upward-turning tracheotomy tubes; the treatment must be gradual with small-size stenosis cannulæ.

Mortality.—This is high. There are many deaths at base hospitals in France in the first week after the injury, but in cases that reach England the prognosis is more favourable. There were only five deaths in the 108 cases mentioned above. In thirty-two cases of fatal larynx wounds the principal causes of death were sepsis, pneumonia, and hæmorrhage.

After-results.—In two-thirds of the gunshot injuries of the larynx that survive for more than a week recovery is complete save for alteration in the voice. Out of 108 cases, normal voice was obtained in 17 cases, strong hoarse in 24, weak hoarse in 12, falsetto 1, whisper 15, dumb 1; not stated 38. Officers generally obtain a useful voice more quickly than men. Some of the patients become neurasthenics. Some cases of abductor paralysis recover. In a few total paralysis supervenes.

As a method of prevention of these wounds it is suggested that a band of steel should be inserted within the soldier's collar, reaching to the jaw.

Dr. H. Smurthwaite.

WAR NEUROSES.

(ABSTRACT.)

My experience of laryngeal neuroses has been gleaned from 262 cases, classified as follows: Absolute mutism, 13; aphonia, 239; stammering and stuttering, 10. Probably the great majority of men who lose their voice or speech at the Front from shell or gas shock have it restored in the course of a few days at a base hospital by various methods—e.g., electricity, or it returns suddenly without any treatment but rest.

I shall discuss only those cases of long standing, in which, in spite of time and various treatment, a more or less complete silence persists month after month. In these functional cases I find there are four distinct variations in the position of the vocal cords on attempted phonation.

(1) Cords elliptical. Thyro-arytænoideus internus paresis.

(2) Cords can be freely abducted, but there is no attempt at adduction; cords in cadaveric position.

(3) Both true and false cords tightly pressed together—a spasm of adductor and constrictor muscles of larynx.

(4) Cords approximate in anterior $\frac{2}{3}$, triangular space in posterior $\frac{1}{3}$, paresis of inter-arytænoideus.

When there is no organic mischief to account for loss of voice and patient can freely abduct cords, but on attempting vowel "a" or "e" brings cords into any of these positions, we can be more or less certain that the case is functional. It is very important to exclude early tuberculosis, causing myopathic paresis. We must consider position of vocal cords for perfect phonation. Phonation and speech is a matter of pressure and resistance equally balanced at the site of vocal cords. False cords and ventricles play a great part in voice production. Whilst examining so many of these aphonic cases I have noted how very often the false cords are forced together—that is, they come very prominently into play as soon as the true cords offer excessive resistance to the upward pressure of air.

Etiology.—Purely psychic. We have to appreciate the influence of mind on the various bodily functions—how feelings act upon the body causing different functional troubles—emotional fatigue, fear, &c. Mental and physical over-exertion prepares a soil in these patients for the final shock producing fright and anxiety—an emotional neurosis in form of aphonia, &c.

Treatment.—(1) Moral in form of persuasion and suggestion; (2) Physical.

The moral is much the more important. The patient has lost his will-power and confidence, which we must restore by force of our own energy and influence. We are dealing with a malady of psychic origin and must therefore use psycho-therapy for treatment. The patient is lacking in the power to make an initiatory effort to bring the cords into the proper position, or the necessary expiratory blast for voice production. We instil that necessary power into him. The moral effect of one cure is a great help in our efforts in subsequent cases, therefore the "cured" should speak to other patients waiting their turn for treatment.

Physical: Physically we are dealing with two classes of cases: (1) Those in which there is lack of sufficient obstructive pressure to the expiratory blast; (2) those in which the obstruction is excessive and the expiratory blast not powerful enough. Without exception, they all breathe shallowly. They never seem to fill their lungs with air preparatory to phonation. Some of them attempt to phonate during air intake, especially men who stammer. These faults we have to correct. I make the patient expand his chest two or three times by deep breathing. Then when he is able to breathe deeply I tell him to hold the breath at the full expansion of the lungs and make a quick expiratory effort or cough. As a rule we can elicit quite a good note by this simple method, and this fact is pointed out to the patient. The simplest class of case belongs to (1) and (2). (1) Where the cords are in the elliptical position, that is lack of tension; or (2) in the cadaveric—that is, no semblance of adduction on attempted phonation. Often in these, the act of laryngeal examination, the tongue being forcibly pulled out and the patient told to say "ah," is sufficient. Here we have both a suggestive and a physical force acting. We have prepared the patient's mind previously for a cure, we now suggest a vocal sound. The physical force is an increased tension on the cords through reflex action from touching the pharynx; and also the pull on the tongue, pulling on the epiglottis tends to help the tension of the cords. But all are not so simple as this and we have to resort to various methods. In the most obstinate cases where there is an increased tension, strain or tonic spasm, we have to overcome this by either increasing the upward pressure of air, or in some way making the patient relax the cords. In many of these cases I vary my way of dealing with the patient. Often I speak most harshly to him, and in some almost brutally, but it has its effect. I make the patient carry out the most strenuous expiratory effort, and to assist him in this I place my hands round the lower ribs, the thumbs pressing just below the xiphoid cartilage, and use pressure directing him to use his abdominal muscles forcibly to phonate the sound "ah." This effort is first in the form of coughing, and by degrees he takes up my suggestion of the sound "oh" or "ah." As soon as I get, by this method, the slightest semblance of a musical sound he must replace the "oh" by a numeral, proceeding up to twenty, still employing the expiratory cough effort, and gradually he phonates the numbers without the cough; that is gradually lessening the expiratory effort. Faradism is worse than useless in these obstinate spasmodic cases. Numbers of such have been sent to me aphonic for many months, and have had prolonged internal and external laryngeal faradization, yet without result. I have never yet resorted to this form of treatment. It may act successfully in early cases of Class 1 or 2, and probably in malingerers.

With regard to ether anæsthesia, in the hope that the patient will speak normally on coming out, I have only once adopted this method, but without success. I much prefer to rely on the moral effect my words will have on the patient; not to mention the time and preparation a procedure of anæsthesia would take in such large numbers of cases.

The results are as follows: Of thirteen dumb cases all regained normal speech except two. One who had been dumb for two years now speaks in a loud whisper. The other speaks, but in a very weak and hesitating voice—though improved, he is still a physical and mental wreck. Of the 239 aphonic cases, all recovered except eleven. The majority at one sitting; in the remainder treatment had to be prolonged for one or two days. Occasionally I find that a case having recovered his voice under treatment at the out-patient department becomes again aphonic after returning to his unit the following day or a few days later. To guard against this I make the patient carry out vocal exercises before me and direct him to continue them after leaving, until he gets complete confidence in his voice. He must draw his tongue out forcibly himself and practise the sound "ah." He also must go through a course of deep breathing and humming a tone. Having once regained his voice he is thus able to maintain it by practising the natural vibration of the vocal cords.

Considering most of these cases had only spoken in a whisper for many months, the cords require, or rather their muscles, a proper exercise so as to increase their tonicity, and maintain them in a natural position for vibration. By humming a tone we get a musical sound with least effort. There is no spasm in the mouth, pharyngeal, or laryngeal muscles. The cords are quietly approximated and a continuous and uniform pressure of expiratory air keeps them in vibration. Having got the patient to fix the sound in the mouth and nasal cavities by this humming practice, I next direct him to let go the pressure at the lips and sound the vowels "ah" or "oh," thus making it "ma" or "mo." Having succeeded in this he next phonates "ah" without the preparatory humming. I next direct him to count, phonating each note slowly, holding the note so as to keep the cords in natural vibration—the exercises which they require. Having counted up to twenty he has got more or less confidence in his power and I then make him read aloud.

Dr. PERMEWAN.

As regards shock, I am inclined to think that there must be some real injury to the nerve—e.g., hæmorrhage into the sheath—in all cases when there is organic paralysis.

Dr. Smurthwaite has been successful in treating functional aphonia in soldiers. My own experience has been by no means so favourable; but probably his treatment has been more determined than mine. Sir William Milligan tells me he puts these patients under light anæsthesia, provokes a sound by the introduction of a direct laryngoscope, and that the voice often comes back when the patient has recovered. Has Dr. Smurthwaite any experience of that method? Many of these cases suffer from other nervous symptoms as well as the aphonia, and their cure is more in the sphere of the neurologist than the laryngologist. In any case a serious attempt should be made to cure these very numerous cases, and in particular they should not be aggregated together in numbers in one hospital, or they react on one another.

Dr. E. A. Peters.

I showed here to-day two cases illustrating a method of treating laryngeal stenosis in gunshot wounds of the larynx. The obstruction of the larynx is due to: (1) Solid œdema resulting from vasomotor change and inflammation; (2) it may also be due to secondary cicatricial change. A Durham tracheotomy tube is inserted into the trachea, and a small intubation tube is placed above that, with the expanded end below. The intubation tube is secured by a thread passed round the neck. The tube, in the first instance, did not reach as far as the inter-arytænoideus: the glottis is so tense that it splits if even a small tube is passed through. So a shorter tube was used, and tubes of larger calibre were gradually introduced. There was no attempt made to dilate the larynx, but to keep a certain amount of opening and induce functional activity. This was further encouraged by inserting, later, a Parker's tube with an upper perforation above. Later cicatricial stenosis of the cricoid was treated by introducing a piece of cartilage from the rib, inserted under a quadrilateral flap. This cartilage provided a basis for the contracting material to adhere. In this way a fairly good result was obtained, although, as Sir StClair Thomson pointed out, in one case there was very considerable contracting of the glottis, but that was recent, and was due to the case having been pushed rather rapidly through these stages.

Mr. J. F. O'Malley.

In cases of unilateral paralysis of the cord I think there must be a physical injury of some sort when the nerve is involved. The possibility of a shock remote from the nerve, such as wind pressure, causing injury, is of interest, but I do not subscribe to that opinion. Nearly three years ago I wrote a short article on the subject, in which the views I put forward were largely those given by Dr. Smurthwaite now. I was interested in the point Dr. Smurthwaite made about the ventricle, because I noted in many of the cases that when the patient attempted to phonate, the true cords came together with sufficient firmness to give a phonatory effect with the expiratory blast, but it seemed to be damped down by the approximation at the same time of the false vocal cords. My idea was that it was a chronic inflammatory thickening of the false vocal cords: I think they are in the nature of chronic laryngeal cases, rather than of pure neurosis.

With regard to treatment, the whole key to the treatment of a pure case of neurosis of the larynx is that the surgeon should act the part of the will-power of the patient. One should adopt a method to cause the patient to approximate the vocal cords and keep them in that position while the expiratory air passes through. If sound can be procured in that way, it will be a basis on which to tell the patient he can be cured. The simplest method is to hold the tongue as when examining in the ordinary way, and with the pharyngeal mirror rubbing up and down on the pharyngeal wall, so producing considerable glandular secretion, which drops into the larynx, causing a protective reflex, which keeps the cords together. If at the same time the patient is told to cough and finish the coughing effort with the "ee" sound, phonation will probably be induced. A pure neurosis case can be cured in that way inside three minutes, but not a chronic case with an associated catarrhal condition.

Mr. HERBERT TILLEY.

I want to bring forward a method I have found useful, but I do not claim that it is new. I have used it for stenoses of the trachea caused by bullet wounds or other injuries. The mechanism is an ordinary tracheotomy tube with a perforation about $\frac{1}{2}$ in. behind the collar, which admits a pliable and hollow lead tube. This extends upwards through the whole extent of the stenosis, and the calibre of the tube is enlarged as the stenosis becomes dilated. The tracheotomy opening should be made well below the stenosis, and then the constricted portion opened, granulations and connective tissue carefully dissected away, and the tracheotomy tube with the lead dilator inserted. In this way the larynx is spared functional activity, and the lead plug can not only be taken out and cleaned, but it can be bent to fit the parts. The difficulty of knowing when the upper part of the lead tube is level with the upper part of the stenosis is overcome by direct laryngo-tracheoscopy. At the end of three weeks, the tube is taken out and replaced by a wider one. Complete dilatation may be a matter of two to four months, which one need not apologize for in this difficult kind of stricture. I am a great believer in doing a low tracheotomy, and well below the strictured parts. Mr. Harmer, quoting Kocher, says the higher the tracheotomy the better. My cases have done better with the lower opening: the patient is more comfortable and the after-treatment is easier. Where the larynx is involved and there is stenosis and ankylosis in the arytænoid region, I do not know any treatment which will cure. I think most of such cases will be condemned to wearing a permanent tracheotomy tube.

I have little to add concerning the neurotic cases, but I agree with Mr. O'Malley that much depends on the personality of the surgeon. If the latter shows or expresses any doubt about effecting a cure, he will probably fail with these cases. If you tell them they will be cured immediately, it is surprising in what a large number the voice is restored forthwith, and does not relapse.

Mr. C. E. Jones-Phillipson.

I have treated many cases of laryngeal and gas affections in the past two and a half years, and would like to say a word on conditions not spoken of in this paper, which lead to functional aphonia. One is when two sides of the larynx are not acting in co-ordination. I have seen this in six cases: The right vocal cord has approximated to the mid-line in attempted phonation, while the opposite cord has been in the position of full abduction, or at any rate is tucked away beneath the false cord. When this cord makes for adduction, the opposite cord swings out to the position of full abduction, so no voice is produced. Another condition shows extreme over-adduction of the cords with full expiratory effort, so no voice results, and the patient turns red in the face in the attempt. I have examined patients within twelve hours of "gassing," and seen them many months later. The first stage is one of acute irritation of the entire respiratory tract, but long before the acute hyperæmic stage the patient takes on a whispering voice, due largely to the irritative cough, which is the first symptom. The condition of the larynx and pharynx is more marked within the first forty-eight hours. At this time it is common to see a yellowish wash-leather-like slough on the pharyngeal wall, the inflammation from which spreads to the larynx, causing swelling of the arytænoid region; a red mass in that region is frequently seen coupled with a muco-purulent secretion. I maintain that the third stage is productive of three conditions. Often as the first sequel there is a chronic laryngitis, with the ordinary paretic conditions, of which the most common is the elliptical and double arytænoids. Secondly, the vocal cords may be quite normal, the acute condition having subsided, you find pale swellings in the arytænoid region. The third sequel is true functional aphonia, of which the paretic varieties are many. The vocal cords may be of normal colour, and the arytænoid swellings may have disappeared, but there is a paretic condition, which I have found easily curable in France, but difficult in cases in England, from which I gather that the cases sent home are either the worst or the most persistent types, or too many of these cases are crowded into one hospital, where they communicate with each other in whispers and never improve. I had a case of recurrent nerve paralysis of the left vocal cord, which was examined by a physician, and no skiagraphic evidence in the chest could be found to account for it. He remained in that condition one week, then, following the advice of Mr. O'Malley, I briskly rubbed the pharyngeal wall with the mirror, and he recovered completely. I found that the man had had hoarseness some months previously, and it had suddenly disappeared.

Mr. A. E. Hayward Pinch.

I have only had a few cases of gunshot wounds, and those have not been in my hands more than three months at the outside. To get away scar tissue in these laryngeal cases you must proceed with great caution, otherwise there will be an extensive reaction and great œdema of the glottis, possibly requiring the performance of tracheotomy. In some cases nerve may be involved in scar tissue. Heavily screened applicators have to be used, and a considerable time must elapse before beneficial results become evident. I can tell you of a case which may throw some light on the subject. In 1912, a lady singer was brought to me: she was of considerable professional eminence, aged 30. She developed a thyro-adenoma. An eminent surgeon removed it with great skill. All went well for two months, and then hoarseness developed, and gradually became very pronounced. A laryngologist found definite adductor paresis of the left vocal cord. The idea was that there was scar implication of the recurrent laryngeal on that side from the operation. Further operation was negatived, and the surgeon brought her to me to see what radium would do. The first thing which struck me was, that the scar of the operation was becoming keloidal, and I thought there might be excessive scar tissue implicating the laryngeal nerve. Radium applicators were used screened with 1 mm. of silver. I gave her a week's treatment, and she came back in two months' time with the hoarseness much less evident, and we thought there was slight return of movement of the left cord. We gave her the treatment on three more occasions, and she recovered her voice, and is again getting a handsome income as a singer. Mr. Harmer has sent me cases of gunshot wound of the larynx to treat with radium, but the greater number are extremely severe ones, and I doubt if I can do much for them. But in one of the cases there is a mass of scar tissue in the inter-arytænoid region, with incomplete adaptation of the cords, and as it is partly keloidal I hope to get a definite result. It is as yet too soon to make a definite pronouncement as to the effects of radium therapy in these cases.

Sir StClair Thomson.

Was the cord of the lady singer seen to move again? I have heard people sing who have had permanent paralysis of one vocal cord. [Mr. Pinch: The point has not been raised before, but I do know she is again singing well.] By adaptation and benefiting by teaching, these cases with an absolutely paralysed cord can go on the concert stage again. In connexion with Mr. Pinch's remarks, I suggest that the laryngologist should not be hasty in blaming surgeons for cutting the recurrent laryngeal nerve at operations for enlarged thyroid. I have seen this condition come on several weeks after operation. I think it occurs in cases which do not heal by first intention, but have suppurated, and the recurrent nerve becomes implicated in the contracted scar.

From such a wealth of material we ought to draw some general deductions. One of these is the great tendency of scars of the larynx to contract, such as is seen in the cases of web formation in the anterior commissure. In many instances I attribute this to the perichondritis which occurs and the subsequent contraction. The next general deduction is the importance of resting the larynx. We learn it not so much from these cases, as from what we learn elsewhere. There are a number of cases of lupus of the larynx which get well without any other treatment than putting in a tracheotomy tube which rests the larynx. That brings me to the point that in these cases tracheotomy is still our stand-by. Mr. Harmer says tracheotomy "may be necessary in any form of stenosis." I might almost say it is necessary in all forms, if we are agreed as to what stenosis is, for there are various degrees. My standard is, that if a patient has laryngeal stridor when he is at rest or asleep, he has got a stenosis which requires treatment. Several of the cases in the next room now have stenosis, but are going about without a tube: they do not notice the need. I am insistent not only on the need for tracheotomy, but that the tube should be inserted low enough, and that it should not be taken out too soon. The top of the tracheotomy tube should be just above the sternum. The first reason for that is, that it is a tremendous convenience for the patient: the lower down it is, the less is it subject to the various movements of the neck, and the friction and irritation in consequence. A tracheotomy tube low down does not irritate the trachea: these cases do not get tracheitis leading on to bronchitis and death. Again, it is easier for a lady to hide it there, and for a man also. Also, it is far away from the danger region—i.e., the subglottic. We know the rich lymphatic area below the vocal cords, which, if wounded, leads to contraction. When tracheotomy is done for tubercle, or for malignant disease, the farther away it is from the disease the less likely is infection of the wound—if it occur at all. I would only make one exception—the lower the tracheotomy tube is in the neck. the more difficult it is to get the dilator up. Mr. Harmer and I have discussed this point, and we agree that if we know there is stenosis in the larynx which can possibly be dilated later on from below, then there is a disadvantage in low tracheotomy. If we have a discussion on the subject some day, I could refer to an old lady aged 80 who had worn a tracheotomy tube for fifty years.

I am disappointed that in the paper and the discussion we have not heard more of the results of two other methods. One is laryngo-tracheostomy. I have no experience of it, but I would like to hear, from Mr. Harmer or others, what the results are, and whether it is worth adopting. The other point concerns intubation tubes. If I had known earlier in life of the work of Dr. Rogers and Dr. Delavan, of New York, I would have tried more to carry out their principle of long-continued intubation for these cases. In this country, we are apt to think that you can take out your intubation or tracheotomy tube soon. Several of the patients in the next room would find out they were starved for air if they were to rush for a train after eating a hearty dinner, or if they were to have a rough cross-Channel journey and were sick.

Another point concerns repair of the divided recurrent laryngeal nerve. I mention it as a warning. I have had only one case of it—namely, in a Dominion officer who had complete paralysis of one vocal cord from wound of his recurrent laryngeal nerve. I saw him in consultation, and told him he would have to put up with it for the rest of his life. A young surgeon, who has a reputation as a nerve surgeon, said he would cut down on it and tie the ends together. Asked if it was an easy job, he replied he was used to tying nerves together! I heard the usual sequel—he never got hold of the ends of the nerve. Paralysis of the recurrent laryngeal we all look upon as due to some irremovable condition, like aneurysm or malignant disease of the œsophagus, and it is only in recent years I have learned that it can be recovered from. My experience of this is chiefly in one walk of life, and that is seeing tuberculosis patients at a sanatorium. I have seen six or eight cases of distinct paralysis of one vocal cord from a lesion of the recurrent laryngeal, no doubt due to tubercular pleurisy, in which the movement of the cord has recovered.

Dr. Smurthwaite has given us great help by the review of his cases, and I should like to know more of his method in functional aphonia. I learned from him in private that the great thing is to get the patient alone and try to succeed at the first sitting: otherwise, as in operations for cancer, one may succeed with a first try, but rarely with a second.

Mr. MARK HOVELL.

As so much has been said about tracheotomy, and there are present to-day so many surgeons from military hospitals, I wish again to call attention to the great comfort to the patient arising from the insertion of a piece of elastic into the tapes which hold the tube: 3 in. of elastic on each side is sufficient. This arrangement was introduced by Sir Morell Mackenzie half a century ago, but is still comparatively unknown.

Dr. JOBSON HORNE.

The all-important question in the discussion on warfare neuroses of the larynx is, what is "functional aphonia"? The term is bad. In all these cases we must decide whether the aphonia is a neurosis due perhaps only to asthenia or not; we must exclude organic diseases of any kind, especially the aphonia due to an interstitial myositis, following upon a prolonged catarrhal condition, such as the cases which came from Flanders during the winter campaigns. And, before all, we must exclude the possibility, however remote, of tuberculosis. In the absence of organic disease, I do not think there is so much difficulty in bringing home to these people the fact that they can speak. If it is not a neurosis, then by trying to make the patient speak we are doing him definite harm.

Mr. W. Stuart-Low.

The case I have shown emphasizes the importance of low tracheotomy and also of leaving the tube in long enough. What I find useful, and what military men may also find so, in order to get rid of sepsis and to prevent and diminish cicatrization is to attend to the teeth by means of mouth-washes and to rub ammoniated mercury ointment into the neck regularly for weeks. Also, in acute cases where congestion and œdema of the larynx have occurred dry-cupping should be applied to the neck over the outside of the larynx, the position of the cups being changed from side to side. This man who came in such an acute condition was treated by the employment of the dry-cupping method. This treatment was described in "Dry-cupping in Laryngeal Affections," in the *Lancet*, January 23, 1915. By means of the laryngeal mirror one can see the diminution of the vascularity of the vocal cord and the swelling of the ventricular band and arytænoid processes visibly diminishing while the dry-cupping is being applied.

With regard to neurasthenic cases I am sure these men smoke too much. One of my soldier patients was smoking forty cigarettes a day. These I cut down to fifteen and finally to six, and then I let him out in the sun on condition that he did not smoke. Fresh air and sun are most beneficial to these neurasthenic cases, and basking in the sun together with tinc. nux vom. did this man much good, and greatly hastened his recovery. Tobacco dries up the larynx and pharynx, and is a source of harm to many patients, particularly neurasthenic cases.

Mr. Whale.

With regard to Mr. Stuart-Low's conclusions, I would like to criticize the first point, the advisability of letting shrapnel rest in the tissues. Of course Mr. Stuart-Low has got a very nice result, but I do not think it is safe to generalize from that. During the last twenty-one months I have been dealing, almost exclusively, with wounds above the clavicle in France, and many of the men have been wounded in the larynx. I cannot remember a case in which I, or anyone else working in that department, regretted opening the tissues widely; but I remember cases in which patients have died without being opened up, and would not probably have done so if we had acted differently. Cases in which small bodies enter high in the head or neck and finish low down seem to do well for a day or two, but then they have a high temperature and a running pulse with extraordinary fluctuations, and asymmetry of pupils, and die of abscess round the vagus, the process involving also the sympathetic. I do not think it is safe to leave foreign bodies in the neck at all. As in civil cases you get ulceration of the carotid sheath by leaving a tube in the neck three or four days, how can it be safe to leave shrapnel in that neighbourhood? It seems to me quite wrong.

Mr. MARK HOVELL.

I think it is an opportune occasion to call attention to the use of the term "functional aphonia." I do not want to carp at those who have used the term, as it is an accepted term, although obviously incorrect. The function of the voice is to produce sound, therefore want of sound cannot be functional. I suggest as an alternative the term "neurasthenic aphonia" to distinguish it from loss of voice due to obvious injury or a definite nerve lesion. The term being incorrect, I think it is for our Section to say so, and to bring about reform. The incorrect term is copied from text-book to text-book, and thus becomes of common use.

The PRESIDENT.

I wish in the first place to condemn any arrangement whereby patients suffering from neuroses of the larynx are placed under a medical man who cannot make laryngeal examination, or who, without such an examination, proceeds to carry out treatment. Dr. Jobson Horne's remarks lead me to say that many of our aphonic soldiers are not suffering from a functional neurosis but from the results of laryngitis caused by cold or gas, or from weakness, exhaustion, early tuberculosis, or organic paralysis, or they may be simulating.

During acute laryngitis, some patients whisper in order to conserve their voices, and after the inflammation has subsided the aphonia persists until they are taught how to overcome it. A partial knowledge of this fact is taken advantage of by a few men who pretend to be voiceless, and it is difficult to distinguish this condition from real aphonia: they usually capitulate, however, to faradism or painting the larynx.

Aphonia may also be due to a variety of laryngitis which has not been mentioned—viz., exudative or fibrinous. This is not a neurosis, but it can be classed as a warfare injury, for I have not seen it in civilian practice, whereas last year, after the cold weather, I had about a dozen cases. The vocal cords are more or less red, and on the middle of the ligamentous portion a white patch is present.

The action of gas on the larynx used to be of but little account, but the cases which come into hospital now suffering from the effects of mustard shells may present large areas of exudation on the posterior wall of the pharynx, and smaller patches in the larynx. On Wednesday I saw ten such cases. Most of them had conjunctivitis, and were sent from the eye department.

Reverting to neuroses of the larynx, I have been surprised that so little has been written or said in this country regarding spastic aphonia, which the war shows to be fairly common. Dr. Smurthwaite, Mr. O'Malley and another speaker have referred to this class of cases, without applying the term. [Dr. SMURTHWAITE: I called it tonic spasm.] In future, when speaking of hysterical or functional aphonia, it will be necessary to distinguish the paralytic from the spastic variety.

The laryngeal appearances in aphonia paralytica vary greatly, but too much attention should not be paid to what muscles are involved, for the same patient may, at different times, present an entirely different picture. It should also be remembered that all the appearances observed in functional aphonia may be produced by whispering: thus, in one person the glottis may assume the form of an isosceles triangle, in another it may be elliptical, and in another merely a triangular gap in the posterior third. Although the appearances are those of pareses, pareses as such do not exist, because on painting the larynx in most of these cases the cords come together, but on asking the patients afterwards to speak, they are still voiceless.

In spastic aphonia the picture during phonation varies: in the slight forms the anterior portions of the false cords meet; while in the most marked forms the epiglottis is depressed, the arytænoids drawn forwards, and no view is obtainable of the interior of the larynx. A common variety is that in which the anterior two-thirds of the false cords meet, and in the gap between them posteriorly a small portion of the true cords, also slightly apart, is seen. This condition of spastic aphonia explains the cases in which the chief or only feature is a triangular gap in the cartilaginous glottis—a case of the kind was shown at our last meeting by Dr. Banks-Davis.[1] It seems to me that the small triangular gap is not due, as is generally supposed, to paralysis of the transversus, but to the excessive action of the thyro-arytænoideus. When this muscle contracts strongly it draws the epiglottis and the arytænoid together, and this action prevents the full contraction of the transversus, hence the gap. It has been suggested that the excessive action of the false cords is to overcome the opening due to the imperfect action of the transversus, but if you work at a case you can get this opening closed completely and still the patient is aphonic.

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[1] *Proceedings*, p. 163.

X-ray examination of the diaphragm sometimes helps one to understand the nature of the disability. The excursion may be too small during respiration, or on phonation the diaphragm may rise very quickly, showing there is no resistance to the expired air, or it may not rise at all, showing that the glottis is firmly closed.

With regard to treatment, I divide patients with functional aphonia into two classes: (1) Those easily cured; (2) those cured with difficulty, or not at all. Most laryngologists have a favourite method of treatment which they try first, and only if that fails do they try another, but as the majority of cases are easily cured and yield to the measure first employed, while the remainder prove obstinate to all measures, each laryngologist comes to regard his pet method as the best. I do not think it matters what method is used, provided it be carried out with decision and perseverance. As a preliminary to the treatment of questionable characters it is useful to confine them to bed in a room by themselves or screened off; allow no visitors, tobacco, or literature, and there is no harm in occasionally forgetting a meal. I cause patients to intone, and in most cases a cure is effected at the first sitting. Faradism has only helped me when simulation was practised. General anæsthesia and massage have failed in my hands.

With regard to the treatment of stammering, I have nothing favourable to say. I had five patients at one time, and I got a highly experienced lady to give them treatment, but at the end of three months they were no better. I think that patients with shell-shock stammering are best left to themselves.

I have treated eleven mute patients: eight were cured completely, two stammered afterwards and one had to be discharged from the Army still mute. The last referred to wrote me later that he had recovered his speech after having been nearly run over.

Mr. CORTLANDT MACMAHON (a visitor who was asked by the President to speak as he had given many of these cases vocal education) said :—

My experience in shell-shock stammering has been largely at Palace Green and at St. Bartholomew's Hospital. I read a paper on the subject last year at the Medical Society of London, reported in vol. xxxix of the Society's *Transactions*. In a large number of these cases the prognosis is good, the patient soon regains a good speech, and it is the easiest class of case I know to treat, provided one can get past a certain stage. I have had one or two disappointments in cases which had been going on well until upset by air raids, and it became useless to try again until the patients had been sent away from London. A discharged soldier who arrived at St. Bartholomew's three months ago with stammering is back again in the Army. Shell-shock stammering seems to me to be best treated by counter-stimulation —namely, stimulating the abdominal viscera by the strong descent of the diaphragm, so that the blood supply to these organs is increased. I agree with Dr. Smurthwaite as to the importance of securing relaxation, mental and physical; when that is acquired work on the speech is commenced. If a man has been a stammerer in early youth, the condition may be resuscitated by shell shock. These cases take a long time to improve.

DR. SCANES SPICER.

I cannot think Mr. MacMahon's views as stated are quite consistent with each other in that he says he aims at inferior costal respiration and yet seeks descent of the diaphragm on inspiration. I believe what he really does in practice is to raise and fix the abdominal viscera by the muscles of the abdominal wall (recti, obliqui, &c.) so that these viscera form a fulcrum for the centrifugal action of the diaphragm, quadrati lumborum, &c., in expanding the lower margin of the thoracic cage— which is inferior costal inspiration. Then, on expiration, there is a conscious contraction or willed intensification of action of the abdominal walls which compresses the abdominal viscera, tends to empty the portal system and so forces on the blood to the right ventricle and later on to the brain. Hence there is a *suction-pump* action of the expanding thorax in inspiration, and a *force-pump* action of the abdominal muscles on expiration, both acting in the same sense of promoting the general blood circulation and hence the blood supply to the brain, including the higher co-ordinating centres which in functional aphonia and stammering are *hors de combat*.

I further believe—a matter no one to-day has referred to—that an essential preliminary to rapid and lasting success in overcoming the neurasthenia from which these mute and stammering men suffer is the setting up of the human machine *by posture*, so that it can and does act to maximum advantage as a machine, and that all breathing and other physical exercises and training are done in and from that posture.

This is effected, *in the sitting posture*, by consciously straightening up and stretching up the spinal column to the fullest extent. The effect of this is the enlargement of the thoracic cage and its being brought into a state of easy poise and free mobility, and to replace the collapsed and sagging viscera of the thorax, abdomen, &c., into their normal position and natural relations.

Neurasthenics are practically all in a state of physical collapse as well as mental weakness. Their visceral organs are (relatively to the normal) in a condition of ptosis, they are compressed and congested with the functioning tissues, channels, vessels, ducts and nerve tracts obstructed from the abnormal positions and altered relative pressures of the organs, &c., upon each other. Hence the ideas of "Hold up your head," "Straighten and stretch up your back," and "Sursum corda" are essential preliminaries to treatment. The patient must consciously adopt these ideas and carry them out in his own body, and the breathing and other physical exercises should always be done in and from this posture, at first sitting on a stool of the appropriate height. Later he can commence any voice or speech training indicated with the usual "ah" sound both phonated and whispered, but his attention should always be kept off his larynx. The rapidity, progress and permanency of recovery were often quite remarkable following these measures, not only in voice and speech but generally in neurasthenia and shock.

I do not share Mr. Mark Hovell's objection to the term "functional aphonia," since it merely serves to contrast and define aphonia due to some derangement of function in centre or nerves, as opposed to aphonia due to structural disorders of the vocal mechanism, so I think the term may well be retained.

Mr. Harmer (in reply).

Paralysis of a motor nerve may be due to division, involvement in scar tissue, to toxæmia, to shock. The neurologist apparently recognizes shock, by which I understand him to mean a condition where there is nothing demonstrable in the nerve itself afterwards. Mr. Tilley said I had referred to Kocher as having said a low tracheotomy is more dangerous than a high tracheotomy. He read more meaning into that than I had intended in my paper. Kocher was pointing out that in the accidents after tracheotomy, for whatever it was performed, there is more danger with low tracheotomy than with high, and he quoted a number of cases in which hæmorrhage had occurred from large vessels at the root of the neck. I found that this was correct when I was writing an article for Burghard's "Operative Surgery." But tracheotomies for warfare injuries are often performed when inflammation has subsided, as deliberate operations, and are therefore unlikely to be attended with the same risks as in the class of case he was describing.

With regard to Sir StClair Thomson's complaint that evidence has not been brought forward as to the comparative results of laryngo-tracheotomy and intubation, I am disappointed myself that I have not been able to obtain enough evidence to be able to lay down any general rules. I think there is a tendency to perform laryngofissure too often and too early. It must be wrong to take a man who is suffering from acute laryngitis, perichondritis, &c., and perform a laryngofissure without giving the inflammation time to subside. There is evidence in the cases that I have quoted to show that well-known laryngologists have said, "This man will never be able to live without a tracheotomy tube unless laryngofissure is performed," and yet a year or two later the stenosis of the larynx had disappeared. These were cases not of scar tissue which had contracted but of a general œdematous infiltration of the tissues, which later disappeared entirely. In many cases of scarring I am still doubtful whether you cannot obtain the same results by intubation, if it be carried out for long periods. There remain the very severe cases such as Moure classes as circular stenosis, which I think nothing but an external operation will cure. Sir StClair Thomson also asked me why I said that tracheotomy may be necessary in any form of stenosis. I meant stenosis in that sense to apply to recent inflammations and paralytic forms in which, by waiting a little time, it may be possible to avoid the performance of tracheotomy.

Dr. H. Smurthwaite (in reply).

With regard to Mr. Mark Hovell's remarks in connexion with the term "functional aphonia," if it is thought necessary to change it I would suggest "psychogenic aphonia" for this class of case, thus indicating that the loss of voice has a psychic origin.

In answer to Dr. Permewan I have not found it necessary to use electrical treatment. On the other hand I have restored the voice in several who have had electrical treatment previously for some weeks before coming under me.

In answer to Dr. Jobson Horne, I may say that I made special reference to the necessity of accuracy in diagnosis in these cases; that the aphonia might be due to a myopathic paresis of the cord muscles—a harbinger of early tuberculosis. In fact I have sifted out quite a number of tubercular lung patients from among the aphonia cases sent to me.

In reply to Sir StClair Thomson, I am sorry that in trying to cut my paper short while reading it I should have skipped the part where I refer to treatment. In the first place I rely to the greatest extent on the moral effect my words will have on the patient. I convince him that he really has no disease in his throat, and that without a shadow of doubt he will leave the room with his voice restored. I then tell him to breathe deeply several times and at the end of a deep inspiration to hold his breath and cough. This will, as a rule, elicit some sort of note. Next I place the laryngoscope in the pharynx and tell him to say "ah," and in many cases I am able to get the normal vowel sound, purely by suggestion and from the fact that his mind has already been prepared and he is expectant. If this fails I make him cough forcibly, gradually getting him to sound the vowel "ah," and then "oh." Following this he must cough the numbers, one, two, &c., and by degrees I succeed in making him say the numbers without the cough. I also aid his expiratory effort by compression with my hands on the lower ribs. This forced expiratory effort on the patient's part is practically that carried out by Kaufmann, which he described in a paper in 1916 in the *Münchener med. Wochenschrift*.

To some of the most difficult cases I have given much time and trouble; often as much as three-quarters of an hour have I battled with a single case.

Finally, I will say that everything depends on the mental force I can put into it, and for this reason I am only really successful when I am not too tired and can bring the whole force of my will and energy to bear on the case.

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(*November* 27, 1917.)

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Serum Disease after Intrathecal Injections of Serum.

By H. D. ROLLESTON, C.B., M.D.

(ABSTRACT.)

[This paper is printed *in extenso* in the *Lancet*, December 1, 1917, p. 821.]

(1) SYMPTOMS of meningeal irritation may precede or accompany the appearance of the serum rash. (2) Though serum disease is frequent after intrathecal injection of serum, especially after Flexner's antimeningitis serum, it is not clear that it is more frequent than after hypodermic injection of serum. (3) Intrathecal injection of serum necessarily involves risk of inducing increased intracranial pressure and may introduce a secondary infection; but it is not proved that the volume of serum injection favours a secondary infection of the meninges from the blood stream.

(*November* 27, 1917.)

A Note on Serum Sickness in Cerebrospinal Meningitis.

By CLAUDE B. KER, M.D.

(ABSTRACT.)

[This paper is printed *in extenso* in the *Lancet*, December 1, 1917, p. 822.]

SEVERE reactions occurred in no less than 77 per cent. of cases of meningitis, treated with Flexner's serum, which lived for nine days or longer. The intrathecal injection of serum, however, must not be regarded as more liable to cause serum rashes than the subcutaneous method, and an undue percentage of serum reactions must be attributed to peculiarities in the horses used to supply the serum. It is important to distinguish the prodromal fever which often precedes these rashes, and the meningeal symptoms which may accompany it, from a recrudescence of the specific infection. The serum reactions, although often very severe, appear on the whole to be beneficial to the patient. The most usual time for their occurrence was the ninth day following the first injection.

DISCUSSION.

Dr. J. D. ROLLESTON: My experience of the serum disease is mainly derived from a study of cases treated with diphtheria antitoxin. I pointed out in my paper, to which reference has been made, that the frequency and severity of serum phenomena are in direct relation to the size of the dose, and in inverse relation to the severity of the attack of diphtheria. I therefore attributed considerable prognostic value to a good development of the serum manifestations, especially the later ones, and compared the absence or deficiency of a serum reaction in fatal cases to maldevelopment of the pustules and an absence of reaction in the surrounding skin in malignant cases of small-pox. The occurrence of meningeal symptoms following intrathecal injections in cerebrospinal fever finds an interesting parallel in the occasional return of the throat symptoms in diphtheria at the time of the serum phenomena. White patches may appear on the tonsils constituting an enanthem corresponding to the cutaneous exanthem. This phenomenon may be mistaken for a relapse of diphtheria, and I have known a medical man not familiar with the condition unnecessarily re-inject such a case.

Dr. O'BRIEN: The occurrence of rashes and other symptoms of serum disease is probably due to several factors. It seems to me that dosage—i.e., the volume of serum injected, must be a very important one. One is justified, from the figures given to-day, in assuming that the percentage of rashes following the injection of serum remains about the same whether the serum be given intrathecally or otherwise. I am hoping that the Registrars of some of the large military hospitals will publish statistics, but I believe that rashes do not occur in more than about 3 per cent. of men injected with the prophylactic dose of 500 units (U.S.A.) of tetanus antitoxin, which is usually contained in about 2 or 3 c.c. The smallness of this percentage, when compared with the 30 to 60 per cent. recorded in long series of subcutaneous injections, must be, I think, connected with the smallness of the amount of serum injected. If, as seems almost certain, the use of concentrated serum is followed by a great reduction in the occurrence of these rashes, then we may look forward to the day when all serum used will be concentrated.

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Section of Neurology.

President — R. PERCY SMITH, M.D.

(*October* 25, 1917.)

PRESIDENT'S ADDRESS.

Aphasia in Relation to Mental Disease.

By R. PERCY SMITH, M.D.

MY first duty is to express my thanks to the members of the Section for having done me the honour to elect me as its President for the ensuing year, an honour which I felt bound to accept, although it involved the burden of a Presidential Address, in addition to one a year ago to the Section of Psychiatry, of which I am still President. Perhaps the accident that I was for some few years the editor of *Brain*, which was at that time the journal of the Neurological Society, in succession to Dr. de Watteville, induced the Section to place me in this chair, to hold which however I feel myself unworthy in presence of and in succession to so many distinguished neurologists.

To one whose work has lain for so many years in the domain of psychiatry the choice of a subject for a Presidential Address to this Section has seemed somewhat difficult, but it appeared to me that my best course was to search through my case-books for cases which might be of interest both to the neurologist and the alienist and lie in the borderland between the practices of the two, and as to which either of them may be consulted by the general practitioner. In this way many cases of disease of the nervous system, where there has been more or less pronounced mental disorder, have come before me.

It has seemed to me that those cases in which there has been aphasia more or less pronounced whether with or without hemiplegia (apart from cases of general paralysis where it has been an occasional symptom) and in which I have been consulted as to the patients' mental condition would be the most likely to be of interest to the Section.

The subject of aphasia has of course a voluminous literature, both from the side of neurology and of psychiatry, and with regard to this I think we owe an eternal debt of gratitude to Dr. Henry Head, the present editor of *Brain*, and our senior Vice-President, for having in vol. xxxviii of that journal reprinted many of the important papers of our great master Hughlings Jackson, and so prevented them from passing into obscurity, and for having in his paper, "Hughlings Jackson on Aphasia and Kindred Affections of Speech," given so admirable a summary of the views and conclusions of that great English neurologist. I may also mention the valuable reviews and summaries given by James Collier (*Brain*, 1908, xxxi, p. 523) and by S. A. K. Wilson (*Review of Neurology and Psychiatry*, 1909, vii, p. 151) on the subject.

As recently as 1915 Head writes: "Speech is a function of mental activity and however much that mental activity may ultimately be linked up with the integrity of some portion of the brain substance the problem is primarily a psychological one," and again, "no one but Hughlings Jackson has recognized that all the phenomena are primarily psychical and only in the second place susceptible of physiological or anatomical explanation" (loc. cit., p. 4).

Therefore no apology is needed for examining those cases of aphasia which come under the notice of the alienist. In any particular case it is important to ascertain whether there has been mental disorder of any kind preceding more definite affection of the speech mechanism as well as to see in what way cases beginning with aphasia are associated with mental disorder. There frequently arises also in any of these cases the question of business or testamentary capacity.

Although during my tenure of office at Bethlem Hospital a few cases of aphasia associated with certifiable insanity were admitted, in some of which an autopsy was obtainable, the larger number of cases which I have met with in consulting practice have only been seen clinically, and there has been no opportunity of ascertaining how far the affection of speech corresponded with any particular pathological condition of the brain. With regard to this however I may again quote Head's remarks in reference to Hughlings Jackson's views: "But no one has assimilated his views on defect of speech and applied them to a series of actual cases of this condition. We failed to appreciate how much closer these conceptions would lead us to the phenomena of aphasia than the glib generalities founded on the anatomical facts of cortical localization." And again, "Neurology has become frozen stiffly in the grip of pseudo-metaphorical classifications which neither explain the condition nor correspond to the clinical facts" (loc. cit., p. 3).

Hughlings Jackson has said, "We shall do no harm to clinical medicine, if we simply record all the facts" (*Brain*, 1915, xxxviii, p. 37). I hope, therefore, that the absence of pathological findings in this paper may not render this communication entirely worthless.

The alienist may be called in consultation for the following reasons in cases where there is aphasia:—

(1) Because of the confused or incomprehensible speech of the patient and other conduct suggesting confusion or disorder of mind, the nature of the speech affection being misunderstood by his relatives and not always recognized by the practitioner. This is especially so in those cases where there is no hemiplegia associated with the aphasia, but also occasionally in cases where there has been a previous transient hemiplegia or even some slight right-sided paralytic signs or unrecognized hemianopia, so that in taking the history one may be confronted by such descriptions as "talks incessantly, uses the same word again and again, shouts at times and shows excitement"; "on waking up was unable to speak, did not know anything, and was mumbling"; "could not find words, was worried and excited, then violent and resistive"; "was light-headed, could not see or read properly"; "makes inarticulate noises"; "talked babble, emotional and angry, but no loss of consciousness"; "talked gibberish, cannot put six words together"; "said to be 'mad' and could not speak properly"; "was brought back home and did not know how to eat, was thought to be intoxicated"; "speech incoherent"; "said to be childish and incompetent and imbecile": "emotional and confused, unable to read, does not ask for anything or propose anything"; "speech inarticulate"; "found walking about in his office unable to speak, then talked gibberish." Sometimes such patients are found wandering in the street, unable to give any account of themselves and are regarded at first as being demented. Brissot calls attention to the various speech disorders met with in insanity, which require careful differentiation from true aphasia of organic origin.

(2) Because of definite signs of mental disorder predominating over the aphasic speech troubles. Previous attacks of insanity may have occurred and been recovered from and the attack of aphasia may occur in association with a return of the previous symptoms or be masked by loss of memory, mental confusion or apraxia. As Brissot says ("L'aphasie dans ses rapports avec la démence et les vésanies," Paris, G. Steinheil, 1910): "Many aphasics are met with in asylums whose internment is justified by demential or vesanic troubles." Ideas of persecution (sometimes justified) may be met with, or temporary excitement, delusions and hallucinations, coming on immediately after a "stroke." During my residence at Bethlem Hospital some eleven patients who had either previously been aphasic, or were so at the time of admission, were admitted under certificates, and many such may be met with in the wards of county asylums.

(3) To decide on the proper care and treatment of patients in whom aphasia of various degrees of severity is associated with disorder of mind and conduct. Many of these of course can be treated at home, if the means and accommodation allow of proper nursing and supervision, but, as Savage has often said, "the treatment of insanity frequently depends on the length of the purse," and the infirmary wards of county asylums afford better care in such cases in the poorer classes than can be met with in any but the best of the workhouse infirmaries.

(4) To assist in forming a prognosis as to mental recovery or otherwise, or in the diagnosis from other organic conditions, such as general paralysis.

(5) To give an opinion as to the business or testamentary capacity of a patient suffering from aphasia, or occasionally to assist in the solution of the question of responsibility of an aphasic in criminal cases.

(6) Occasionally it has happened to me to be consulted by a patient with manifest aphasia, about his or her own mental condition, as to why there was difficulty in writing letters, and in getting "command of words," and what was the outlook for the future.

In all some forty-five cases of varying degrees of severity of aphasia and mental disorder have come under my notice in the last thirty years.

Dejerine ("Sémiologie des affections du système nerveux," 1914) points out that the degree of change of intelligence in cases of aphasia depends on (1) the extent and intensity of the lesion, (2) its reaction on neighbouring regions, (3) on the state of the vessels, (4) on the condition of the circulation and kidneys, (5) especially on the age of the patient, and says: "Il ne faut pas oublier qu'un aphasique peut devenir dément, de même qu'un dément peut devenir aphasique."

It will be well to give statistical particulars of my cases as to the age of the patient, the condition of the heart, vessels and kidneys, the presence or absence of definite hemiplegia and also as to heredity, previous attacks of insanity, and history of syphilis or alcohol.

(1) *Age.*—The average age of the male patients was 57·6. But excluding syphilitic cases, four of whom were under 40 years of age, the average age was 62. The average age in the cases with a history of syphilis was 47, showing the much more detrimental effect of this poison on the arterial supply of the brain than mere senile or presenile degenerative changes. The average age in women was 62, and there were no syphilitic cases. In each sex the average age was somewhat higher in those cases seen in consultation than in those found at Bethlem Hospital.

(2) Definite heart disease, either valvular or degenerative, was found in 26 per cent. of the men and 22 per cent. of the women.

(3) Renal disease was found in 26 per cent. of the men and 11 per cent. of the women, and there was glycosuria in one of each sex.

(4) Arteriosclerosis was found in 29·6 per cent. of the men and nearly 17 per cent. of the women.

(5) A history of alcoholism was found in 26 per cent. of the men and nearly 17 per cent. of the women. In many cases several of these factors were combined.

(6) Definite hemiplegia more or less pronounced was found in nineteen out of twenty-seven cases in men. In all but one of these cases the hemiplegia was on the right side at the time of observation, and in that one case the patient, who was syphilitic and was under care in Bethlem Hospital for acute mental excitement, suffered from seizures, with left hemiplegia and anæsthesia ending in coma and death. There was however a history of a former attack of right hemiplegia and aphasia which had ended in recovery. Post mortem there was found obliteration of the right middle cerebral artery by syphilitic arteritis with recent softening of the area supplied by it, and also old syphilitic arteritis of the left middle cerebral with an old cavity in the left internal capsule accounting for the former attack. Ten women out of eighteen had right hemiplegia either early or late in the case, and one

of them had had a previous attack of left hemiplegia. In no case was left hemiplegia immediately associated with aphasia.

(7) A family history of insanity, neuroses or alcoholism was found in 33 per cent. of the men and 39 per cent. of the women. In one case the patient's brother and sister had both died of right hemiplegia with aphasia.

(8) Previous attacks of insanity which had passed off had been present in three cases, but in many mental disorder or failure was present for some time before the onset of definite aphasia.

The cases which have come under my notice seem to me to be divisible mainly into four groups, viz.:—

(1) Those in which dementia, or mental disorder or failure sometimes amounting to certifiable insanity, preceded the more definite and classical affection of speech designated as aphasia.

(2) Those in which considerable mental failure was concurrent with or subsequent to an attack of aphasia. In severe and fatal cases the extreme mental dissolution of coma is seen.

(3) A third group in which, although there is severe affection of speech, the patient possesses such a degree of mental capacity as to permit of business or testamentary capacity.

(4) Cases in which there is some slight hampering of speech with very little mental disorder, although some may be present.

With regard to the *first* group it will be manifest that a patient who has already become disordered in mind is likely to have his mental capacity further deteriorated by the additional weight of an attack of aphasia, though this result does not always follow.

Hughlings Jackson has well said: "To speak is not simply to utter words, it is to propositionize:" "the unit of speech is a proposition." And again: "Loss of speech is, therefore, the loss of power to propositionize. It is not only loss of power to propositionize aloud (to talk), but to propositionize either internally or externally, and it may exist when the patient remains able to utter some few words" (*Brain*, 1915, xxxviii, pp. 113, 114).

If therefore his mind has first failed and his ideas and propositions have become morbid ones or there has been such defect of memory that recent events are not recorded and the patient lives in the past, as in many senile cases, it will be evident that as he has been "lame in his thinking" before the occurrence of definite aphasia, the lameness of thought will tend to be worse afterwards. The addition of "inferior speech" and "inferior comprehension" makes the ruin more complete.

To quote Head (*Brain*, 1915, xxxviii, p. 23): "Suppose, however, 'imperception' is added to the defect of speech, the formation of images, arbitrary symbols and those unconscious processes which precede their development will be disturbed. The 'general intelligence' will then appear to suffer greatly; for the mind will be struck, not only on its emissory, but also on its receptive side."

In this first group "imperception" has in many cases preceded the defect of speech and the "general intelligence" has already suffered.

I have already pointed out that excluding syphilitic cases the average age of patients has reached the seventh decade of life and that cardiac, arterial or renal changes are frequent, therefore it may be safely assumed that there is commonly in these cases some degenerative change in the cortex or other tissues of the brain with deficient blood supply, the occurrence of aphasia marking a more definite pathological change in some part of the speech areas of the cortex. This complication naturally increases the gravity of the prognosis so far as life is concerned, and such cases frequently die of cerebral hæmorrhage or softening.

A few selected cases are given:—

(*a*) Mrs. R., widow, aged 60, seen May 25, 1908. No heredity, no history of alcohol. For seven years memory had progressively failed so that it was said to have become blank. Two months before I saw her she had had seizures with loss of consciousness and stertorous breathing after which she appeared not to recognize her children and lost control over the bladder. She was said to be "incoherent and not able to put six words together." When I saw her she had no hemiplegia, she could not express herself, constantly using wrong words in trying to answer questions. She could not name objects, and could not tell the time by a watch. She was word-blind, and could not read even her own name, or do what was written, and could not write. She was not however word-deaf, but did simple things she was told to do, and repeated words she heard. She was certainly deficient both in internal and external speech and very "lame in thinking."

(*b*) Mrs. L., a widow, aged 78, first seen in August, 1902. Father insane, sister had senile dementia, brother insane, two cousins insane. For some years there had been failure of recent memory; for one year delusions, followed by excitement and confusion, with delusions of poisoning and of her son being arrested. She mistook the identity of people, for instance mistook her daughter for her own sister, spoke of her husband, who had long been dead, as being alive, then gradually became more childish and demented and lost control over the bladder. In 1905 she had a seizure followed by right hemiplegia and loss of speech. She was unable to frame words, but understood such simple orders as to put her tongue out. On one occasion however an "occasional utterance" took place under emotional stress. An enema was being given with some difficulty and a nurse told her not to worry, when she suddenly said "I will worry." Apart from this there was absence of external speech and no test for reading or writing could be made in consequence of the profound dementia. Death followed very shortly.

(*c*) P., aged 63, an accountant, who had been pensioned five years before, in consequence of failing memory and confusion of ideas, by the railway company in whose employment he had been. There was a doubtful history of alcohol and his arteries were thickened. For nearly a year he had become much worse mentally, was disorientated as to time and locality, did not recognize his own house, had forgotten his age, talked chiefly of his boyhood, thought he was still employed by the railway company, had been threatening violence to his wife and others, was dirty in habits and apraxic in dressing. On examination he was found to have slight paralysis of the right side of the face, but no other paralysis. He could talk in a hesitating way, but could not give the name of the town or road in which he lived, could not give his son's name, could not name objects—e.g., "glove," "watch," or "pen"—but recognized the names when spoken; he could not write his name correctly and had not written a letter for some months. He could not say what was the use of a pen, but when asked what I was doing said "writing." He could read print but did not understand what he had read.

The association of apraxia in dressing and writing with some aphasia, but without hemiplegia, is interesting, and will be referred to later.

(*d*) Mrs. B., aged 48, no heredity, had lived in the Tropics, and had suffered from malaria and dengue fever. There was a history of frequent "whisky and soda," and she acknowledged a craving for it. For some months she had become irritable, she had ceased to write letters, found everything an effort, was said to be talkative and muddled and to forget where she had put things, recent memory had failed and she never knew the date or day of the week. She was advised to return to England and on the voyage was alarmed by an impending attack by torpedo. When seen on July 27, 1917, her recent memory was found to be bad, but remote memory good, she could not name the month or day. She recognized her failure of memory and craving for alcohol, sleep was defective and she dreamed of standing by the boats when the torpedo attack was impending. Heart, lungs and urine were normal, knee-jerks were found to be absent without any other signs of tabes, and there was some tenderness of the muscles of the legs. Catamenia were irregular (impending climacteric). Speech was then normal. She appeared to be a case of alcoholism with some failure of memory and alcoholic neuritis. She had never had a fit. She was advised against alcohol and against returning to the

Tropics, which she had a great desire to do when her husband went back in the autumn. Four days afterwards, July 31, she had a seizure in which she was generally convulsed, bit her tongue badly, passed water and motion and was unconscious. When seen again on August 2, in consultation with Dr. Friend, she had recovered consciousness and had no paralysis on either side of the face. Extensor plantar response was found on both sides, but knee-jerks were still absent. There was no ocular paralysis, pupils reacted normally, and optic disks were normal. She was however aphasic. She talked a great deal but her conversation was generally quite irrelevant, and she could not ask for anything or give any account of her symptoms. When asked where she had seen me she said, "I am getting nearer, I shall get old and die nearer and will die in the streets (? Straits), I am getting old, I am getting in the streets soon, I will have to 47, will die in the streets, I am getting tired and cross and nearer 80, soon nearer 97." Then again she said, "Somebody said, never soon die in the streets one day nearer 85 soon." When asked to do so she at once put out her tongue, and it was protruded straight, but was badly bitten on the right side. After being asked several times, "Which is Dr. Smith?" she pointed and said, "It is you." Then she went back at once to her recurring utterances about dying soon. When asked if her tongue was sore she kept rubbing it on the right side and after being asked several times, "Is it sore?" said distinctly "Yes." When asked if she had slept the night before, she said, "Oh yesterday will soon die on Saturday," then again, "I am getting old and cross and stout nearer 50," "it was on Sunday morning will soon die all nearer 80." When asked if she had headache, she did not answer for a long time and then said "No." Her answers "Yes" and "No" apparently had propositional value. She could not give the address of the house, kept on saying "nearer 86." She could not read or recognize letters, could not name objects—e.g., watch, knife, &c.—but laughed at the suggestion that these objects were a toothbrush or pencil. She recognized the word "watch" when correctly applied, but could not tell the time. She could not name or count fingers. She could not write. She got out of bed when told to do so for testing knee-jerks and gait, but was rather slow in understanding what was wanted and all the time kept on with the recurring utterance. She seemed to be word-blind but not word-deaf. Examination of the blood showed a negative Wassermann reaction. On August 23 the aphasia had passed off with the exception of some difficulty in remembering names; she had a very indistinct memory of the attack or of seeing me and Dr. Friend; her memory for dates was still very bad but she had written a letter and could read. The attack having passed off so quickly, there was no apparent increase of mental failure, but she was evidently in need of nursing supervision. It is very likely, as Hughlings Jackson suggested, that the recurring utterances referred, however imperfectly, to what she had been thinking or discussing about her case immediately before she had the seizure.

The *second* group of cases—viz., those in which considerable mental failure is concurrent with or consequent on an attack of aphasia—contains many examples of the different ways in which aphasia may manifest itself, and here I make no claim as it were to "pigeon-hole" the cases according to the various speech-centres which have been described in works on aphasia. As Collier says: ("Recent Work on Aphasia," *Brain*, 1908, xxxi, p. 539): "Recorded cases show every degree both of severity and permanence, and they give no means of clinical distinction between cases claimed as examples of Broca's aphasia and of Wernicke's aphasia respectively."

The following are some examples from my case-books:—

(*a*) Mrs. S., aged 67, widow, seen November 27, 1902. Sister and daughter had been insane. Her urine contained albumin and some sugar. On November 23 she complained of headache, and the next day "could not find words," was worried and excited and repeated the word "come," possibly a recurring utterance due to a feeling of need for help when the attack began. She became violent and resistive, especially after visits by relatives, who considered that she must have "something on her mind" to account for her conduct. When seen she had no hemiplegia. She took a long time to understand what was said, but did simple things such as putting out her tongue when asked. She used words in a wrong sense, saying "upstairs" instead of "downstairs." Speaking of herself she said "she is very bad." Some of her utterances had a propositional value, for instance she said to the doctor, "I don't want you; go away." She had other ejaculatory utterances such as, "Albert wants to get to get," and "I don't want it," which were incomprehensible to others. She could not find the word "key" when she wanted to open a box, but called it "linen," then took out some securities, but could not explain what she wanted to do with them. She could not read or write. She remained mentally enfeebled and unfit to manage herself or her affairs, and died four years later without any definite hemiplegia.

(*b*) Mrs. D., aged 73, widow, seen November 23, 1904. Six years before she was said to be deaf, possibly there was some word-deafness at first, but there was no deafness when I saw her. She had begun to miscall objects and gradually lost speech, being able only to make inarticulate noises. Her friends had to stop her from going to church on account of these noises. There was no definite seizure or hemiplegia at the outset. She refused to spend money, and was said to have the delusion of poverty. She became unable to care for herself and needed constant supervision. When I saw her she could not speak at all and did not try to, but occasionally made an inarticulate noise. She understood what was said, and at once got her daughter's photograph when her name was mentioned. She could read what was written, and did what she was in this way instructed to do. She could write, but expressed herself wrongly, though the sense of it could be made out; for instance, she had written to her nurse, "I will wash her hair," meaning her own. In answer to my questions as to how she occupied herself and what her age was she wrote, "I am read papers, you are 73." She wrote firmly and quickly in answer to written questions, but always using wrong expressions or pronouns. The proper names of relatives were given correctly. She had no paralysis. The chief defect seemed to be in the motor, or emissory side, as there was no word-deafness or word-blindness. She gradually failed, and died the next year, but no autopsy was made.

There was no doubt in this case of the presence of "internal speech," as shown by her ability to express her thoughts in writing, although there was some defect in this.

(*c*) S., aged 58, widower, seen May 31, 1911. Father died of apoplexy, mother of cancer. He had an enlarged and irregularly acting heart. Eighteen months before he had suffered suddenly from loss of speech while staying in an hotel, and since then had lost business capacity, so that his business failed, and had to be wound up. There was no hemiplegia. He had no energy, had lost control over his bladder, was apraxic in dressing and feeding himself, and speech was said to be "incoherent." On examination he was very conscious of his speech defect, recognized that he made mistakes in words, and had lost bladder control, and wept about it. He could understand everything said, but answered confusedly, could not always name objects, but knew their uses, for example called a watch "timepiece." After naming "penknife" there was marked preservation of idea, all subsequent objects shown being called "penknife." In attempting to write he was quite unable to finish words.

This was again a case in which aphasia, apraxia and agraphia were associated, without hemiplegia.

(*d*) B., aged 83, married twice, had eleven children, had been a hard-headed business man, and was described as a *bon viveur*, and always full of energy. Until five years before he had ridden regularly, but then broke his leg, and ceased to take active exercise. Two years before he had had pneumonia, and since then he had shown signs of cardiac degeneration. For one year he had begun to lose words and to lose his memory. His speech became progressively worse, he was emotional and violent if opposed. He was disorientated in time and place, would get up in the middle of the night and mistake time, would insist on going to the City, but did nothing when there, and on returning could not always recognize his house. He lost control in cleanliness. When first seen on July 12, 1917, he could slowly understand what to do when told—e.g., to put out his tongue, put his hand on his head, get out of bed and walk round the room. He imitated movements. He tried to talk, but his speech was generally incomprehensible. He could not name a watch or other things, could not tell the time, could not give his address or the name of the road, but recognized names of objects when repeated to him. It was interesting that although he had lived 74 years in this country he could not make sentences in English. He gave his first name in German, and made some attempt to answer in German. For instance, when asked if he knew me he said, "nie gesehen." He could say "Yes" in answer to some questions, but it was often irrelevant and of no propositional value. With regard to his attempt to answer in German, I subsequently learned from his son that the first nine years of his life had been spent in Germany. He was apraxic in various ways, especially in fastening his clothes, and when an attempt was made to get him to write, he did not seem to know the use of the pencil, holding it upside down or letting it drop. He could not write and could not read. He was undoubtedly word-blind, but not word-deaf. There was no hemiplegia, his tongue was not well protruded, but there were no other bulbar symptoms. A fortnight later he was more confused and silly, did not seem to understand so much, was vacant, and had been noisy and violent at times; he was also more apraxic. He said much less, but still tried to use German, and when asked if it was a cold day (it was really very hot) said "ein Bischen." On August 3 he made inarticulate noises, did not try to speak, was drowsy and confused, failed to recognize people, had no control over emunctories, and had pulmonary congestion with rise of temperature. There was still no definite paralysis. He died in a few days.

It will be noted that in this case also there was apraxia and agraphia with no definite hemiplegia.

(*e*) Mrs. M., aged 81, widow, seen October 3, 1904. There was a history of chronic arthritis, of phlebitis ten years before, and of a "threatening of a fit" at the same time. As a child she had lived in Italy, and then habitually talked Italian and French more than English. Since marriage at the age of 17 she had lived in England. On August 5, 1904, she had an attack or seizure in which she was said not to have lost consciousness but to have "talked babble," and was emotional and angry. When seen she appeared to understand what was said, but screamed when others did not understand her, she talked volubly, but could not frame intelligible sentences which contained many adjectives but no nouns. It was interesting that she spoke Italian and French but no English at this time, the law of dissolution holding good as in the previous case. She could not name objects in any language, but recognized wrong names. She was completely word-blind, could not read or do what was written, and could not write spontaneously or copy. There was at first no hemiplegia, but later weakness of the right side with deviation of the head and eyes and inability to stand developed. Some improvement followed for a time, in which she became more intelligible, and her English returned. Eventually she died.

(*f*) B., aged 56, widower, a German who had long been resident in this country, seen December 12, 1914. There was a history of syphilis thirty-five years before. In November, 1913, he had had slight right hemiplegia with "some difficulty in words," which had passed off. His urine was albuminous, and he had hypertrophy of the left ventricle. One week before I saw him he had become dizzy, complained of weakness of the legs, and would have fallen if not prevented; seemed unable to read and was confused and emotional. The day before I saw him he was unable to sign his will, which had been drawn up and to which he had given his approval. On examination he had no paralysis, but some defect in localizing sensation. He was apraxic in that he could not feed himself, although there was no paralysis, and on being given a pen held it with the nib reversed. He did not ask for anything or propose anything. He could answer questions slowly, but could not volunteer information about himself or talk spontaneously. He was not word-deaf and did what he was told. He could not name objects, but recognized the correct name. He could not even give the names in German, his native language, with the exception of a watch, which he called "Uhr." He could not tell the time by it, could not give his address, or name the locality in which he lived, or give the month. He seemed to be word-blind, or nearly so, could not read except one or two isolated words, and could not name the letters in words pointed out to him. He was quite unfit to exercise testamentary capacity or to transact business. He died shortly afterwards.

(*g*) The last case of this group which I shall narrate was H., aged 62, married. There was a history of excess in alcohol and sexual irregularity. I saw him on October 12, 1916. In the previous May he had had a seizure without resulting paralysis, but following which he was unable to read for some days. Three weeks ago he had had another attack, in which he could not speak for one whole night, and could not write or read, and had slight loss of power in the right arm and right side of the mouth. He then recovered speech, but became very depressed and worried, accused himself of moral lapses, especially of sodomy with women, and dreaded prosecution for this, although none was pending, and he could not remember the circumstances. He was completely obsessed by this dread, and had spoken of suicide. On examination he was found to have a systolic mitral murmur and auricular fibrillation. The hemiplegia had passed off. He talked connectedly and answered questions, did not seem depressed, and said he had exaggerated the idea of prosecution, and was not troubled about it any more. He could not give the name of pencil, chain, matchbox, &c., but after giving the name to a watch, he showed perseveration by afterwards calling everything a watch. He could repeat names of objects when told, and recognized their correctness. He could not tell the time or name coins or give their value. He was word-blind and totally unable to read. He could not write even his name spontaneously, but could just copy it. He was quite unable to transact business.

His condition was at first masked by his ability to talk. It was quite evident that he could "propositionize," but he had evidently had insane propositions, and this, together with the manifest organic disorder of at least the visual speech area, led to the advice that he should be sent away from home for care and treatment. While arrangements were being made for this he eluded his relations, although they had been warned, went out and bought a gun-licence and a revolver, and shot himself.

It is difficult to say whether this result was determined by the morbid dreads which he had shown or by the difficulty in which he found himself by his inability to write or read: no doubt both factors acted. Evidently he had sufficient speech, both internal and external, to enable him to come to a decision and carry out his desire.

I have called attention to cases in which apraxia, agraphia and aphasia were associated. I may say that of eight cases which have come before me in which apraxia in various forms was noted all were also agraphic but none of them had definite right hemiplegia. In nearly all of them the aphasia was chiefly of the motor type.

S. A. K. Wilson, in a comprehensive study of apraxia (*Brain*, 1908, xxxi, p. 164), calls attention to the reason for believing that there is a centre in the first and second convolution of the left side where movements are combined ideationally analogous to the centre for the co-ordination of movements requisite for speech in Broca's area, and points out that in motor aphasia we have a form of apraxia, and that agraphia is a variety of apraxia, which may be either of sensory or motor origin, and that there may be agraphia without any paralysis. J. S. Collier (*Brain*, 1908, xxxi, p. 529) also refers to the evidence pointing to a lesion of the first and second frontal convolutions of the left side in cases of apraxia, and says "the bearing of this evidence upon the localization of a motor speech centre in the left third frontal convolution is obvious and striking, for motor aphasia bears the same relation to movements of the muscles concerned in speech as does apraxia to the movements of the limbs."

We now come to the *third* group, in which, in spite of severe affection of speech, the patient possesses internal language and such a degree of mental capacity as to permit of business or testamentary capacity. I shall refer to three cases of this nature.

[illegible] M., single, aged 64, seen May 31, 1912. There was a history of syphilis twenty-five years before, and he had lived a great deal in South America, where Spanish was his usual language. In August, 1909, he had a seizure, followed by right hemiplegia and loss of speech except for one or two Spanish words. Between July and December, 1910, he had six fits, and two others up to May, 1912. He was considered by a relative to whom he was unfriendly to be childish and incompetent, but he had always been found by his solicitor to be alive to what he thought right for himself. He had exercised volition in signing an authority to his solicitor and had made a will twelve months before. On examination he was found to have right hemiplegia with wasting and contracture of the right arm. His right leg was weak, and he walked stiffly. His right knee-jerk was exaggerated. He was unable to speak spontaneously, but could say "yes" and "no" correctly in answer to questions, and they certainly had propositional value. He could not say his own name, he could not give the names of places he had lived in, but recognized the name of a South American town saying "yes, yes." He could not always name objects seen, but could repeat the word when it was said, and afterwards there was perseveration of the idea when a fresh object was shown. He could not name coins, but knew if a wrong name was given. He could say the names of his solicitor and two relatives. His expression was that of intelligence, his hearing and sight were good, he was able to do what he was told and could pick out objects of which he heard the names. He could not read aloud but recognized one or two words and repeated them. He appeared to read to himself and could answer correctly "yes" and "no" as to what it was about. His right arm being completely paralysed he could not write with it. He was however able to sign his name slowly with the left hand and had signed an authority in this way to his solicitor to receive money and make disbursements for him. He could copy from print to writing with his left hand, and could write slowly from dictation. In consequence of the laboriousness of writing with his left hand he did not write letters. He showed by gestures and by saying "no, no," that he had antipathy to the relation who thought he was an imbecile. He knew perfectly well whom he wanted to manage his affairs. He could answer as to his income by exclusion when wrong amounts were suggested to him. He was quite happy in a nursing home.

In this case there was no word-blindness or word-deafness, and it seems a fair presumption that this was one of the cases in which Broca's region was chiefly affected as far as speech was concerned, and the history of seizures suggests cortical damage. Internal speech appeared to be unaffected. There was no difficulty in reporting that he was able to understand and execute a legal document, which was the question at issue.

(*b*) D., aged 83, widower, seen March 13, 1917. He had lived abroad for many years and most of his immediate relatives being well provided for he had made a will in December, 1915, leaving various legacies to friends and a nurse who had attended him through a severe illness five years previously and one to a nephew. On May 15, 1916, he had an attack of right hemiplegia with aphasia. He was speechless except for occasional ejaculatory words. He could not read aloud or understand written language, he could write his name automatically, but could not copy it, and could write nothing else. He was certainly "word-blind." He was not however "word-deaf," but could understand what was said and do what he was told. He could by gesture express agreement with or dissent from leading questions. On July 7, 1916, he had another attack depriving him entirely of speech. His mind was said to be much more confused but not blank. Some months later he appears to have made some improvement in intelligence although he remained speechless, and was said to show dislike to the will he had made and appeared to want to improve the position under it of the relative who had shown much attention to him in his illness. Although he could not speak he went through the gesture of striking out the name of an old friend to whom he had left money, and he seemed agitated and emotional. He was unable to give instructions either orally or by writing to his solicitor, and neither the latter nor another physician considered him at that time to be in possession of testamentary capacity. Further improvement in his condition took place and he frequently conveyed the impression of dislike to his will as it stood and was often looking at it. When seen on March 14, 1917, he was still suffering from defect of speech, but the hemiplegia had to a great extent passed off. He could answer questions in monosyllables, but frequently used wrong words in trying to speak. He often took hold of his tongue, as if he felt it would not work properly and knew he was using wrong words. His answers "yes" and "no" were to the point and had propositional value, and his memory appeared to be good when interrogated about his past life and occupation, the names of his relatives, and the extent of his property. He gave assent readily and emphatically to the question as to whether he wished to alter his will. He occasionally said a short sentence, and gave the names of relations. He definitely expressed affection or dislike for individuals, and was found to have knowledge of those whom he would naturally benefit and of the reasons for doing so. All this was elicited by a long series of questions and by propounding to him suggestions to which he was able by gesture and emphasis and by the tone of his answers to give reasonable assent or dissent. His expression was that of a man alert and appreciative of the position. He was unable to read aloud, but was able to read to himself and showed by answers that he appreciated what he read. He could not write his name but made attempts to do so. On this occasion he was neither word-deaf nor word-blind, the chief defect appearing to be a motor one both in speaking and writing. A full report was made as to his condition, and the opinion given that he now appeared to have testamentary capacity. At a subsequent interview with his solicitor and another physician it was possible to take instructions from him and a fresh will was executed.

6. Miss R., aged 60, seen April 12, 1913. Brother and sister both had [illegible] hemiplegia and aphasia. Both ovaries had been removed some years before and [illegible] had the operation of "short circuiting" in consequence

of intestinal trouble. After this a drug habit had begun, dating from the use of morphia to relieve pain. In the autumn of 1912 she had had a short attack of mania from which she had recovered. A short time before I saw her she had had an attack of what was supposed to be influenza, followed by right hemiplegia and hemianæsthesia. For three days she could not talk clearly and for ten days she could not sign her name. She had recovered writing to some extent but complained that she could not "make the pen spell." In talking she missed words and used wrong words, could not name objects, but knew their uses. She complained of losing her brain, and said she had better be locked up. She knew who were her relations and the extent of her means and whom she wished to benefit, and had no delusions. She was anxious to know if she was fit to make her will and after a full consideration of her condition the opinion was given that she had testamentary capacity.

In these testamentary cases the same general rules apply as in cases where there is no aphasia—namely, that the testator "shall understand the nature of the act and its effects; shall understand the extent of the property of which he is disposing; shall be able to comprehend and appreciate the claims to which he ought to give effect; and, with a view to the latter object, that no disorder of the mind shall poison his affections, pervert his sense of right, or prevent the exercise of his natural faculties, that no insane delusions shall influence his will in disposing of his property, and bring about a disposal of it, which, if the mind had been sound should not have been made."

In cases such as I have mentioned the extreme importance of long unhurried interviews need not be emphasized. Moreover there is the more need in such cases for an accurate record of the questions put to the patient and his answers, whether in faulty speech or writing or associated with gestures and emotional emphasis. The use of shorthand in this respect is very great.

Sir William Gairdner, in opening a discussion on "Aphasia in Relation to Testamentary Capacity" (British Medical Association, Annual Meeting, Edinburgh, 1898; *British Medical Journal*, 1898, ii, p. 581), laid stress on the point that "The fact of aphasia (unless it be very limited in extent) interferes either with the graphic and visual speech processes or with the auditory and vocal speech processes and therefore throws the *onus probandi* upon those who consider the will genuine or wish to prove the will genuine."

Hughlings Jackson has well said: "Such a question as 'Can an aphasic make a will?' cannot be answered any more than the question, 'Will a piece of string reach across the room?' can be answered.

The question should be: 'Can this or that aphasic person make a will?'" (*Brain*, 1915, xxxviii, p. 115).

In other words every case must be considered on its merits after the most careful examination.

I shall not give any details of very slight cases or of those cases which have come under my notice where a severe vascular lesion causes right hemiplegia and aphasia, ending quickly in death.

It will be well to consider how far these cases correspond with Hughlings Jackson's views on aphasia. I may take it that his great principle that dissolution occurs first in the most highly organized products of neural or mental activity, leaving the more lowly at liberty to express themselves freely in the resulting symptoms, is beyond dispute. Positive and negative symptoms are seen both in the mental state and in the condition of speech, but the preponderance of senile cases accounts for the fact that the negative side is the more conspicuous, as shown by the frequent occurrence of loss of memory and perception of judgment and control.

The return to an early acquired language and the loss of a more recent one in attempting to speak has been exemplified in some of the cases narrated.

Hughlings Jackson divided cases of aphasia into two classes:—

(1) Severe cases in which the patient is speechless or nearly so, or in which speech is very much damaged, and

(2) Cases in which there are plentiful movements but wrong movements, or plenty of words but mistakes in words.

These groups have been exemplified in various degrees in my cases, as have also his differentiation of speech into superior and inferior, internal and external, his description of recurring utterances and occasional utterances and his insistence as to the use of the words "yes" and "no" as being in some cases of propositional value, though often otherwise.

With regard to recurring utterances, I may refer to Jackson's view (*Brain*, 1915, xxxvii, p. 158) that the lesion in the left half of the brain "is not the cause of the recurring utterance," and that if the patient had not been "taken ill" he would not have had such a recurring utterance as "he would have been able *not to utter it*" (italics in original).

Again, he says (p. 174): "A patient who recovers soon from aphasia loses his recurring utterance, becomes able not to utter it." In other words the higher centre has regained control and prevented the over action of lower centres. It appears to me that the case of one lady mentioned above is a good illustration of this.

Hughlings Jackson held that speech was a part of mind and that we must get rid of the feeling that there was abrupt and constant separation into mind and speech. At the same time he pointed out clearly the fact that in some cases there may be great affection of external speech and yet little affection of mind, as shown by the evidences of internal speech especially in writing. In one of the cases I have narrated the patient having practically no external speech was also unable to write with his right hand and very little with his left, yet there was no doubt that he had considerable mental capacity. On the other hand a patient who had a considerable amount of external speech, but who had lost writing, was so disordered in mind that he had delusions and committed suicide.

I have shown that aphasia may supervene on pre-existing insanity or mental decay, no doubt due to vascular or degenerative changes, which might have led to the same result in the absence of the mental disorder, and that, on the other hand, aphasia may be the first symptom indicative of cerebral and mental decay. In such cases the question arises how far the mental disorder is intimately associated with the aphasic disturbance of speech or is due to widespread vascular and nutritive changes in the brain. The mental disorder does not necessarily amount to certifiable insanity, but may in varying degrees affect such mental processes as perception, memory, emotion and volition, without much disorder of conduct. It appears to me that in all my cases, except those which may be looked upon as examples of Broca's aphasia, there has been some disorder of mind, though not always marked failure of intelligence. In a review of the question of aphasia (*Review of Neurology and Psychiatry*, 1909, vii, p. 151) S. A. K. Wilson says: "Speech is but a specialized part of the intellect. And, therefore, there can be no disturbance of the function of speech, however slight, in which there is not a disturbance of certain psychical states." He, however, combats Marie's view "that in cases of aphasia (i.e., in Marie's sense) defect of intelligence only occurs and always occurs in lesions behind an imaginary line drawn from the posterior end of the island of Reil transversely to the lateral ventricle." I cannot believe that the disorder of mind in such cases as I have observed is only associated with a lesion of a single centre of intellect specialized for language, but believe that, on the contrary, it is associated with widespread vascular and nutritive changes in the brain, such as are commonly found in senile or syphilitic cases.

Once more to quote Hughlings Jackson (*Brain*, loc. cit., p. 167): "We must bear in mind that 'will,' 'memory' and 'emotion' are only the names men have invented for different aspects of the ever present and yet always changing latest and highest mental states which in their totality constitute what we call consciousness."

In conclusion, I may say that my observation of those cases of aphasia which have come under my notice leads me to agree completely with the views expressed by Henry Head in the Summary at the end of his paper, to which I have already referred.

Section of Neurology.

President—R. PERCY SMITH, M.D.

(*January* 24, 1918.)

Muscular Atrophy and other Changes in Nutrition associated with Lesions of the Sensory Cortex of the Brain, with Especial Reference to the Possible Existence of Trophic Representation in the Post-central Areas.

By LEONARD GUTHRIE, M.D.

THE works of Colin Russell and Horsley,[1] Campbell,[2] Mills and Weisenburg,[3] G. Bergmann,[4] and above all, the elaborate and masterly researches of Henry Head and Gordon Holmes[5] have made us familiar with many of the functions of the sensory, or post-central cortex. But I am not aware that any observations have been recorded on the muscular atrophy and other changes in nutrition which occur in consequence of injuries of these cortical areas.

Since the beginning of the War, a number of cases of injuries of the post-central cortex which illustrate these conditions has been received at the Empire Hospital for Officers. For the notes and facts concerning them which I am able to bring to your notice, I am entirely indebted to Captain George Riddoch and Captain Maurice Nicoll, Resident Medical Officers to that institution.

Muscular Atrophy in Post-central Lesions.—In all cases of post-central lesions a certain amount of muscular atrophy is noticeable within a few weeks after the receipt of the injury. In unilateral lesions the atrophy is of the muscles of the contralateral limbs, and affects the distal more than the proximal portions of the extremities. It is more marked in the hand and forearm than in the upper arm, and in the foot and leg more than in the muscles above the knee. In vertical injuries (e.g., those involving the superior longitudinal sinus) the atrophy of the limbs may be general, but chiefly affects the muscles of the legs below the knees. The wasting is in no case excessive. It never approaches the degree of wasting seen in spinal muscular atrophy, or in that due to injuries of the peripheral nerves or roots, but it is sufficient to attract attention directly the limbs are exposed to view. Muscular wasting in these cases is rapid in development, but having reached a certain stage it is not progressive. It tends to remain stationary in spite of treatment. It is usually, perhaps always, preceded by motor paresis, if not paralysis, which, however, passes off in the course of a few days or hours, leaving the affected limbs weak, but capable of all voluntary gross movements, though not of movements requiring delicacy and precision. The weakness is not greater than can be accounted for by the wasting, and it may be demonstrated by the lessened power of resistance which the patient can exert against passive movement especially of the fingers and toes.

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[1] *Brain*, 1906, xxix, p. 137.

[2] "The Localization of Cerebral Function," Cambridge, 1905.

[3] *Journ. Nerv. and Ment. Dis.*, 1906. xxxiii, pp. 617-650.

[4] *Brain*, 1909, xxxii, p. 342.

[5] *Brain*, 1911, xxxiv, p. 102.

Later Changes in Nutrition.—After the lapse of some months, and within about a year of the injury, certain changes in nutrition make their appearance in addition to simple muscular wasting. These are most noteworthy in the hand, which loses its former character and becomes quite unlike the unaffected hand. Without being in any way deformed, it is smaller and more delicate than the other. A glove which formerly fitted is now a size too large. The fingers become tapering, the pads thinned, the nails long and narrow. The thenar and hypothenar eminences lose their prominence, and the lines of the hand in general are ill-defined, or less obvious than those of the hand on the opposite side. In some cases, it may be described as an expressionless or infantile hand; in others, it resembles a lady's rather than a man's, or that of an artist rather than that of an artisan. It is a hand which has regressed to an earlier stage of development, and has lost its individual character. X-ray examination reveals slight rarefaction and general diminution in thickness of the phalangeal and metacarpal bones, together with loss of clarity in their outlines. Such other changes in nutrition as "glossy fingers," erythema, ulceration, overgrowth or loss of hair, brittleness of nails have not been observed, though minor grades of all these conditions may be present.

Vasomotor affections such as ischæmia, cyanosis, "dead fingers" may occur to a limited extent in distal parts in these cortical injuries. Temporary inequality of the pupils is sometimes noted.

Hyperthermia and Hypothermia are more readily induced by the higher and lower degrees of heat and cold than in normal subjects. At normal ranges of temperature, the affected extremities are colder to touch than those of the opposite side of the body.

Sensory Affections.—Complaint of pain, heat and cold as such is never made, but the patient is subjectively conscious of coldness, and of some indefinable change in the general sensibility of the affected part. The hand or foot "feels funny," he will say, or "as if it were not mine." "I cannot forget it, and am always thinking and worrying about it."

Tactile Sensation.—The loss in general is slight, but the patient shows on testing, the extraordinary vagaries, inconsistencies and hallucinations which have been so lucidly described by Head and Holmes, and were detailed in full by Dr. Head at the last meeting of this Section.

Topognosis and Compass-test Reactions.—Appreciation of both these forms of testing sensibility is defective. The error in localizing touch is usually proximal to the body, as maintained by Russell and Horsley. The major loss may be præ-axial, post-axial, or mid-axial, and in each case the loss fades towards the less affected parts.

Reactions to Heat, Cold, Pain.—There is no grave defect in appreciation of either pain or temperature. The phenomena so admirably detailed by Head and Holmes can usually be demonstrated in these cases, when there is any disturbance of the temperature sense.

Sense of Posture, and Appreciation of Measured Movements.—The postural sense and appreciation of measured movements are always grossly disturbed, and as pointed out by Head and Holmes, the defect is in regard to distal much more than proximal parts of the affected limbs. Moreover, the loss of appreciation of posture and measured movements is again præ-axial, post-axial, or mid-axial in distribution. The patient shows the same inconstancy in his answers to these as to other tests of sensibility.

Stereognostic Sense: Recognition of Shape and Form in Three Dimensions. Estimation of Weight.—These faculties are always seriously impaired in lesions of the sensory cortex. The defect is always greatest in those portions of the extremities which are proved by other tests to be most at fault.

Ataxy is always present to a greater or less extent in accordance with the degree with which other forms of sensibility are affected. The patient may be unable to stand with his eyes closed, or to stand or walk at all without assistance.

Tendon Jerks.—Muscular wasting and weakness are constantly associated with exaggeration of proximal tendon jerks—e.g., of the elbow and knee.

Loss of Muscular Tone.—Hypotonia is always a prominent feature in post-central injuries, and follows the general rule in affecting distal more than proximal portions of the limbs, and in being maximal either præ-axially, post-axially, or mid-axially. It can be estimated during active or passive movement, or in a limb that is stationary in posture.

Vibration (tuning-fork test).—The sense of vibration is not materially altered in these cases.

REMARKS.

If we regard the post-central convolutions as one large sensory end-station, composed of a mosaic of neurons or "centres" for the reception of every kind of afferent impulse, each "centre" specialized yet correlated with others, and with the adjacent motor "centres," it must be that it occupies a considerable portion of the cerebral cortex. Hence, lesions in this region are hardly likely to be complete without endangering life, or at least rendering the patient's condition impossible to investigate within a short time of the receipt of the injury. Wounds of this area are, however, usually partial, and as they involve different structures in different individuals, the symptoms and conditions produced must also vary widely. No case is likely to exhibit complete and permanent abolition of all cortical sensory functions.

All the changes in nutrition which have been described above will therefore not be present in every case of injury to the sensory cortex, nor is it possible at present to localize the exact site of the lesion which causes them. They occur, however, so frequently in association with hypotonia, loss of muscular sense, which includes loss of sense of posture and of position of the limbs in space, loss of appreciation of passive movements, loss of power to estimate form, shape, and weight of objects, and also loss of capacity to gauge the amount of muscular effort necessary to perform a desired action, that we may assume that the portions of the cortex at fault are in close propinquity.

In regard to the ætiology of muscular atrophy, and changes in nutrition in post-central injuries, the questions in need of solution are:—

(1) Are muscular atrophy and changes in nutrition simply the

result of disturbance of those functions which are undoubtedly impaired in cases of post-central injuries?

(2) If not, are they attributable to disturbance of some special trophic functions of the sensory cortex?

Motor paralysis and disuse may at once be excluded as sole causes. Cerebral motor paralysis does not cause muscular atrophy unless it has been of long standing and extensive. In these post-central cases, motor paralysis or paresis has been of such extremely short duration that it seems incapable of producing the degree of muscular atrophy present. Even if it be admitted that *disuse* alone induces wasting of muscles, it will hardly be contended that such a result follows disuse of a limb or limbs which has only existed for a few hours or days. It is still more improbable that such temporary disuse is responsible for the changes in the configuration of the hand described above.

Are they the result of hypotonia, ataxy, and loss of muscular sensibility? Disease of the posterior roots and columns causes hypotonia, ataxy and loss of muscular sensibility, which are usually associated with some amount of muscular wasting. But hypotonia and ataxy may occur to a marked extent without muscular wasting, as in cerebellar disease, and cannot therefore be regarded as solely responsible for muscular atrophy. Nor indeed are the changes in configuration and character of the hands described above ever seen in cases of cerebellar hypotonia, or in hypotonia owning other causes. It is true that well known changes in nutrition occur in tabes dorsalis, but they do not resemble the changes in nutrition now under consideration. Loss of muscular sensibility with all its concomitants may again occur without such changes in nutrition as have been described.

It seems reasonable therefore to ascribe muscular wasting and changes in nutrition associated with post-central injuries to the loss of some function other than those which concern muscular tone and sensibility, and to conclude that the sensory cortex has some special trophic functions which are separate though closely allied to the other functions which it performs. Evidence of such trophic functions is afforded by the class of infantile or congenital hemiplegia of cerebral origin, in which there is arrest of growth of all parts of the affected limbs including the hands, together with ataxy, and slight motor spasticity, but without motor paralysis.

In another class of congenital cerebral disease described by Foerster as "atonic diplegia," we find an extraordinary amount of muscular wasting coupled with extreme hypotonia, spasticity and exaggerated tendon reflexes, and Dr. F. E. Batten, at a recent meeting of this Section, suggested that the anatomical lesion in these cases might be in the post-central sensory cortex.

One must admit that the term "trophic" does not convey much information. Although we may find reasons for not growing, we do not know what makes us grow. It is still a moot point whether the influence of the nervous system on the nutrition of tissues is excited through special "trophic fibres," or through the ordinary motor, sensory or vasomotor nerves. Muscles waste if their motor nerve supply is destroyed, but this does not prove that the wasting is solely the result of motor paralysis. On the other hand, the spinal ganglia, we know from Head's classical work on the subject, possess trophic functions as shown by the acute changes in the skin (herpes zoster) which accompany disease of them. Gordon Holmes says: "It seems probable that the centrifugal conduction of the trophic influence is a function of the sensory fibres, or more probably that the normal nutritional equilibrium is in some way determined by the sensory nerves."[1] But vasomotor nerves also have, undoubtedly, considerable influence on the nutrition and growth of tissues. As we have seen in the cases under consideration there is not much evidence of gross disturbance of the sympathetic, yet the evidence is sufficient to suggest the existence of some control exercised over it by the sensory cortex.

It seems probable that whilst growth and nutrition are dependent on the integrity of the nervous system below the brain, the hierarchy of the whole resides in the brain itself, and that some of the centres which govern growth and nutrition are situated in the post-central areas of the cortex. Also, that these re-represent spinal segments which preserve nutrition, perhaps through the agency of the sympathetic and sensory nervous system.

As an example of spinal muscular atrophy and altered nutrition precisely analogous to the cortical form which has been described, I show photographs of a child aged 6, who at the age of 3 had tuberculous caries of the cervico-dorsal spine. This was followed by arrest of growth of the left upper extremity, atrophy and atony of the hand muscles, and ataxy, together with paralysis of the left oculo-cervico-sympathetic nerve. The condition is obviously the same, though more pronounced than that produced by cortical lesions, and suggests that the cervico-dorsal spinal segments governing nutrition are re-represented, as I have maintained, in the post-central sensory cortex.

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[1] Osler and McCrae, "System of Medicine," vii, p. 171.

Section of Neurology.[1]

President — Dr. R. Percy Smith.

Cases of Wounds of the Nervous System.

By Henry Head, M.D., F.R.S.

Case I: Partial Hemianopia with Considerable Power of Appreciating Movement but not Stationary Objects.—Gunner H. C., aged 29. Wounded on January 10, 1918, probably with shrapnel. Trephined the same day; dura showed two small punctures. Right hemianopia from the beginning. Admitted to the London Hospital on January 27, 1918, with a perfectly healed scar in the left occipital region, 30 to 32·5 cm. along nasion-inion and from middle line to 3·5 cm. to left; total nasion-inion 34 cm. Now right hemianopia with much loss to stationary objects and comparatively little loss in the upper quadrant to moving objects.

Case II: Parietal Lesion, with Hypotonia and profound "Feminine" Appearance of the affected Hand. — Private J. H. W., aged 22. Wounded on November 14, 1917, by a sniper's bullet. Trephined on December 8; fissured fracture; dura perforated. Admitted to the London Hospital on February 6, 1918, with a perfectly healed scar in the right parietal region, 18·5 cm. along nasion-inion and 9 cm. from middle line; total nasion-inion 37 cm. Fully healed flap of operation. No loss of isolated movements in the left hand; grasp weak: ataxy of fingers on closing eyes; hypotonia; reflexes not affected. Sensation profoundly affected to passive movement, compass-test, localization, tactile hairs, weight, and form. The affected hand is thinner, rounder, softer, and more "feminine" in appearance than the right. The difference is profound, even in the shape of the nails.

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[1] At a meeting at the London Hospital, February 21, 1918.

Case III: Typical Unilateral Lesion of the Cerebellum.—Private W. A., aged 27. Wounded on November 8, 1917. He knows nothing about it. Recovered consciousness three days later. Admitted to the London Hospital on November 24 with an open wound in the right occipital region, with a perforation in the bone 33 cm. back and 2·5 cm. to right; total nasion-inion 35 cm. Signs of a pure right-sided cerebellar lesion. Inco-ordination, asynergia, dysmetria, deviation, adiadochokinesis, tremor.

Case IV: Destruction of Spinal Roots and of the Termination of the Spinal Cord: Automatic Bladder: no Facilitation possible owing to Absence of all Spinal Reflexes below the Lesion.—Gunner J. P., aged 21. Wounded on August 14, 1917, with fragment of a large shell. Fragment of metal found at operation to be adherent to pedicle of twelfth dorsal vertebra; this in turn had been driven across the theca; dura torn. Admitted to the London Hospital on August 21. Loss of sensation and motion from level of first lumbar on the left and second lumbar on the right lower extremity. Abolition of all reflexes, superficial and deep. Much subsequent wasting of both legs. Automatic micturition with no facilitation.

Case V: Injury to one or more Anterior Roots by a Missile now lodged in the Body of the Vertebra without affecting Posterior Roots.—Sergeant R. M., aged 40. Wounded on December 6, 1917, with aeroplane bomb. Fractured lamina of second lumbar with a piece of metal in the body of the vertebra. Motor loss, but very little if any sensory affection from the beginning. Admitted to the London Hospital on January 17, 1918. Total inability to dorsiflex left foot or extend toes. Plantar extension of foot and flexion of toes well performed. No loss of sensation of any kind. Left knee-jerk greatly diminished and ankle-jerk absent: plantar normal. Left leg much swollen below the knee. Anterior tibial group of muscles do not react to the interrupted current; to the constant current they react with a slow vermicular contraction.

Case VI: Toxic Polyneuritis of the Trenches.—Gunner P. F., aged 20. November, 1917: Pains in legs. December 4: Sent to Rest Station and treated for boils. Became so ill he was sent to the Base and seen by Major Foster Kennedy, who diagnosed toxic polyneuritis. Admitted to the London Hospital on December 29, 1917. All deep knee-jerks absent; abdominals and plantars present. Weakness of all four limbs, and of right angle of mouth. Intense Romberg: cannot walk. Much loss of sensation to vibration, and want of recognition of posture in both feet. Soles insensitive to cotton wool, heat, and cold; prick appreciated. Rapid recovery.

Section of Neurology.

President — Dr. R. Percy Smith.

Muscular Hypotonia and Changes in Nutrition associated with Lesions of the Posterior Columns of the Spinal Cord and of the Posterior Roots.[1]

By George Riddoch, M.D.

It is now generally believed that muscular tonus is reflex in nature. Sherrington [3] defines reflex tonus as postural contraction, and although his definition may be criticized as too sweeping it well explains one aspect of tonus. When applied to the skeletal system he believes that it is distributed to those muscles which maintain the animal erect in standing, and which are in contraction in the first phase of step. It is a proprio-ceptive reaction of anti-gravity muscles and the impulses which excite it originate in the receptors of the muscles themselves, their tendons, fascial sheaths, &c. The adequate stimulus is tension or stress of these muscles, and the impulses travel to the spinal cord by way of the muscular nerves and the posterior roots. The controlling centres of the reflex system underlying postural tonus are situated chiefly in the pons, mid-brain and bulb, and their activities are adjusted by impressions from the labyrinths and cerebellum. Ablation of the cerebral hemispheres is followed in the dog by a heightened condition of tonus in the extensor muscles which is manifest as "decerebrate rigidity."

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[1] At a meeting of the Section, held January 24, 1918.

The results of experimental physiology corroborate the views expressed thirty years ago by Hughlings Jackson. He argued that the rigidity in the affected limbs in hemiplegia is not due to the cerebral lesion nor to the degeneration of the cortico-spinal tracts, but is the outcome of unantagonized influence of the cerebellum. He believed that "the cerebellum is the centre for continuous movements and the cerebrum for changing movements." That the cerebellum itself is not the only source of influence nor probably the most important, as indicated by more recent researches, does not detract from the principle formulated by that great teacher.

The maintenance of posture or postural tonus is subserved thus by a higher reflex mechanism whose activities may be entirely unconscious. As Head and Thompson [2] clearly emphasized, "sensation may never be evoked until some disturbance of equilibrium produces impulses which, passing up sensory paths, reach the highest centres concerned with sensation." These observers believed that the posterior columns which convey impulses subserving the sense of passive position and movement are closely associated through collaterals with afferent cerebellar tracts, notably with the direct cerebellar tract. They suggested that "in their passage through the spinal cord, the afferent impulses of deep sensibility, concerned with the position and movement of joints, divide into two groups—those destined after transformation to become the non-sensory afferent impulses of the direct cerebellar tract, and those which continue up the fibres of the primary system in the posterior columns to subserve the sense of position and movement." It is therefore clear that of the impulses which originate from the deep structures, such as muscles, tendons, fascial sheaths and joints, and enter the posterior columns of the spinal cord, some reach the cerebral cortex and cause a sensation of altered posture while others pass to the centres in the pons, bulb and cerebellum of the reflex postural mechanism and never excite consciousness at all. On these latter depends the automatic, unconscious adjustment of the posture of the body, which is almost continually changing. In uncomplicated injuries of the cerebellum the reflex system is upset without there being any interference with the passage of sensory impulses; but in the spinal cord and in the posterior roots the sensory and non-sensory afferent paths are so closely associated that the one cannot be affected without the other. In other words, with lesions at these levels hypotonia is always associated with defect in spacial appreciation.

Can it be determined whether the injury causing these symptoms is of the posterior columns or of the posterior roots? Thanks to the researches of Head and his co-workers by investigating the condition of the other aspects of sensation the problem can be solved. At the level of the posterior roots, where all sensory impulses are still in their primary grouping, epicritic and protopathic will be affected with deep sensibility and associated with loss of tone in the muscles. Further, the areas of sensory loss will have a regional distribution and will be determined by the particular roots involved.

To give an example. Lieutenant H. was wounded by a piece of shell in the lumbar region on October 19, 1917. At the Casualty Clearing Station the track of the metal leading down to the fourth lumbar vertebra was explored, but the piece of metal was not found. His lower limbs were weak but he could perform all movements. At that time the sensory condition was not noted. At No. 14 General Hospital, where he was admitted on October 20, it was observed that he passed urine normally. All movements were possible in both lower limbs but they were weak, the right limb being more paretic than the left. Knee- and ankle-jerks were not obtained. On November 20 he arrived at the Empire Hospital, and was placed under the care of Dr. Guthrie. His present condition is as follows: The right knee-jerk is not obtained while the left is present but sluggish. Both ankle-jerks are absent. Abdominals are brisk and symmetrical in all segments. The plantar reflexes are of the normal type. Cremasteric and bulbocavernosus reflexes can be evoked, and the patient has had several erections of the penis. All movements of the lower limbs are weak, those of the right limb more than of the left. There is a moderate degree of general wasting of both lower extremities, and definite hypotonia most noticeable at the joints of the toes, ankle and knee, and of the right leg more than of the left. The legs are ataxic. The patient walks with a high stepping gait. When he is walking the right foot tingles. In the right lower limb there is defective appreciation of touch, pain and temperature, of deep pressure and pressure pain in the fourth and fifth lumbar and first sacral areas. Though he complains of pain only when firm pressure is applied to the calves, it is of more disagreeable character than normal. The threshold for compasses is high in the right sole, and at the joints of the toes and of the ankle and knee the appreciation of posture and passive movement is grossly affected. There is defective appreciation of vibrations (C 128) below the knee, and faulty recognition of shape, size and form with the sole of the foot. Sensation in the left lower limb is now almost normal, but any loss that remains is of the same type as in the right. Apart from a slight delay in starting micturition his bladder acts normally. He tends to be a little constipated. Here then was a lesion involving principally the lower lumbar and upper sacral posterior roots, and with hypotonia all aspects of sensation were affected in the regions corresponding to the root distribution.

Compare this with what obtains when the posterior columns of the spinal cord are damaged. Sensory impulses associated with pain, heat and cold, tactile localization and, for the most part, touch are grouped in paths of the secondary level in the opposite antero-lateral columns of the cord, while those which subserve the appreciation of vibrations of a tuning-fork, the recognition of size, shape and form, passive position and movement and tactile discrimination pass up uncrossed in their primary grouping in the posterior columns as far as the nuclei of these tracts. Muscular hypotonia following a lesion confined to the posterior part of the cord will therefore be associated with defect of those aspects of sensation which are concerned with the appreciation of changes in posture, &c., while heat and cold, pain and tactile localization will be unaffected. A very good example of this combination was seen in a patient whose case was demonstrated by Dr. Guthrie at the Clinical meeting of this Section held on November 22, 1917.

Lieutenant B. was wounded on August 12, 1917. A bullet struck him high up in the back of the neck at the level of the fourth cervical vertebra. At the Sixty-fourth Casualty Clearing Station it was noted that there was paralysis of both arms and partial loss of sensation. He could move both legs well and passed urine naturally. He was admitted to the Empire Hospital on October 20, 1917, under the care of Dr. Guthrie. The following are important points in his case: Left pupil larger than the right; both reacted to light and on accommodation. Slight ptosis was present on the left side. Arm-, knee- and ankle-jerks were brisk and equal on the two sides. Poorly sustained ankle clonus was obtained in either lower limb. Abdominal reflexes were present in all segments. On scratching the sole of either foot the great toe moved down and the hamstring muscles contracted. All movements were possible. Both arms were weak, the right more than the left, and especially the hands and fingers; but the paresis was due more to faulty co-ordination than to actual motor loss. Voluntary power in the trunk and lower limbs seemed to be only slightly affected. There was a moderate amount of wasting of the arms, but more of the fingers. The trophic changes in the hands will be described later.

The tone of the muscles of the lower extremities and of the upper- and forearms was diminished, while the muscles of the hands were grossly hypotonic. There was great inco-ordination of the movements of the arms, especially of the hands and fingers, and he was entirely unable to find an object with his hands in the dark or when his eyes were shut. To turn on the bed-lamp at night he had to grope for the switch with his upper arm and then manipulate it with his teeth. Inco-ordination of the lower limbs was of slighter degree. There were no involuntary movements of the body or limbs. His gait was ataxic and he could only with difficulty stand when unsupported with his eyes closed. He was a professional dancer at one time and he could now dance much better than he could walk. Touch tested with cotton-wool was everywhere appreciated, in the lower limbs, trunk and upper arms. In the hands and especially in the thumb and first three fingers and in the pre-axial segment of the forearm tactile sensibility was gravely disturbed. There was no demonstrable loss to pain, heat and cold except over the areas defective to touch. A pin-prick in the hand was very disagreeable to him and the pain radiated widely. Tactile localization was affected in the hands and outer parts of the forearms, as also were deep pressure and pressure pain, but everywhere else they were undisturbed. The appreciation of posture and passive movement and of the vibrations of a large tuning-fork (C 128) and tactile discrimination (compass test) was extremely impaired in the hands and fingers, less in the arms and still less in the legs and feet. The recognition of shape, size and form and of differences in weights was likewise grossly affected in the hands. The spacial elements were disturbed in the hands out of all proportion to the other aspects of sensation.

In this patient then the lesion in the spinal cord was confined almost entirely to the posterior columns, but in addition some damage had been done to the lower cervical and first dorsal posterior roots, as evidenced by the disturbance of sensibility as a whole in the hands and in the forearms. At the same time the disproportionate loss in these parts of appreciation of posture and passive movement, of vibration of a tuning-fork, of tactile discrimination and of estimation of size, shape and form indicates that this was due also to the effects of the lesion of the posterior columns of the cord. Those parts—namely, the hands and fingers—in which the disturbance of the aspects of sensation which underlie the appreciation of changes of position in space was severe, also exhibited marked diminution of muscular tone.

It would seem clear, therefore, that by investigating the condition of the different aspects of sensation in cases of muscular hypotonia due to interruption on the afferent side of the system which subserves the maintenance of postural tone, one can frequently say whether the lesion is at the level of the posterior roots or at the level of the spinal cord. And differentiation may sometimes be possible when the lesion is more peripherally placed, for in certain diseased conditions of peripheral nerves the degeneration seems to affect chiefly the branches to the muscles, tendons and joints, while the cutaneous fibres almost escape. Hypotonia would then be associated with dissociation between cutaneous and deep sensibility.

Nutritional changes form a very interesting group of symptoms which frequently follow injuries to the posterior part of the cervical enlargement of the spinal cord. They are chiefly confined to the hands, though the forearms may be slightly involved, and are always associated with vasomotor instability. These alterations exist quite apart from the excessive sweating over one half or the whole of the body, which is so often a concomitant of injury in this region. In Lieutenant B. the muscles of the hands, though not paralysed, became flabby and soft. The lines in the palms lost their distinctness, the fingers became tapered and the nails long and brittle. The skin was thin, transparent and glossy. At first the hair on the back of the hands grew profusely, but later it almost disappeared. The joints of the fingers and wrists were supple, and arthritic changes were not detected, but radiograms showed that there was much rarefaction of the bones, especially of the phalanges, which were delicate like those of a woman. At ordinary temperatures the hands felt colder than normal and were cyanosed. The beds of the nails were blue. When very cold the hands and fingers showed a dirty grey colour. On the other hand they reacted to heat more excessively than do normal parts. The colour of the skin was then bright red, and the palms were moist. Sweating was never excessive, but was often present. These changes resemble closely those associated with causalgia of the median nerve.

The clinical picture of sensory loss presented by the patient may seem to indicate, if the examination be hastily performed, that the lesion is almost entirely confined to the posterior columns of the spinal cord. But if more care be taken it will be found that the posterior roots must also have been damaged. Thus in Lieutenant B. all aspects of sensation were affected in some degree in the hand and forearm over the regional distribution of the sixth, seventh and eighth cervical and, to a less extent, of the first dorsal posterior roots, though the major loss was in the appreciation of posture and passive movement, tactile discrimination, recognition of size, shape and form, &c. The nature of the sensory loss in the upper arms, trunk and lower limbs, was entirely of the posterior column type. I would suggest that the nutritional alterations in the hands were the result of the changes in the posterior roots and not of those in the posterior columns, and that these changes involved the afferent fibres belonging to the sympathetic nervous supply of the arm and hand.

The work of Gaskell [1] and of Langley and Anderson has shown that in animals the motor neurons which supply the muscles of the blood-vessels and of the sweat glands over the whole body, also all unstriped muscles below the skin, are connected with the thoracico-lumbar outflow of connector nerves and belong to the sympathetic nervous system proper. The connector or preganglionic fibres of this system pass from the spinal cord by way of the anterior roots of the first thoracic to the second lumbar nerves. It is probable, too, that the afferent fibres have the same regional distribution as the efferent fibres and pass into the spinal cord by the corresponding posterior roots.

If the assumption, therefore, be true that the nutritional and vasomotor changes observed in cases of injury to posterior roots are due to interruption of afferent fibres of the sympathetic system, it is clear that these alterations will be seen only when the roots involved are of the thoracic and upper lumbar region. In such a case as the one quoted, of damage to the lower lumbar and the sacral posterior roots, with defective sensibility in their regional distribution, there is usually a moderate degree of general wasting of the voluntary muscles of the lower limbs, but thermal and trophic alterations in the skin are, as a rule, entirely absent.

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Section of Neurology.

President—Dr. R. Percy Smith.

Cinematograph Demonstration of War Neuroses.[1]

By A. F. Hurst, M.D.

I.

These cinematograph films have been taken during the last nine months under the auspices of the Medical Research Committee, in order that a permanent record of some of the remarkable neuroses which have developed in soldiers should be obtained to form part of the medical history of the War. At the same time it is hoped that a careful study of the films may in the future yield results of scientific value.

The complete series illustrates the development of our methods of treatment. A year ago we were satisfied at Netley if we taught a man, admitted for hysterical paraplegia of several months' duration, to get about within a few days of admission, but we realized that he would require several weeks of further treatment by re-education before he could walk normally. This also applied to stammering, the immediate and rapid improvement being followed by slow improvement under daily re-education, whilst we regarded it as almost impossible to produce any rapid improvement in men suffering from the various forms of tremor and tics.

Increased experience has gradually given us greater confidence, and we are now disappointed if we do not cure all cases of hysterical paralysis and contractures, speech defects such as mutism, aphonia, and stammering, and even tremors and tics, within twenty-four or forty-eight hours of admission, however long the symptoms have persisted before the men reach Netley. This will be seen in the different series of pictures: in the earlier ones slow, but steady progress is shown; the latest are actual records of complete recovery, occurring in the course of an hour or two from hysterical paraplegia which had lasted for several months.

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[1] At a meeting of the Section, held March 12, 1918.

[A series of cinematograph films were then shown, illustrating a large variety of hysterical gaits, paralyses and contractures, the association of spasmodic or rhythmic movements with speech, and different forms of tic.]

II.

The steady improvement in our results at Netley during the fifteen months I have been in charge of the Neurological Section has been accompanied by a corresponding simplification of methods. We now realize that the only essentials for success in dealing with the hysterical cases, which constitute about a third of the war neuroses which come to us for treatment, is complete mutual confidence between the patients and ourselves. From the moment the patient enters, everything possible is done to convince him that he will quickly get well. It is explained to him that any treatment he has already received has prepared the way, so that nothing now remains but a properly directed effort on his part under our guidance for complete recovery to take place. During the whole course of treatment he is engaged in conversation, and the meaning of each successive step is explained. We have given up hypnotism for these cases, and only use such aids to suggestion as electricity and etherization in exceptional cases, being convinced that it is greatly to the advantage of the patient that he should co-operate intelligently in his own cure, and that everything in the way of mystery should be avoided. Our method can be shortly described as vigorous persuasion with the aid of manipulation. In the common variety of pseudo-spastic and tremulous paraplegia the patient lies on his back, and his legs are passively moved until complete relaxation occurs. He then co-operates by voluntary efforts, less and less help being given until he is able to perform the movements without difficulty and without any of the clonic spasms, which in many cases shook him from head to foot whenever he made an effort before the commencement of treatment. He next sits up, and the passive and active movements are repeated. When those are finished he is taught to stand, and as soon as his balance is perfect he begins to walk, at first with slight support, which is imperceptibly reduced until he suddenly discovers he is progressing without assistance. Among our last twenty cases of hysterical paraplegia all but one walked with a normal gait within a period varying between ten minutes and a couple of hours, although most had been lying in bed at other hospitals from three to eighteen months before admission to Netley. The one exception was a big fat man, who had been paraplegic for sixteen months, and required two hours of treatment a day for ten days before he could walk alone. We overcome hysterical contractures by forcible manipulation, however much the patient may complain of pain, the manipulation being repeated until complete relaxation occurs and the power to perform voluntary movements is restored—generally in the course of a very few minutes. Tremors are always accompanied by more or less rigidity: this too is overcome by passive movements and rubbing, the last traces of the tremor disappearing with the rhythmic performance of voluntary contractures of the affected muscles. We have learnt that it is fatal to discontinue until complete success is attained, so that much patience and energy are often required. My colleague, Captain J. R. L. Symns, at present holds the record of six hours' continuous treatment of a severe jaw tremor, which he finally overcame: if he had given up in despair at the end of a couple of hours, months of re-education would have been required.

III.

There is nothing exceptional about our results, as they are identical with those obtained by others who have had equal opportunities of learning how to deal with these cases by actual experience during the present War. But there are hundreds of men suffering from war neuroses scattered through the central and convalescent hospitals of England, where they spend many weary months without adequate treatment. At the end they are often invalided from the Service still suffering from their original symptoms, like two men admitted under Captain Symns, who were out working on a farm the day after their arrival at Netley, although only ten days earlier they had been awarded 100 per cent. pension for total incapacity due to hysterical paraplegia.

These men should be admitted direct from France into special hospitals, situated in the country, with special medical officers, who know how to create a true atmosphere of cure. Here, along with the neurasthenics and psychasthenics, they will quickly regain their strength and confidence in themselves by being given, when convalescent, healthy and interesting outdoor employment, instead of lounging about the wards and corridors of large general hospitals with no opportunity of taking exercise except in the streets. Many will in time return to military duty, whilst the remainder will be discharged from the Service after a few weeks or months, but never before they have recovered sufficiently to earn a living in some civil occupation of national importance.

Essentials of Treatment of Soldiers and Discharged Soldiers suffering from Functional Nervous Disorders.

By E. G. Fearnsides, M.D.

Before any soldier or discharged soldier can be treated for his functional nervous disorder, his case must first be diagnosed. An analysis of the discharge documents of the examples of chronic functional nervous disorder which have been sent to me for treatment at the "Home of Recovery," Golders Green, shows that quite 35 per cent. of the patients were invalided on a diagnosis which at first sight would not have led one to expect that these men were really examples of a functional nervous disorder, and my experience when going round various general hospitals, both in this country and in France has led me to believe that many soldiers who are admitted there suffering from this class of complaint are wrongly diagnosed or mis-diagnosed. In Germany, according to Naegeli[1] and Kuehn[2], there are probably more examples of neurosis in the general wards of military hospitals than there are in the nervous hospitals, and in these general hospitals the patients are wrongly diagnosed.

There is no single system which may not be affected by a functional nervous disorder, and if, in every single patient, the mental aspect of disorder and disease be not considered, the psychical nature of the complaints on the part of many patients may be completely missed. Perhaps the commonest mistakes which arise are those in which the diagnosis refers to muscles, nerves, joints, and the extremities. A very large proportion of the cases requiring special neuro-psychological treatment are labelled "myalgia," "rheumatism," "lumbago," "sciatica," "neuritis," "rheumatoid or osteo-arthritis," "trench feet," "frost bite," or by some other term bearing reference to peripheral structures and none to the actual seat of the disordered function. Other common mistakes in labelling are "gastritis," "dyspepsia," "dysentery," "appendicitis," "colitis," or some other of the diagnoses given to describe affections of the alimentary tract. The majority of patients diagnosed as suffering from "D.A.H." would be benefited by special treatment directed to the psycho-neurological aspect of their disabilities. "Bronchitis," "pulmonary tuberculosis," "effects of gas-poisoning," and other diagnoses suggesting physical changes in the pulmonary system are but too frequently attached to patients whose real trouble is of psychical origin. The number of mistakes made in calling functional nervous disorders organic diseases of the central or peripheral nervous system is probably much less. Diagnosis in this class of case, in my experience, has been more accurate.

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[1] *Neurolog. Centralblatt*, 1916, xxxv, p. 816.

[2] *Zeitschr. f. Med. Beamte*, 1917, xxx, p. 497.

Two causes for these mistakes in diagnosis in the case of patients suffering from functional nervous disorder seem worthy of comment: (*a*) The fact that in the whole of our medical education too little stress has been laid upon the mental condition of our patients and too much upon the physical, with the necessary consequence that medical officers in the Army rarely consider such an aspect before labelling their cases. During our student days minor deviations from normal functionings received but scant attention. Patients suffering from such trivial complaints are not admitted to the wards of ordinary civil hospitals, and in the casualty room or receiving department obtain little sympathy or treatment. (*b*) The amount of trouble entailed before the diagnosis in the case of our military patients may be altered. In the Army, before the diagnosis of a patient can be officially changed the routine completion of records of discharge for the one complaint and readmission for another must be performed, and until both these phases leading to failures in diagnosis are realized and made the subject of special inquiry, a large proportion of patients suffering from functional nervous disorder will remain wrongly diagnosed in general military hospitals without appropriate treatment and will there become chronic.

So long as military patients after minor injuries, due to gunshot wounds and other traumata, and after febrile illnesses, such as "trench fever," "malaria," "influenza," "dysentery," and the like, are not instructed that physical fitness is *ipso facto* lost by the change of habit and life entailed by the treatment for the local injury or general disease, amongst the nervously disposed many recruits to the class of patients suffering from chronic functional nervous disorder will accrue. Adequate inspection of every single patient in general military hospitals, command depots, reserve units and the like by a person with a psychological training and a knowledge both of man and of disease would, I think, well repay the military authorities.

The diagnosis of functional nervous disorder having been made, where ought the patient to be treated? The large number of cases of this sort which have arisen since the outbreak of war, and the wastage of man-power which this number has entailed, have led to the development of a series of schemes for the treatment of this class of patient. Universal experience, both in this country and amongst our Allies and our opponents, has led to the general acceptance of the necessity for removing them from too close association with examples of other classes of diseased and disabled soldiers. All those who have concerned themselves with the treatment of these men are agreed that special hospitals are a *sine qua non*. Schemes for the distribution of soldiers or pensioners in houses, with isolated families up and down the country, even from a purely administrative point of view, are impossible, and owing to the size of the problem could never deal adequately with it. In such houses, moreover, there is no atmosphere of cure and but rarely will the *milieu* be satisfactory. The majority of the world, like the majority of doctors, do not feel drawn to the nervously disposed, the emotional, or the self-centred. Special hospitals therefore are essential.

Hospital treatment is a system devised for the treatment of people whilst they are ill and does not lend itself to the rendering of a man fit either for military duty or for civil life. In civil life a man when he does not feel equal to a full day's work has some other choice between working and being in hospital. Under military conditions this possibility apparently has not been fully considered—a man must be either doing military duty of some sort or other, or be in hospital. There is no half-way house in the ordinary scheme of military service; and yet no person who has broken down with a functional nervous disorder is fit on the day that he leaves secluded "ward-life" for meeting the difficulties of the every-day life of a soldier of the New Army. The hospitals, therefore, must have attached to them grounds of sufficient size for the treatment of patients in the stages between hospital and duty.

Patients who suffer from functional nervous disorders in many instances will show evidence of disease or disorder of other portions of the body besides their central nervous system. No medical officer at the same time can be expected to have a sound opinion upon hearts, lungs, joints, eyes, ears, teeth, and be a good surgeon or an average dentist. The hospital, therefore, should be situated not too far from experts in other classes of diseases and injuries, and should lie not too far distant from a general military or large civil hospital. In the case of military hospitals these must lie near a railway and railway station or difficulties in transport and conveyance both for the patients and the consulting staff will be experienced.

Amongst the examples of functional nervous disorder two great groups, each requiring special consideration from the point of view of treatment are readily separable: (1) The passive suggestible *hysteric* who has shrunk into himself and developed fixed, circumscribed, obvious, objective disabilities and is usually best dealt with by a *force majeure* sent in as a stimulus from the external environment; and (2) the restless, discontented, hyper-sensitive, more delicately poised *psycho-neurasthenic* with his much less easily comprehended subjective worries, fears and bothers, ego-centric and yet expansive when approached sympathetically, who if treated in the coarse manner so adequate in the case of the hysteric will not only not be cured, but will be rendered well-nigh incurable. These two classes of patients do not react badly upon one another and may be treated in the same special hospital, but with success only if the methods appropriate to each are applied.

The treatment of every single patient who suffers from a functional nervous disorder must be individual and personal, and the medical officer in charge of each patient ought to be to him doctor, guide, friend, counsellor and leader. My own experience has proved to me that the official, casual, fleeting visit of the officer-in-charge of a hospital from the point of view of treatment is worse than useless. Such a visit undermines the authority of the patient's medical adviser and diminishes the patient's reliance and confidence in the attending physician's powers, and at the same time throws the responsibility for treatment on to his more senior colleague.

The results of treatment depend upon many factors, but above all upon the personal equation of the medical officer in charge of the patient—much more indeed upon this than upon the exact methods of treatment upon which he relies—a statement which has been voiced by authorities and writers in all parts of the world. Training teaches men many things, but training alone cannot make patients willing to follow the lead of their medical officers, instructors, teachers and nurses. An untrained man or woman will not see and will not find out the difficulties with which each single patient is faced, and yet training alone will not give this insight. Cure depends upon the influence of the medical officer in charge of the patient and upon the influence of those who live with the patients. The medical officer in charge of patients should, therefore, live with and develop an interest in the particular problems by which each individual patient is dominated. More than in any other class of disease or disorder it is physicians and living men and women who work cures. In this class of case the troubles are of psychological origin and require psychological treatment.

Over any particular period of time the rate of recovery in such a special hospital depends upon the *milieu* and atmosphere with which the patients during that period are surrounded. Every hospital given over to the treatment of cases of functional nervous disorders should be a community whose unity of aim is the restoration to full life of the patients undergoing treatment there, and for this end both patients and staff should work together. Rapid progress on the part of a single in-patient does more for the remaining patients than anything else with which I am acquainted to promote rapid cures. On the other hand the dragging on into chronicity of even a single patient reflects itself in the delayed recovery of others. No person who does not enjoy the work of treating cases of functional nervous disorder ought ever to be employed in such a special hospital. The nursing staff, orderlies, masseurs, instructors, &c., must all pull their weight in the hospital team or results will flag. The suggestive influence of the people around, their persuasive powers, their personal influence, are essential factors, and to get the maximum amount of benefit both men and women must be included on the staff. In such a hospital precedent is of great importance. In the case of civil hospitals, which long ago established their reputation, one finds that the results obtained in the treatment of every class of patient are better than in the "mushroom" hospitals which necessity is daily calling into being.

Ennui and the "hump" are great deterrent factors in the recovery of patients suffering from functional nervous disorders. For each patient each day should provide its own novel and complete programme. Occupations of the type of needle-work, mat-making, wood-carving, jig-saw puzzles, and the like, should be provided for "bed" patients, but no patient should be allowed to pursue such "feminine" employments as soon as he is allowed to get about freely. Instruction in subjects likely to help the patient in his future military or civil life, both orally and from books, I have found a great asset. Entertainments of some sort for the patients as a whole, and more especially those with a competitive aspect, in which the patients themselves take part, greatly increase the rate of recovery. As soon as the patients are allowed to get up and go about, outdoor occupations and games ought to be arranged. Physical drill and gymnastic displays are extremely useful, but training in some occupation likely to be of use to the patient in after life has provided the most rapid and most stable recoveries. For patients who are not going back into the fighting ranks instruction in workshops is a necessary part of the treatment in this class of case, and for those whose civil and military work will in the future lie out of doors, work on the land (gardening, farming, &c.) should be provided. I know of no greater incentive to work than the production of a tangible something which can be displayed to admiring friends, relatives and fellow patients, but money or extra leave are also excellent inducements.

In the treatment of all diseases, as Hilton in this country first insisted and workers in sanatoria for the treatment of phthisis have detailed to the full, there must be a true balance kept between the amount of time spent in resting and in following the business of active life. As a general rule, the psychoneurasthenic patient hates resting, which is good for him, while the hysteric is contented to lie abed all day. The hysteric requires driving into life, the psychoneurasthenic requires leading into it.

The exact mode in which any form of treatment is applied will and must always depend upon the training and previous experience of the medical officer in charge of the patient. This officer must establish a *rapprochement* with his patient: as to how he attains this matters but little. He should learn to know his patient or else he cannot cure him, though he may be able to remove many of the patient's grosser disabilities. In consequence of this necessity for personal attention, too many patients upon his hands will make his results poor. Either he will have to slur over some cases, or he will not have sufficient time to deal adequately with all. In general fifty patients per medical officer will be a maximum.

Schematized psychanalysis, as a mode of treatment, has its uses, but some method by which the patient and his medical adviser come to know one another is essential; and, therefore, modified psychanalysis, if so it be termed, is essential. Here, in passing, the use of dreams in diagnosis and in helping to elucidate the divers problems of psychological origin with which the patient is faced may be mentioned. In the hands of some hypnotism and hypnotic suggestion have been successful, in the hands of most of little value. Suggestion in some form or other as a part of treatment in all this class of case is essential, whether used by means of strong faradism, by personal contact or during hypnosis. Massage, electricity (high frequency, faradism, static breeze, &c.), baths (sinusoidal, *eau courante*, hot and cold douches, &c.), radiant heat, remedial exercises, gymnastic exercises and physical training, all have their place in treatment, are useful but secondary adjuncts, and all should be at the disposal of those treating cases of functional nervous disorder in special hospitals. Workshops, in my opinion, form an absolutely essential part of the equipment of such hospitals, and of such workshops in this country, in France and in Germany those given over to wood-work and carpentry have been found the most useful. Carpentry provides exercises for both muscles and joints, and re-educates a man's judgment, power of concentration, and self-control, and at the same time enables the patient to see that he still has his place in society.

Treatment on the lines described, from October 1, 1917, to April 30, 1918, has yielded the following results:—

	Oct.	Nov.	Dec.	Jan.	Feb.	March	April	
Patients medically unsuitable for Golders Green	5	2	3	9	5	5	3	32
Patients who resented treatment ...	7	1	3	1	—	2	4	18
Left for good family reasons ...	—	1	1	1	—	—	—	3
Disciplinary discharges	—	2	1	1	—	—	—	4
	12	6	8	12	5	7	7	57
Practically attained pre-war standard	—	4	—		—	1	1	6
Fit for work	17	9	3	10	—	2	—	41
Returned to former employment by arrangement with former employers	15	9	10	12	7	2	14	69
Went to new work	8	9	6	5	5	5	7	45
Went to further training in skilled trades arranged through the Ministry of Pensions	—	1	2	6	5	11	3	28
	40	32	21	33	17	21	25	189
			Total number of discharges				...	246

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VOLUME THE ELEVENTH

SESSION 1917-18

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1918

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SECTION OF OBSTETRICS AND GYNÆCOLOGY.

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Section of Obstetrics and Gynæcology.

President—Dr. G. F. Blacker.

Report of the Committee appointed by the Section of Obstetrics and Gynæcology to investigate the Effects of Scopolamine-morphine Narcosis, "Twilight Sleep," in Childbirth.[1]

Members of the Committee:

Dr. John Fairbairn, St. Thomas's Hospital and York Road Lying-in Hospital.
Dr. Eardley Holland, City of London Lying-in Hospital.[2]
Dr. Hubert Roberts, Queen Charlotte's Hospital.
Mr. T. G. Stevens, Queen Charlotte's Hospital.
Dr. Herbert Williamson, St. Bartholomew's Hospital.

Scopolamine is a lævo-rotatory alkaloid first isolated from *Scopolia atropoides*, but obtainable also from *Hyoscyamus niger*, *Duboisia myoporoides*, *Atropa belladonna* and other members of the Solanaceæ group. It is closely related to hyoscine, but whereas some specimens of hyoscine are dextro-rotatory, scopolamine is lævo-rotatory. The drug is administered in the form of the hydro-bromide salt, a body which crystallizes in rhomboids and is soluble in water and alcohol.

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[1] Presented at a meeting of the Section, held December 6, 1917.

[2] Dr. Eardley Holland attended two of the earlier meetings, but owing to military duties he was unable to take part in the investigations. Mr. Comyns Berkeley and Mr. Gordon Ley presented a report from the City of London Lying-in Hospital in place of Dr. Eardley Holland.

It is difficult to isolate, and tends to deteriorate rapidly in solution. It is probable that the varying results reported from different clinics where scopolamine-morphine narcosis has been employed in childbirth depend not only upon the technique of administration, but also upon the particular preparation of the drug used. We felt therefore that our investigations should be conducted with a stable preparation of the drug of a specific rotatory power.

In 1913 Straub showed that the sugar mannite possessed the property of preventing decomposition and racemization of scopolamine, and that in a 10 per cent. alcoholic solution of mannite scopolamine hydrobromide remained therapeutically active for an indefinite period.

We decided, therefore, to use a mannite-alcoholic solution of scopolamine of known lævo-rotatory power. Messrs. Burroughs Wellcome and Co. undertook the preparation of our solution, and we wish to express our indebtedness to this firm for the help they have given us in the preparation of the drug, and for their kindness in supplying it at a much reduced price to the hospitals where this investigation was carried out.

Each dose was put up in a separate glass container and the solution prepared as follows: The mannite was dissolved in distilled water, filtered, and the solution then sterilized by boiling. The solution was cooled, the alkaloid and alcohol were added, and the solution diluted to the requisite volume with sterilized distilled water. The glass containers before being filled were heated to 100° C. for half an hour, and the burette and filling apparatus were washed out with distilled water before use. Two separate sets of ampoules were prepared, one containing $\frac{1}{150}$ gr. of scopolamine hydrobromide for the initial dose, the other containing $\frac{1}{450}$ gr. for subsequent doses. The alkaloid employed possessed a specific lævo-rotatory power of 22°.

In many of its physiological and pharmacological properties scopolamine resembles atropine: it dilates the pupils, paralyses accommodation, inhibits secretion, and paralyses the nerve endings of the cardiac vagus. Its action on the respiratory centre is in marked contrast to that of atropine, for whereas atropine in physiological doses is a respiratory stimulant, scopolamine is a respiratory depressant. It is therefore necessary to note the pulse- and respiration-rate before each injection, for a slowing of the respiration or an increase in the pulse-rate may be the first indication of over-dosage or of an idiosyncrasy towards the drug on the part of the patient.

Scopolamine further exerts a sedative action on the unstriped muscle of the urinary tract: this action is often marked in scopolamine-morphine narcosis, and unless watched the bladder may become so distended as to interfere with the course of labour. The use of the catheter is often necessary.

The action of scopolamine on the nervous system differs from that of the common hypnotics. These drugs act by dulling perception and by cutting off sensory impulses; scopolamine does not act in this way: sensory impulses are still perceived, and hence it is necessary in inducing narcosis to screen the patient as far as possible from sense impressions from the outside world, as for instance by plugging the ears and covering the eyes. The precautions we adopted in this series of observations will be described later.

The first effect of the action of the drug is a pronounced weariness associated with muscular relaxation, and this stage is sometimes followed by motor-restlessness. Soon the patient passes into a sleep from which she is awakened by each successive pain; at this stage the pains are clearly perceived both objectively and subjectively, but the amount of suffering they cause is usually diminished. In the majority of patients a further effect is produced, and whilst under the influence of the drug sense impressions, although perceived, are no longer registered in the memory cells of the brain. There is complete loss of memory for all events taking place during the period the patient is under the influence of the drug. Perception is not abolished, but "apperception"—the power of storing the perceptions—is lost.

Scopolamine-morphine narcosis was introduced into obstetric practice by Von Steinbüchel; he employed it to diminish pain, and to produce anæsthesia. In 1906 Gauss[1] described a new method of using the drugs and introduced the term "twilight sleep." It is essential to understand clearly that what is meant by "twilight sleep" is neither analgesia nor anæsthesia, but a condition of amnesia. In twilight sleep sensation is still present though in diminished degree; the patient feels the pains of the uterine contractions, frequently she moans, draws up her legs, and in other ways shows that she is suffering, but these painful sensations are not recorded in the memory cells, and after the influence of the drug is removed, the patient will vow she never experienced them. If asked a question, she will answer often in a dazed and confused fashion, but later will have no recollection of the question having been asked—perception is not abolished, but apperception is lost. When this condition of complete amnesia is reached, the goal at which Gauss aims has been attained. Gauss saw clearly that no standard dosage could be adopted since patients vary greatly in their reaction to the drug; if the dosage be too small, the amnesia will be incomplete, and there will be "islands of memory" from which the patient may reconstruct the whole picture; if it be too great, poisonous effects will be produced in both mother and child. He believes he has solved the problem by following the technique which he first described in the *Archiv für Gynäkologie* in 1906, and we adopted that technique as the basis upon which to found our observations.

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[1] *Archiv f. Gynäkol.*, 1906, lxxviii, pp. 579-631.

In his paper Gauss asks: "What qualities should be required in a narcotic which is to be employed in spontaneous physiological births?" And he answers it thus:—

(1) There must be a marked reduction of pain.

(2) It must be free from unpleasant after-effects, or if such effects are inseparable from the action of the drug, they must be harmless to the patient.

(3) The normal progress of the labour must not be interfered with during the dilatation of the cervix, the birth of the child, the separation and expulsion of the placenta or the arrest of hæmorrhage.

(4) The child must not be injuriously affected in intra-uterine life; after delivery there must be no interference with the initiation of its physiological functions or with its later development.

The Committee at their first meeting decided to test the value of scopolamine-morphine narcosis in a series of labours conducted in the wards of hospitals to which they were attached, and to ascertain how far the results demanded by Gauss were obtained.

It was decided that in the first observations the technique of Gauss should be adopted, but that it should not necessarily be followed in later cases. It was agreed that a standard solution of scopolamine should be employed, but the initial dose should consist of $\frac{1}{4}$ gr. of morphia and $\frac{1}{150}$ gr. of scopolamine, and that subsequent doses should consist of $\frac{1}{450}$ gr. of scopolamine. The patient was to be isolated during the labour, and as far as possible precautions were to be taken to avoid sensory impulses. The cases were to be selected by the officer in charge of the ward, and the patients were preferably to be primiparæ, in whom no complications were anticipated. Two special charts were drawn up for use in the labour room, and at each injection the following observations were to be recorded:—

CHART I.

Precautions taken to exclude sensory impulses.
Time of injection.
Drugs employed and doses.
Effects on uterine contractions. Frequency, strength, and duration of pains. Presence or absence of voluntary expulsive efforts.
Frequency of maternal pulse and respirations.
Rate of fœtal heart.
Subjective symptoms: Pain, thirst, nausea.
Objective symptoms: Sleep between the pains, sleep during the pains, flushing of the face, vomiting, mental confusion, restlessness, excitement.
Condition of reflexes: Bakinski's reflex, pupillary pain reflex, dilatation of pupil, reaction to light.
Memory of object shown.
If a general anæsthetic was employed, state anæsthetic used and duration.

In CHART II *the following questions were asked:—*

Was the placenta expelled from the uterus spontaneously?
Was bleeding greater than usual?
State amount, if estimated.
Did the child breathe or cry spontaneously?
Was breathing irregular or shallow?
How long after birth before normal breathing was established?
What method (if any) of resuscitation was adopted?
What colour was the baby shortly after birth?
Was the cord pulsating when the child was born?
What was the condition of the patient after delivery?
What was the duration of sleep after delivery?
What degrees of amnesia and of analgesia were attained?
What was the patient's own account of her labour?
Was the course of the puerperium normal?
What was the condition of the child on leaving hospital?

The observations were carried out independently in four hospitals. St. Bartholomew's Hospital, St. Thomas's Hospital, Queen Charlotte's Lying-in Hospital, and the General Lying-in Hospital, York Road.

The results obtained in each of these hospitals are recorded in a separate report.

REPORT ON TWENTY CASES AT ST. BARTHOLOMEW'S HOSPITAL.

From St. Bartholomew's Hospital we are able to report only twenty cases in which the treatment was carried out under sufficiently favourable conditions to justify us in drawing conclusions as to the value of the method.

Precautions to avoid Sensory Impulses.—In each case the labour was conducted in a separate ward; the room was darkened; the patient's ears were plugged with cotton-wool, and her eyes were shaded. The labour-room was kept absolutely quiet, and the passage leading to the room as quiet as possible. In many cases no vaginal examinations were made, and when they were essential, they were made with as little disturbance as possible. The patient was never left without a nurse, but it was only in five cases that she showed signs of excitement or attempted to get out of bed. In none of these cases was it necessary to discontinue the injections or to use forcible restraint.

Dosage.—The initial dose was given when the cervix was dilated sufficiently to admit a finger and the pains were recurring regularly at intervals of not less than a quarter of an hour. In one case, a girl aged 14, we were particularly anxious that no recollections of the labour should remain, and consequently commenced the injections at an earlier stage. In another case, a multipara aged 42, who had already been in labour for six days, we commenced the administrations when the pains were weak and irregular, in order that we might test the effects of the drug upon uterine inertia. With these two exceptions the conditions stated above were complied with in all cases. The first dose consisted of $\frac{1}{4}$ gr. of morphia hydrochloride and $\frac{1}{150}$ gr. of scopolamine hydrobromide, the preparation supplied by Messrs. Burroughs Wellcome and Co. being used. The morphia was first injected and then without withdrawing the needle the scopolamine was administered. Each subsequent injection consisted of $\frac{1}{450}$ gr. of scopolamine hydrobromide, and in no case was the morphia repeated. The smallest number of injections given was three, the largest fourteen, and the average 6·2.

One hour after the administration of the initial dose $\frac{1}{450}$ gr. of scopolamine was given, and twenty minutes later an object with which the patient was not familiar was shown to her; we usually employed for this purpose a small christening font which was kept in the ward.

From this point onwards we employed the memory test alone, following rigidly the technique recommended by Gauss. At the end of an hour the font was shown to the patient a second time; if she recollected it clearly a third injection was given; if she failed to recognize it, another hour was allowed to elapse before the injection was repeated. In our later cases we varied the object shown from time to time, fearing that by repeatedly drawing the attention of the patient to the font during the stage of induction, the memory of it might become so deeply impressed upon the cells of her brain as to vitiate the test. The second object shown was usually a forceps blade. We were able to observe in some cases the gradual clouding of the patient's consciousness and the confusion of mind which supervened before the power of apperception was completely lost. For some patients, ecclesiastical associations clustered round the font, as in the case of one patient who, after gazing at it for some time, expressed the opinion that it was a pew; another thought it had something to do with dead babies. In a third case the font called up associations less ecclesiastical, and the patient remarked: "Thank you, I will have another drink." In these borderland cases the injection was not repeated. Sometimes the failure to recognize the object was complete, and in answer to the question, "Have you seen this before?" replies were given such as "It's the wax doll out of the kitchen," and "It means wet weather."

The number of cases is not sufficiently large to enable me to draw definite conclusions with regard to the value of the memory test as a guide to dosage. I may state, however, that where the memory of the object was lost and not regained during the labour, amnesia was complete. As an instance of this I may mention the case of the patient who thought the font was a wax doll. She was a primipara; the labour lasted forty-six hours; the first injection was given on December 8, at 12.15 p.m.; after the second injection her mind became confused and after the fourth injection she failed to recognize the object and never regained apperception. The first stage was completed at 9.15 p.m., and after the second stage had lasted three hours, she was delivered by forceps under chloroform. Apparently the suffering was not diminished at all; she slept between the pains but awoke with every pain, cried out and complained bitterly, so much so that Mr. Blount, who was conducting the labour, regarded the case as a failure. When questioned the next day, the patient said, "It was one of the happiest days of my life. I was wandering in a beautiful garden all day and did not feel any pain after the second injection. I did not know that I had

had chloroform or that the child was born until I wakened up in bed."

On the other hand there were four cases in which the object was clearly recognized each time it was shown, and nevertheless amnesia was attained. As an example, I may quote the case of E. G. This patient had previously borne five children, and was admitted on February 15 of this year on account of ante-partum hæmorrhage in the thirty-third week of her pregnancy. The labour lasted nine hours. The first injection was given at 5 p.m., the sixth and last at 11 p.m. On the next day I made the following note: "The patient recognized the objects shown throughout the labour, but amnesia was complete from the second injection until the stimulus of the birth of the head brought the patient out of the twilight sleep and she remembered the last two pains. Thus the memory test in this case was no true guide as to the degree of amnesia produced. Amnesia was achieved although apperception was present throughout." The patient's own words were: "I remember the first two injections and nothing more until the last two pains, when the baby was being born."

There were three cases which illustrate what Gauss called "islands of memory." After apperception had been lost, too long a time was allowed to intervene between the injections, and intervals occurred in which apperception was regained. From these memory islands the patient was able, more or less, to reconstruct the course of the labour, and stated next day that she remembered everything. From this small series of cases, I formed the opinion that the memory test is a safe guide but not an absolutely reliable one, for there are instances in which apperception is present throughout, but amnesia is complete. I believe the memory test is the best guide to dosage we possess, but it demands considerable experience and powers of close observation in the administrator. Its value is diminished, however, by the necessity of arousing the patient each time it is applied.

Two alternative tests have been suggested—namely, Babinski's reflex, and the pupillary pain reflex. These were tried in all cases. There is no doubt that when the patient is under the influence of scopolamine, the extensor response of the great toe to stimulation of the sole of the foot is obtained in many cases; we obtained it, however, frequently when amnesia was not achieved and failed to obtain it in some cases where amnesia was complete. It is therefore of little value as a guide to dosage. The pupillary pain reflex is of no greater value; the pupils are always widely dilated, and it is seldom that we observed further dilatation in response to the uterine contractions; we could trace no relation between amnesia and loss of pupillary pain reflex.

Effect of the Drug upon Uterine Contractions.—It is difficult to measure the strength of a uterine contraction, and we made no attempt to do so in a strictly scientific fashion. After each injection we charted the frequency of the contractions, the duration of the contractions and the strength of the contractions as estimated by placing the hand upon the uterus. The contractions were charted as "strong," "fair," or "poor." We found that if the pains of the first stage were good before the administration was commenced, little effect was produced; if they were weak—that is, if primary uterine inertia was present—injection of the drugs sometimes led to their entire cessation. The most marked instance of this was the case of E. H., aged 42, the mother of eight children; her labour lasted for five days and ten hours. Throughout the first stage the pains were weak and irregular, but caused severe suffering. Labour commenced on January 19 and continued until January 23; on this date scopolamine-morphine injections were commenced at 2.30 p.m., when the pains were coming regularly at intervals of ten minutes. The first four injections produced no obvious effect; after the fifth injection mental confusion was marked and she talked nonsense. From this time the pains became less frequent, and after the seventh injection at 12.20 a.m. on January 24 ceased altogether. The treatment was therefore abandoned. The pains recommenced at 9.15 a.m. and Champetier de Ribes's bag was introduced. The pains continued all day. The bag was expelled at 9.10 p.m. and ten minutes later the child was born spontaneously. The third stage lasted two hours and forty minutes, and at the end of this time the placenta was removed manually on account of hæmorrhage. Our experience thus confirms the conclusion reached by Gauss, that in uterine inertia scopolamine-morphine narcosis should be avoided, for under its influence the pains become weaker and sometimes cease entirely.

In our twenty patients, the pains of the first stage were apparently unaffected in thirteen, became stronger in three, and weaker and less frequent in four. It is worthy of note that out of the four patients in whom the pains became weaker, three were multiparæ. In general terms, we found that in the absence of uterine inertia little effect was produced upon the strength, frequency or duration of the contractions in the first stage.

The second stage was distinctly prolonged, especially amongst the primiparæ. This was due to the lack of voluntary expulsive efforts on the part of the patient, but in three patients there was also a weakening of the uterine contractions. When neither mother nor child showed signs of distress, we did not hesitate to allow the second stage to continue for five or six hours, but in spite of this, eight out of the fourteen primiparæ failed to deliver themselves spontaneously—seven children were extracted by forceps and one by traction on the breech. It should be stated that amongst these eight primiparæ were three cases of minor degrees of pelvic contraction, two with a true conjugate of $3\frac{3}{4}$ in., and one with a true conjugate of $3\frac{1}{2}$ in. In the last case, labour was induced in the thirty-ninth week.

Of the six multiparæ, five were delivered spontaneously and one by the forceps. Thus in 45 per cent. of the cases a general anæsthetic was administered and the labour terminated artificially. The average duration of the second stage in the primiparæ was four and a half hours. In the multiparæ it was one hour and thirty-five minutes.

Effects upon the Third Stage.—We found that the third stage of labour was prolonged, the average duration being fifty-five minutes. The maximum time occupied was four hours, the minimum ten minutes. The placenta was expelled into the vagina spontaneously in sixteen cases, it was expelled from the uterus by pressure upon the abdomen in three cases, and was removed manually in one case. In three cases the bleeding was in excess of the normal amount, 18 oz. in the first case, 14 oz. in the second, and 16 oz. in the third. The bleeding was never sufficiently profuse to cause alarm, and was stopped in each instance by the administration of an intra-uterine douche and an injection of pituitrin. All three patients had been delivered under general anæsthesia, two by forceps, the third by extraction by the breech.

In three cases there was relaxation of the uterus three or four hours after the completion of labour, with a loss somewhat greater than the normal. In one case the loss was sufficiently great to call for a hot vaginal douche and an injection of pituitrin. A review of the cases leads to the following conclusions:—

(1) The third stage was prolonged.

(2) There was no severe post-partum hæmorrhage during the third stage.

(3) There was a temporary relaxation of the uterus four or five hours after delivery in three cases.

It will be noted that 45 per cent. of the patients were delivered artificially. The high percentage is not to be attributed altogether to the scopolamine-morphine narcosis, but is due in part to the fact that many patients are admitted to the ward because difficulties in delivery are anticipated, and consequently there is a high proportion of abnormal labours. In this series there were four cases of minor degrees of pelvic contraction, one case of ante-partum hæmorrhage, two cases of induction of labour, one case of breech presentation with extended legs, one case of primary uterine inertia, and one girl of 14 years of age. I am of opinion, however, that some of the patients in whom forceps were applied would have delivered themselves spontaneously had voluntary bearing-down efforts been present, and therefore that scopolamine-morphine narcosis increased the number of instrumental deliveries.

Maternal Pulse and Respiration.—We found no tendency to increase of pulse-rate, and with the doses employed we had no reason to suspect inhibition of the cardiac vagus in any of the cases. In one case the respirations became slow and shallow after the third injection, and no more injections were given, but with this exception the respiration remained normal throughout when compared with a similar series of labours in which no narcotics were administered.

Thirst was complained of by almost all the patients, and they were allowed to drink freely of water, barley water, tea, or imperial drink. Nausea and vomiting were present in four cases, absent in sixteen. Distension of the bladder, with inability to pass urine, necessitating the use of the catheter, was present in six cases.

Flushing of the face and dryness of the skin were marked, but never caused much distress or discomfort.

Restlessness was common during the stage of induction, and in several instances the patient attempted to get out of bed, but never had to be forcibly restrained.

Sleep.—Seventeen of the patients slept in the intervals between the pains throughout the greater part of the labour; two were very drowsy, but did not sleep, and one was wide awake throughout, but in this patient amnesia was complete. Six of the patients were not awakened by the pains during some part of the labour, but all the others woke up with every pain. After the labour was over, all the patients slept; the average duration of the sleep was four hours.

Analgesia.—Analgesia was marked in twelve of the patients, slight in five and absent in three. Upon the whole, the effect of the drug in this respect was striking, and amongst those in whom amnesia was not attained, the majority stated that the pain was much less after the injections. We could trace no definite relation between amnesia and analgesia; in some cases in which complete amnesia was attained, there appeared to be no diminution in the amount of suffering, whereas in others in whom the analgesia was marked every incident of the labour was clearly remembered.

Amnesia.—Complete amnesia was attained in eleven cases, that is in 55 per cent. By "complete amnesia" I mean that on the following day the patient could remember nothing that happened after a particular injection, and was unaware that the baby was born until she awakened up in bed after her sleep. Immediately after the labour was over, some of these patients were shown the baby, and recognized it as their child, but on the next day had no recollection of the incident. In four cases, or 20 per cent., amnesia was incomplete—that is, the patient recollected certain incidents, but did not recollect others. In these patients there were "islands of memory," and some of them said they "remembered everything," but on questioning it became clear that they were oblivious of the greater part of the labour. From these "islands of memory," however, they reconstructed what they regarded as a complete picture of the whole labour. In five cases, or 25 per cent., there was no amnesia, and the whole course of the labour was clearly remembered.

Effects upon the Child: Effects upon the Fœtal Heart before Delivery.—In sixteen cases the rate of the fœtal heart-beat was not affected, its rhythm remaining the same during the course of the injections as before their commencement. In four, there was an increase in the frequency of the beat, but I am doubtful whether this was the result of the drug or due to the labour itself. It was not found necessary to stop the injections because of the condition of the child in any one of the cases.

Condition of the Child at Birth.—One of the children was stillborn; nineteen were born alive. The stillbirth was not to be attributed to the scopolamine-morphine narcosis. The mother was a primipara with a generally contracted pelvis. The pelvic measurements were: interspinous, 9 in.; intercristal, 10¼ in.; external conjugate, 7 in.; diagonal conjugate, 4 in.; estimated true conjugate, 3½ in. Attempts at external version failed, and labour was induced in the thirty-ninth week. The child lay in the first position of the breech; the first stage lasted twenty-one hours, and after a second stage of two hours the breech became firmly impacted in the pelvis with both legs extended. After considerable difficulty the child was extracted by the fillet. Of the nineteen children born alive, eight were born in a condition of oligopnœa: this condition is undoubtedly due to the combined action of the morphia and scopolamine. The child usually gives a cry as soon as it is born, it then makes no further attempt at spontaneous respiration. It becomes first blue, then pale in colour; the frequency of the heart-beat falls in some cases to 80 beats a minute, and the heart's action sometimes becomes irregular. After a time varying from ten to twenty minutes, slow and shallow respirations commence: these gradually become deeper and more frequent, and in a few minutes normal respiration is established. In our earlier cases this condition of oligopnœa caused us much anxiety—we labelled the cases "white asphyxia," and treated them as white asphyxia is always treated in my ward. The air passages were cleared, the child's body was immersed in a hot bath, and slow rhythmical compression of the thorax practised. I find in our twenty cases that eight children are described as born in a condition of white asphyxia; nearly all these were amongst our earlier cases before we were well acquainted with the phenomena of oligopnœa, and I am quite sure that in view of our later experience we should have labelled the majority of them oligopnœa. With fear and trembling we ventured to leave these children alone, and to abandon the bath and compression of the thorax; we found that in the absence of all treatment normal respiration was established in the course of from ten to twenty minutes. On analysing the charts I find the following returns:

	Cases
Breathed normally immediately after birth	3
Born in a condition of oligopnœa ...	8
Born in a condition of white asphyxia ...	8
Stillborn	1
	20

But, as I stated just now, some of the earlier cases of white asphyxia should undoubtedly have been classified as oligopnœa. We found, therefore, that most "twilight sleep" babies are born in a condition of oligopnœa, and that although it appears alarming, the condition is transient and need not cause anxiety. No special treatment is required beyond covering up the child and keeping it warm. In three cases—and these were all forceps-deliveries—we had to deal with a condition of true white asphyxia; the child was deathly white, its muscles flaccid, the reflexes absent, and the heart acting feebly; under the treatment described above all these children recovered.

History of the Children after Birth.—Of the nineteen children born alive, all left the hospital well. In the majority we could detect no evidence that the scopolamine had produced any effect upon the child; three or four were reported as unusually sleepy for the first twenty hours, but were normal on the second day. The gain of weight, the action of the bowels, the separation of the cord, and the general behaviour of the infants certainly did not compare unfavourably with the children of mothers to whom no narcotic was administered. It has been stated that scopolamine is excreted in the milk; we found no evidence of this. Professor Bainbridge in several cases examined the first milk secreted for scopolamine, but did not find dilatation of the pupil where it was dropped into the eye of a cat. Of the development of the children subsequent to leaving hospital I am unable to give any information.

The Puerperium.—We found that the puerperium and period of recovery was not affected adversely by the injections. All the patients were able to leave hospital at the usual time. In one patient the lochia were offensive and the temperature was raised for forty-eight hours; in two cases there was small superficial sloughs of the perinæum; in the other patients the puerperium was normal. We formed the opinion that after long and trying labours the "twilight sleep" patients made a quicker recovery than those who had been delivered without it.

Lactation was not affected unfavourably: 75 per cent. of the patients were able to nurse their children—a proportion rather higher than the average of the ward for twelve months.

Such are the facts of these twenty deliveries under scopolamine-morphine narcosis; doubtless they admit of different interpretations, but if I am to comment upon our experiences, I will say that they illustrate both the advantages and the disadvantages of the method.

I have no doubt that in the majority of cases the pangs of childbirth were materially lessened, and that in more than half the cases the memory of the greater part of the labour was completely obliterated. On the other hand, the duration of the labour was lengthened and the proportion of instrumental deliveries increased. No serious adverse effects were produced upon the mother, excepting that we found some tendency to relaxation of the uterus after delivery, but not sufficient to cause anxiety in any case.

With regard to the children, I cannot say that any child suffered harm from the injections, but some of them gave us considerable anxiety until we learnt to recognize the phenomena of oligopnœa. I am convinced that these phenomena are due to the depressant action of scopolamine upon the respiratory centres of the child, and that any excess of dosage is likely to be attended with disaster.

The demands made upon the medical attendant are great; the patient must be closely watched and not left alone for a minute whilst she is under the influence of the drug. She has to be seen at intervals of an hour over a period which may extend to sixteen or seventeen hours; the attendant must be prepared to remain with her during a second stage of four or five hours, and finally may be called upon to remain for two or three hours after the labour is over. These conditions are possible in a hospital where relays of skilled assistants are available, but with rare exceptions they are not possible in the home of the patient. The majority of women, if they wish to be delivered under "twilight sleep," must be prepared to leave their home and enter a maternity clinic.

REPORT ON EIGHTY CASES AT ST. THOMAS'S HOSPITAL.

Preliminary.—In Mary Ward, St. Thomas's Hospital, eighty cases were specially observed for the purpose of this investigation, the procedure as formulated by the Committee being followed. In a few of the later cases a slight modification in dosage was tried, to which reference will be made later. As considerable experience had previously been gained by house-officers and nursing staff in selected cases conducted in the small labour ward, it may be worth while to draw attention to the point that the observations in this series conducted at the request of your Committee were made by those who were already acquainted with this method of partial narcosis. Special arrangements were, however, made for the purpose of this investigation and more attention paid to details.

Precautions to exclude Sensory Impulses.—The observation ward was adapted for a labour room. It is on the river side of the hospital—i.e., the quieter side, and is disconnected from the main ward by a short corridor with doors at each end. Rugs were laid on its terrazzo floor to minimize noise and green curtains and blinds put up over the windows to exclude light. Visits to, and examinations of, the patients were reduced as far as possible and efforts made to render all necessary procedures in connexion with the labour as little disturbing as could be. To avoid constant coming and going a special nurse was continuously on duty with each patient. The usual plugging of the patient's ears to exclude sound was carried out in all cases.

Cases selected.—With the idea of obtaining a more accurate judgment as to the merits of this form of narcosis in general obstetric work, and excluding other factors which might affect the result as regards mother and child, cases of normal labour were preferred. Naturally there is an undue preponderance of primiparæ, partly because the proportion of such cases admitted to the ward is much above the average of ordinary practice, but more especially because the first labour affords better opportunities for selecting the right moment to begin and a longer time for the induction of narcosis.

Instructions to begin Administration.—The instruction to wait for the establishment of regular pains at about five-minute intervals and dilatation to admit two fingers was taken as roughly indicating the time to begin. Without too frequent vaginal examination it was not possible to be sure of the precise moment, as patients vary greatly in the rapidity of onset of labour. In nervous and excitable primiparæ the tendency is to start the narcosis too early, whereas with a placid phlegmatic multipara labour was often found to be unexpectedly far advanced to allow sufficient time for proper induction. Undoubtedly in many cases in which labour progressed rapidly it was evident that the administration had been too long delayed, and there will always be a difficulty in determining the most favourable moment to start. It is just in these strong and rapid labours that some blunting of sensation and blotting out of the recollection of what it means is most to be desired, and unless the injections are begun too early as a routine there is no means of recognizing such cases without making frequent vaginal examinations.

Analysis of Cases.—Of the eighty cases, sixty were primiparæ and twenty multiparæ. The number of injections varied from one to nineteen, the average being 6·5. In the primiparæ the average was 7·2, in the multiparæ 5·5.

Dosage.—The drugs used and the dosage were as determined by the Committee, except in a few of the later cases when the second injection was $\frac{1}{300}$ gr. of hyoscine instead of the usual $\frac{1}{450}$ gr. The hyoscine injections were given at hourly intervals after the combined morphia and hyoscine injection until some idea had been obtained of the patient being sufficiently under the influence of the drugs to allow the intervals to be lengthened. The judgment as to dosage proved to be the most difficult part of the administration. As had been discovered in a previous experience, the "memory test" proved quite useless. Whether due to an inability to carry out the test properly or to some inherent peculiarity in the Lambeth women is not easy to decide—probably both factors played a part. If the patient was roused sufficiently to bring her muddled wits to bear on the problem, her sporting instincts were aroused, she entered into the spirit of the game and nearly always guessed right. In but two cases out of the eighty was the memory of objects shown completely lost; one of these had seven and the other only three injections, the duration of labour from the time of the first injection being nine and a half and four and a half hours respectively. Both patients slept for some hours after labour and though amnesia and analgesia had evidently been complete in both cases, all that the one would confess to was that "it was easier than she expected," and the other stated emphatically that she "didn't like it."

The behaviour of the patient between and during the pains, and the notice she took of the prick of the hypodermic needle, were found to be more useful than the memory test and, moreover, had the great advantage of involving no additional disturbance to the patient. The common occurrence was that the patient after the first two injections slept between the pains but woke up and became a little restless with each contraction, relapsing into drowsiness as it passed off. If the next injection was unnoticed or if sleep continued throughout the pains, further administration was omitted until she was roused by the pains and began to take notice of what was going on. It was found that the injection due two or three hours after the first could often be omitted. As an instance: In No. 46, in which complete amnesia was obtained, the labour lasted sixteen hours from the time of the first injection, but only nine injections were required, those due two, five, seven, ten, twelve, fourteen and fifteen hours afterwards being omitted. In another case (No. 4) labour lasted four and a half hours from the first injection, and three injections were given, the second one hour, and the third three hours after the first and one and a half hours before delivery. This woman was asked at the moment of the birth of the head if she knew what was happening and said, "No, doctor, but did mummy get the coals." She slept for eight hours afterwards and had no recollection of anything from the time of the original injection. Our method was, therefore, to begin with hourly hyoscine injections, and when the drowsiness and absence of response to the pains, injections or other manipulations indicated that the woman was fully narcotized to omit one or more of the hourly injections.

Amnesia and Analgesia.—The success of the administration is chiefly to be judged by the amnesia and analgesia produced, but as there is no exact means of estimating these effects they have been roughly classified as follows:—

Amnesia is classed as "complete" when the patient remembered nothing from the time of the first injection; as "partial" when she had no clear recollection of her labour but retained some impression of the final expulsive pains, generally saying she remembered "the baby being born" or "some sharp pains at the end"; as "absent" when she was able to recollect the events and progress of her labour. Thus classified—

	Cases		Per cent.
Complete amnesia was obtained in	36	=	45·0
Partial amnesia was obtained in	26	=	32·5
Amnesia was absent in	18	=	22·5
			100·0

The degree of analgesia is even more difficult to estimate. It is considered as complete in all those cases in which complete amnesia was obtained, partial in those cases in which the patient appeared to obtain relief from the injections and was satisfied she had had an easy labour, and absent in those cases in which she did not seem to have had, or did not consider she had had, any relief. Hence all of the cases in which partial amnesia was present and many of those classified under absence of amnesia are considered as having had some relief of pain. This rough classification works out as follows:—

	Cases		Per cent
Complete analgesia obtained in	36	=	45
Partial analgesia obtained in	40	=	50
No analgesia in	4	=	5
			100

In other words 5 per cent. of the cases may be reckoned as complete failures as the patient apparently derived no relief from the injections. In the other 95 per cent. some relief from the pangs of labour was obtained, in 77·5 per cent. some degree of amnesia and analgesia was present, and in 45 per cent. complete amnesia and analgesia.

In discussing the effects of this narcosis on the mother and child, attention should be drawn to the difficulties and inaccuracies in the observation of many of the points suggested by the Committee. With regard to subjective symptoms, for instance, estimation of pain is so much a matter of personal equation, both of patient and observer, that little can be deduced from it; such observations were chiefly of value in helping to classify analgesia. So also the question of mental confusion, as to whether restlessness is sufficiently marked to be noteworthy, or when a patient's condition should be described as one of excitement, must depend on the opinion of the individual observer as well as on the patient's state at the time of observation. Momentary periods of restlessness and excitement are clearly insufficient to give character to the whole labour. Again in a long labour some one or other of the symptoms, subjective or objective, may have been noted as present on one occasion or twice, and as absent at the many other times they were looked for. The presence or absence of reflexes was tested only occasionally and not in all patients. To do so systematically would have meant more disturbance of the patient than was compatible with a fair trial of the method of narcosis.

With the proviso, then, that the discussion of the effect on the mother must be largely based on the rough general impression given by the notes, it may be considered under the heads of—

(1) General effects.

(2) Effect on the labour.

(1) No decided influence on the maternal pulse or respiration-rate was observed. Usually there was a slight slowing of the pulse-rate, probably because there was some preliminary quickening of the pulse-rate owing to the excitement aroused by the start of labour-pains and admission to hospital, with subsequent slowing as the result of the drowsiness and mental quietude produced by the narcosis. In a few cases the slowing of the pulse was more marked than can be explained in this way—e.g., in No. 13 the rate was 76 at the time of the first injection and dropped steadily with each succeeding injection to 52, and a little later without further injection to 48; an interval of two hours elapsed without injection, and the pulse-rate rose to 56; one more injection was given before delivery three hours later when the rate had risen to 68. The respiration-rate fell from 20 to 16 per minute. In eight cases slowing of the pulse-rate sufficient to be noteworthy was observed, in only one case was quickening recorded.

Sleep between the pains was present in all but three cases and in twenty-seven (33·75 per cent.) there were periods in the labour when the pains did not rouse the patient; in such cases it was taken as an indication for the discontinuance of the injections till the condition had passed and was therefore of temporary duration only.

Mental confusion to some degree is present in most patients in whom narcosis is well marked, but it is difficult to recognize its presence without rousing her and making her talk, unless the woman is talkative and excited. Patients frequently talked nonsense and sometimes had hallucinations as to time and place or of objects about them—e.g., one patient complained of seeing mice running about the room. In nearly 40 per cent. of these cases some degree of mental confusion was obvious without special attempts being made to test the mental condition. Restlessness to some degree was noted in twenty-nine cases (34·2 per cent.), but as a rule was only exhibited on the occurrence of strong pains and was easily controlled. In seven cases (8·75 per cent.) it was, however, so marked and was accompanied by hysterical symptoms to a degree described as "excitement." In one case (29), the injections were discontinued on this account after eight had been given. In another case (69) with only three injections the patient appeared quite insensible of her surroundings and was very difficult to control, requiring to be held down in bed, but the pains were very strong, labour only lasting four hours from the first injection. In this case, as in many others in which restlessness was marked and the patient rambling and talking nonsense, the result was very good so far as amnesia was concerned. The patient slept heavily for hours afterwards and remembered absolutely nothing about her labour.

Thirst was present in 75 per cent. of the cases, vomiting in 20 per cent., and flushing of the face in nearly 90 per cent. Dilatation of the pupils was not systematically observed, but was noted in twenty-one cases, or about 25 per cent.

Beyond the troublesome restlessness and want of control, such as may be observed in any form of partial anæsthesia, no general effect on the mother was produced which could be said to be an objection to this form of narcosis. No dangerous effects on the mother were observed.

(2) As regards the effect of the labour itself, no accurate statistics on the duration of the stages of labour can be given, as exact observation was impossible without more internal examination than was compatible with leaving the patient almost undisturbed. Rough generalizations, therefore, can only be given. The notes show a fairly constant diminution in the strength and frequency of the contractions immediately after the first injection, sometimes lasting an hour or two, after which the uterine contractions appeared to follow their normal development. Bearing-down efforts were described as present in

fifty-five, feeble in ten, and absent in fifteen cases, and the impression was obtained that these efforts were undoubtedly diminished as a result of the narcosis. There seemed to be a tendency to delay in the latter part of the second stage, and this was shown by an increase in the number of forceps deliveries. Pituitary extract was also given in eleven cases before the birth of the child—the rule being that if delivery did not follow its administration within half an hour, extraction by the forceps should be carried out at once. Of these eleven cases delivery resulted within a short interval in five. In the other six cases, two of which were unreduced occipito-posterior positions, delivery by the forceps followed, and in four other cases delivery by the forceps was done without any preliminary injection of pituitary extract. There were thus ten cases of delivery by the forceps, or 12·5 per cent. which is about four times the average in the ward of cases in which this operation is done for delayed second stage. One of the cases was in a primipara in which the delay was due to a large child and resulted in stillbirth from delay with the shoulders, and another was in a multipara with slight pelvic contraction and a large child. Even if these cases are excluded, the percentage of forceps deliveries for simple delay in the second stage is more than three times the average ruling in the ward. Making allowance for the greater proportion of primiparæ in this series, the number of forceps deliveries is an indication of delay in the second stage.

The placenta was expelled spontaneously by the patient's own efforts in forty-one cases and was expressed after expulsion from the uterus in thirty-nine cases. In no case was any internal manipulation necessary for the separation or removal of the placenta. Bleeding was stated to be greater than usual in nine cases, or 11·25 per cent., but this is a very rough estimate and includes any case in which in the judgment of the observer the loss was thought to be more than 10 oz. (The average number of cases in ward termed P.P.H. is under 2 per cent. over all cases.) In four of these cases or 5 per cent., the bleeding was described as post-partum hæmorrhage and the following details may be given.

Case 8.—Breech presentation, with extended arms and legs, delivered under general anæsthesia; child stillborn. Long labour in which thirteen injections had been given; placenta expelled naturally with rather profuse loss estimated at 20 oz. Nothing beyond massage of the uterus through the abdominal wall was required, the patient showed no effects from loss of blood, and the puerperium was normal.

Case 10.—Primipara : six injections. Considerable loss before birth of placenta, estimated at 30 oz. Placenta expressed and 1 c.c. of pituitary extract injected, hæmorrhage immediately ceased. No general effect on patient, and puerperium normal.

Case 32.—Prolonged labour in a primipara in which seventeen injections were given : delivery by forceps under general anæsthesia of three-quarter hour's duration. Considerable hæmorrhage before birth of placenta : uterus flabby for a time ; 1 c.c. of pituitary extract was given, the uterus massaged till firm and placenta expressed, after which bleeding ceased. Amount lost estimated at 40 oz. The patient's pulse-rate from 72 just before delivery rose to 128. Saline solution was given *per rectum ;* she quickly recovered and the puerperium was uneventful.

Case 74.—Primipara : four injections, first at 11 p.m., second of $\frac{1}{200}$ gr. hyoscine at 12 midnight, third and fourth of $\frac{1}{450}$ gr. at 2 and 4 a.m. Delivery by forceps at 6.15 a.m. under chloroform and ether ; twenty minutes for prolonged second stage and unreduced R.O.P. Uterus flabby, severe bleeding, abdominal massage and expression of placenta : pituitary extract 1 c.c. and intra-uterine douche controlled hæmorrhage, which was estimated at O iv. Pulse very feeble and almost uncountable—and a little later rate 128. Saline solution and brandy *per rectum.* Slept for six hours, woke up and demanded a meal and to be washed : pulse-rate then 104 and good volume. Uninterrupted recovery.

From these cases it may be argued that taking the effect on the patient as the criterion, there were two cases of post-partum hæmorrhage severe enough to cause an effect on the pulse, and that both occurred after a prolonged second stage terminated by forceps delivery under general anæsthesia ; of the other two reported above, not severe enough to cause a noticeable change in the pulse or to call for restorative measures, one was also a case of complicated labour necessitating manipulation under general anæsthesia. These cases would seem to indicate a slight tendency to increased loss in the third stage though readily controlled, but it is impossible to ascribe the hæmorrhage as due to the narcotism alone.

The condition of the patient after delivery was good throughout and no cases of shock or other dangerous symptom, excepting in the two cases of post-partum bleeding recorded above, were noticed ; in one case headache and sickness occurred on waking. Nothing abnormal was noted in the lying-in time and all the mothers were able to suckle their infants, save one multipara who had failed with her previous children.

The Child.—As the effect on the child is a matter of extreme importance in deciding the justifiability of the routine administration of this form of narcosis, a full consideration of the results so far as the child is concerned is called for. In weighing the evidence from these eighty cases, it must be borne in mind that special attention was paid to the state of the child at birth and that, therefore, even trifling cyanosis of very temporary duration, such as may pass unnoticed in an ordinary way, was noted and attention drawn to it. Hence these results will err on the side of making the most of any respiratory disturbance at birth.

Stillbirth.—Three cases, or 3·75 per cent., occurred, two of which were due to causes quite independent of the narcotism. Case 8, already recorded as a case of post-partum bleeding, was a breech presentation with extended arms and legs in which fœtal death was due to delay with the aftercoming head. (Average number of stillbirths over all cases in ward of viable children, 4·2 per cent.)

Case 25 was a long and difficult labour in a primipara, in which nineteen injections were given. The fœtal heart was heard before delivery by forceps, rate 150 ; there was great difficulty and delay in the delivery of the shoulders owing to the large size of the child (length 24 in., weight 10 lb. 4 oz.) ; 1 c.c. of pituitary extract was given after the birth of the head when the difficulty with the shoulders was first recognized, so that in this case also there is no need to look further than the complications in delivery for the cause of the stillbirth.

Case 64 was a case of induction of premature labour shortly before term by bougies for slight pelvic contraction (C.V. estimated at 3½ in.), in which three injections were given, the child being born without difficulty thirty-six hours after the introduction of the bougies and four and a half hours after the first injection. It was stillborn and no reason for this was discovered. It was 21 in. in length and weighed 6 lb. 9 oz.

In nineteen cases (23·75 per cent.) the child did not breathe nor cry spontaneously at birth, but in nine of these the delay was so trifling as to be scarcely worthy of notice. In six cases external stimulation in the shape of rubbing, slapping, or dashing cold water on the child, and in three a hot bath, was sufficient to start respiration at once. The details of the other ten cases are as follow :—

Case 4.—Primipara ; three injections, birth four and a half hours after first ; easy labour. Cord pulsating, but child blue, and did not breathe spontaneously. Hot bath, insufflation and artificial respiration ; breathing shallow at first and at long intervals, normal breathing not established for three-quarters of an hour. Child regained its birth-weight before leaving hospital.

Case 22.—Primipara: three injections, birth four hours after first; easy labour. Cord pulsating, child white, no spontaneous breathing. Hot bath, artificial respiration: normal breathing not established for half an hour. Child did well, and was 6 oz. over birth-weight on discharge.

Case 24.—Primipara: eight injections, birth thirteen hours after first: head one and three-quarter hours at outlet, and advance very slow: fœtal heart-rate quick, and meconium passed: 1 c.c. pituitary extract given: child born in five minutes in state of white asphyxia: hot bath, insufflation, recovery in a few minutes. Birth-weight regained on discharge.

Case 32.—The case of prolonged labour and forceps-delivery recorded under post-partum hæmorrhage. Primipara; seventeen injections: cord pulsating, child white: hot bath, insufflation: regular breathing in twenty minutes. Child discharged to fever hospital with measles, an occurrence scarcely to be laid to the blame of the narcotic.

Case 41.—Primipara: six injections, birth ten hours after first; 1 c.c. pituitary extract given for delay at outlet, birth five minutes later. Blue asphyxia; external stimulation, hot bath, insufflation: normal breathing in ten minutes. Discharged 4 oz. under birth-weight.

Case 51.—Primipara; seven injections, birth ten and a quarter hours after first; delay with shoulders, child born white, cord pulsating. Hot bath and insufflation; normal breathing in ten minutes.

Case 52.—Primipara; two injections, birth three hours after first: os described as four fingers at first injection, head on perinæum one and a half hours later. Child did not cry spontaneously, and was first blue, then white. Hot bath, external stimulation and insufflation; normal breathing in ten minutes.

Case 60.—Primipara; three injections, birth three hours after first: easy labour: blue asphyxia; hot bath, insufflation: normal breathing in seven minutes. Child 5 oz. over birth-weight on discharge.

Case 74.—Reported already under P.P.H.: forceps delivery under C. and E. for delayed second stage and unreduced R.O.P.: white asphyxia, hot bath, artificial respiration; normal breathing in thirty minutes.

Case 80.—Primipara; three injections, birth seven and a quarter hours after first. delayed second stage, pituitary extract 1 c.c., followed by forceps. Child sluggish, grey in colour, heart-beat feeble and irregular. Hot bath, insufflation, external stimulation, and brandy on gums. Normal respiration in ten minutes.

It is interesting to note that the one stillbirth for which there was no adequate explanation, and that six out of the ten cases of non-spontaneous breathing, occurred after four or less injections.

Besides these cases in which spontaneous breathing did not occur, there were sixteen cases in which the occurrence of blueness in the child was recorded in the notes as being present at some time shortly after birth. For the most part this was trifling in character, and of short duration, but in four cases external stimulation was adopted to encourage respiration, and in one case a hot bath was given. Another case exhibited a most marked failure of respiratory effort after breathing well at first, and is worth recording.

Case 79.—Primipara; three injections: the second, one hour after the first, was of $\frac{1}{300}$ gr. of hyoscine. Child born three and a half hours after first injection, slightly blue, but breathed at once. About five minutes later breathing was shallow, pupils dilated and colour grey. Hot bath, insufflation and artificial respiration produced a good colour, except round the mouth, with good heart-beat. A little later another attack of cyanosis occurred with feeble heart-beat—rate about 80—and continued but with gradual improvement for about fifteen minutes, after which complete recovery took place.

The view that the morphia is the cause of the delayed respiration in the child is slightly supported by the results of these cases. The one case of stillbirth, for which there was no other explanation than the narcosis, occurred in a case in which birth took place four and a half hours after the first injection; but of the ten cases detailed above, six occurred in forty-two, in which four or less injections were given, and four in thirty-eight, in which four or more were given—a difference between 14·5 per cent. and 10·5 per cent. On the other hand, many of the cases in which birth took place soon after the morphia injection showed no effect on the infant's respiratory efforts.

It may be urged that the use of pituitary extract before the birth of the child may be a contributory cause of difficulty in starting respiration, and therefore the following details of the eleven cases in which it was given may be of interest.

Case 17.—$\frac{1}{2}$ c.c. Delivery in a short time: spontaneous breathing.

Case 24.—1 c.c. Delivery in five minutes. White asphyxia. (Cp. notes above.)

Case 25.—1 c.c. after birth of head for delay with shoulders; stillborn. (Cp. notes above.)

Case 29.—1 c.c. Head at outlet three hours; delivery in five minutes. Child did not breathe spontaneously, but external stimulation only required.

Case 41.—1 c.c. Head at outlet three hours; delivery in five minutes. Normal breathing not established for ten minutes. Hot bath, external stimulation, insufflation. (See above.)

Case 46.—1 c.c., followed by forceps in half an hour. Child breathed at once.

Case 50.—1 c.c., followed by forceps in half an hour. Ten minutes before regular breathing. Hot bath.

Case 68.—1 c.c. Head at outlet two and a quarter hours: delivery in seven minutes. Spontaneous breathing.

Case 70.—1 c.c. Forceps in half an hour. Spontaneous breathing.

Case 73.—1 c.c. Forceps in half an hour. Spontaneous breathing.

Case 80 (see above).—1 c.c., followed by forceps; normal respiration in ten minutes. Hot bath, insufflation, and external stimulation.

The case of stillbirth due to delay with the shoulders, in which the pituitary extract was not given till after the birth of the head, may be excluded as definitely explained by the delay in the birth of the body, leaving ten cases for analysis. Of these, spontaneous breathing occurred in five; in four there was blue asphyxia, and respiration was established by simple measures in ten minutes; in one described as white asphyxia, which was overcome in a few minutes, the signs of fœtal distress had been observed before delivery. As all were cases of prolonged second stage, it is difficult to apportion the blame between the delayed labour, the narcotism, and the pituitary extract, but, in view of the fact that in no one of the ten was there any difficulty in the establishment of respiration, it may be said that the pituitary extract, administered as described, does not add appreciably to the danger to the child.

Analysis of the Ten Cases of Instrumental Delivery.—One multipara, nine primiparæ. The multipara (No. 70) had a minor degree of contraction at the brim, and child weighed 9 lb. 13 oz.; five injections, delivery nine hours after first; pituitary extract followed by easy forceps extraction; no trouble with mother or child. The indications in the nine primiparæ were: in one case (No. 25, already quoted), large size of child; delay with shoulders, stillbirth. In three cases (Nos. 50, 73, 74) delayed second stage owing to unreduced R.O.P. position. In five cases (Nos. 32, 34, 46, 56, 80) delay at outlet.

A general anæsthetic was given in thirteen cases (14·25 per cent.)—in the ten cases of forceps delivery, in two cases (8 and 39) of incomplete breech with extended legs and arms, and in a case of mitral disease with failure of compensation (57). In all cases the patients took the anæsthetic very well and required appreciably less than when no narcotic had been given.

There were three cases of cardiac disease (Nos. 40, 55 and 57), all mitral, in one of which the patient had been in the medical wards for several weeks owing to failure of compensation. In all these cases the partial narcosis had a beneficial effect on the patient.

Conclusions.—The routine administration of morphia $\frac{1}{4}$ gr., hyoscine $\frac{1}{150}$ gr., followed by hyoscine $\frac{1}{450}$ gr., at hourly intervals and continued according to the effect on the patient, gave encouraging results. The estimate given showed some relief in 95 per cent. of cases, and complete amnesia and analgesia in 45 per cent. These results showed a considerable improvement on those obtained previously, partly, no doubt, from the greater attention to detail, especially to the precautions taken to exclude sensory influences, and partly to the more exact chemical composition of the hyoscine used, which was specially prepared and tested by Messrs. Burroughs Wellcome and Co. for the purpose of this investigation.

The first difficulty was the decision as to the best moment to begin the administration. The size of the os was not a very reliable index. To watch the dilatation meant more frequent internal examinations than seemed justifiable: dilatation to a florin or half-crown size may be present before active and regular pains are present, and on the other hand strong pains may have been present for several hours without the dilatation reaching a size which will admit two fingers. The occurrence of regular and steady pains, as tested by a hand on the uterus, without considering the dilatation reached, seems to be the best indication for the time to begin. If too early a start is made with the injections the labour may be prolonged, but an increased number of injections did not appear to bring with them any increase in the risk to mother and child. On the other hand, if the administration was not begun until rapid progress was being made so that very few injections were given, the result also was little affected. Some of the most successful cases in the series had not more than four injections. Out of the eighty, forty-two had four or less. In three of these only one injection was given, delivery in each case taking place within three hours, and all the children breathing spontaneously on birth. In two the result was good: one had complete amnesia and analgesia and the other marked analgesia and partial amnesia—the patient (No. 28) saying she only remembered the last pain, and that in spite of a rapid labour with very powerful contractions, which completed delivery in little over an hour after giving the injection, the os being then nearly half dilated. In the third case there was apparently no relief. Five cases had two injections only, in all of which a partial result was obtained, though in one in which a whiff of chloroform was given at the end of the labour amnesia and analgesia are described as nearly complete; spontaneous breathing occurred with all the children except one, for which a hot bath and insufflation were required (No. 52). That the result in these forty-two cases of four injections and under does not differ materially from that in the whole series is shown by the following rough classification :—

	Cases		Per cent.
Result described as good or complete in	14	=	33·3
Result described as fair or partial in	24	=	57·1
Result described as a failure in	4	=	9·5

In the last eight cases of the series $\frac{1}{300}$ gr. of hyoscine was given at the second injection instead of $\frac{1}{450}$ gr. without any appreciable advantage; the impression obtained was that it increased the tendency to restlessness and excitability.

On the other hand, the almost uniform success in the cases in which a general anæsthetic was given at the end of the second stage was very striking. Out of thirteen cases, complete amnesia occurred in all but one, or 92·3 per cent.; the only case of incomplete success was one of induction for slight pelvic contraction, the child not being born for four days after the insertion of bougies. Injections were not begun till good and regular pains were established, so that the patient had no narcotic during a slow and worrying onset, and yet stated only that "she thought she felt the pains." A degree of narcotism which will suffice for the rest of the labour is often insufficient to keep the patient insensible to the pain of the actual expulsion of the child. These pains were frequently the only pains remembered by the patient, and in many cases enabled her to reconstruct the whole labour. This closely corresponds with experience in other forms of anæsthesia, and is exactly similar to an occurrence, which often from the patient's point of view makes nitrous oxide anæsthesia, say, for the extraction of teeth, a failure. The extraction of an overlooked stump is the final stimulus to rouse the patient, and from it the whole process of clearing the jaw is reconstructed. If, however, the patient is allowed to come round slowly without being touched, he has no recollection of anything that has happened, even though he may at times have come so far round as to struggle or even to shout. So with this morphia-hyoscine narcosis, if the pain of expulsion brings the patient to consciousness, the amnesic effect is lost; probably all of the twenty-six cases classed as "partial amnesia" would have been in the category of "complete" had the actual birth been conducted under a general anæsthetic, and possibly a proportion of the eighteen cases in which no amnesia was obtained. A general anæsthetic given for this purpose would no doubt increase the prolongation of the second stage, and thereby the proportion of forceps deliveries, but it would probably raise the percentage of complete amnesia to about 90.

The principal disadvantages attending this form of narcosis are those which may be expected from any form of anæsthesia—the prolongation of labour, the tendency to delayed retraction in the third stage and to sluggishness of the infant in starting respiration. These effects can be judged from the evidence given, but they are not serious, and, beyond occasional attacks of cyanosis in the infant immediately afterwards, no ill-effects on mother and child were noticed during the lying-in period.

The administration calls for greatly increased service of both medical attendants and nurses, and, with regard to the latter, more training and experience than the ordinary maternity nurse has at her command. The absence of groaning and signs of distress in the patient makes the observation of the course of the labour more difficult and calls for increased watchfulness by the nurse. At the same time much expenditure of sympathy and encouragement of her patient by word and deed is saved to her. Owing to the impossibility of predicting the length of the labour and the constant supervision of the patient required, a relief nurse would be necessary in almost every case.

J. S. F.

REPORT ON TWENTY CASES AT THE GENERAL LYING-IN HOSPITAL, YORK ROAD, S.E.

THE small number of cases observed at the General Lying-in Hospital is due to the want of suitable accommodation. It was not possible to set aside a special room for these cases and they were conducted in one of the labour wards, as circumstances permitted, and every now and then another patient was in labour at the same time in the ward. All that could be done under the circumstances to exclude sensory impulses was carried out by keeping the room darkened and as quiet as possible and by plugging the patient's ears. Of the twenty cases nineteen were primiparæ, and one a multipara, second child. Except for one which developed eclampsia, and one brow presentation, all were cases of normal labour. The number of injections varied from one to seventeen, the average being 6·8: in the primiparæ only 7. The methods, dosage and drugs were as laid down by the Committee.

The memory test was not of much assistance; at times in three patients the memory of the object shown was abolished and injection was postponed on that account till some return of memory occurred. In the case of one of these (No. 6), who had fourteen injections and later on had eclamptic convulsions, the object shown was not recognized on three occasions—perhaps the result of a combination of the eclamptic stupor with the effects of the narcosis—but, generally speaking, the chief reliance was placed on careful observation of the patient's condition between and during the pains and on the notice she took of the hypodermic injections.

Classifying the amnesia and analgesia as described in the report of the cases from St. Thomas's, the results were as follow:—

	Cases		Per cent.
Amnesia was complete in	10	=	50
Amnesia was partial in	7	=	35
Amnesia was absent in	3	=	15
Analgesia was complete in	10	=	50
Analgesia was partial in	8	=	40
Analgesia was absent in	2	=	10

That is to say that 10 per cent. of the cases were failures, in 90 per cent. some relief was obtained and in 50 per cent. complete amnesia and analgesia.

General Effect on the Mother.—No great effect was noticed on the mother; in two cases the pulse-rate was markedly slowed and the pulse became intermittent, involving cessation of injections for a time; one of these cases (No. 6) is quoted in outline later. Slight acceleration of the pulse towards the end of labour was noted in three cases. No decided effect on respiration was found. Sleep between the pains was present at some time during all the labours and at intervals during the pains in five cases (25 per cent.). Mental confusion was noted as being present in four cases (20 per cent.), restlessness in six (30 per cent.), and to a slight degree at intervals in four others, so that including all, some restlessness was present in half the cases observed. Excitement, to a degree in which the woman was noisy, talkative or singing and difficult to restrain, was present in two cases (10 per cent.), in one of which the injections were discontinued for a time on this account. Flushing of the face and thirst were present at some time or another in nearly all cases. No dangerous effects on the mother were observed and in all cases the convalescence was satisfactory.

Effect on the Labour.—The diminution in the strength and frequency of the pains following the first injection, as noted in the St. Thomas's series, was also observed in these, but later in the narcosis the rhythm usually returned to its previous character and the labour proceeded naturally. In one case uterine inertia occurred and the injections were postponed for that reason. Delay in the second stage was very marked, there being five instrumental deliveries. One of these, however, was in a case of eclampsia, leaving four of low forceps delivery due to delay at the outlet—i.e., in 20 per cent. of cases, a very high percentage for the hospital. This series, therefore, shows a marked delay in labour as the result of the narcosis, more especially the absence of strong expulsive efforts at the end of the second stage, calling for instrumental delivery when the head is on the perinæum. There was no evidence of delay or complication in the third stage. There was no case of excessive hæmorrhage; the placenta was born naturally in six cases and expressed in fourteen. The condition of the mother after delivery was good throughout.

Child.—There were no stillbirths, but one infant, restored after artificial respiration for twenty minutes, died seven hours later (see abstract No. 9). One required artificial respiration, off and on, for two hours before natural breathing was fully established (see abstract, No. 6), five were blue and did not breathe at once but were easily restored, though one of them had an alarming attack of cyanosis on the second day. Spontaneous breathing occurred in the remaining thirteen cases. The results were: artificial respiration in 10 per cent.; breathing not spontaneous but easily started in 25 per cent.; spontaneous breathing in 65 per cent. A general anæsthetic was given in six cases (30 per cent.), and was taken well in all.

ABSTRACT OF CASES OF INTEREST AND REMARKS.

Primiparæ in all.

Case 1: *Partial Amnesia.*—Patient remembered nothing but actual birth, which she describes as "rather sore." She thinks the baby "just crawled out." She can remember nothing about the afterbirth (which was expressed), and does not think there was one.

Case 2: *Partial Amnesia.*—Also remembers nothing before or after the birth, but only the birth itself, which she says made her "keep wanting to bear down," but that it did not hurt her. Remembers nothing of placenta, which was expressed. The pulse-rate was 76, respiration 20 at time of first injection; one hour later, pulse 52, respiration 16; the second injection was postponed for two hours, when pulse-rate was 60 and respiration 20.

Case 4: *Partial Amnesia.*—After the fifth, injections omitted for three hours owing to uterine inertia. Patient says she felt actual birth and later some one pressing on her stomach and thinks something came away, but neither of these hurt.

Case 5: *Partial Amnesia.*—"I think I felt the baby coming, but I felt no pain. I had no pain all the time. I felt the pins put into me."

Case 6: *Eclampsia.*—Fourteen injections: tedious labour (about thirty-six hours). After seventh injection interval of two hours as object shown not recognized, and again after the eighth. After the ninth injection, pulse-rate, which from 80 to 90 early in the labour had risen to 120, dropped to 60, and was intermittent, so injections were restricted to one in five hours, and then resumed at intervals of one and a half hours. The last injection, the fourteenth, was given at 10.45 a.m., as the pains had almost ceased. At 6 p.m. the patient had an eclamptic fit, followed by a second at 8.30 p.m. The os was two-thirds dilated; dilatation was completed manually under ether anæsthesia (two hours), and the patient delivered by forceps at midnight. Two more fits occurred at 3 a.m., followed by a period of unconsciousness. The third stage was normal. The child was restored by hot baths and artificial respiration, normal breathing not being established for two hours. Mother and child made a satisfactory recovery.

Case 7: *Partial Amnesia.*—Patient says she had one bad pain when baby was born; she remembered nothing about the afterbirth and appeared to have little recollection of her labour.

Case 8: *Forceps Delivery.*—General anæsthesia (C. and E.). Complete amnesia, fourteen injections; after the twelfth two hours' interval owing to restlessness and irregularity of pulse, and again after the thirteenth, restlessness and excitement so that patient at times was uncontrollable. Head showing at vulva for one and a half hours without advance.

Case 9: *Forceps Delivery.*—Seventeen injections; morphia ¼ gr. at thirteenth; one hour after last injection low forceps as meconium was being passed. Fœtal heart 136. Chloroform twenty minutes. Artificial respiration twenty minutes; child immature and badly nourished; died seven hours later.

Case 11: *Partial Amnesia.*—Four injections. Says she slept after the "pricks" but just felt baby coming and it hurt a bit.

Case 12: *Partial Amnesia.*—Five injections. Did not feel anything till baby was born and thinks she remembered three pricks.

Case 14: *Complete Amnesia, Forceps Delivery under Chloroform.*—Second stage lasted four and a half hours, child breathed spontaneously. All she could recollect of her labour was that she remembered her back aching.

Case 17: *Complete Amnesia, Forceps Delivery under General Anæsthesia.*—Slept one hour after last injection with scarcely any pains. Had no knowledge of pains after injections were begun.

Case 19: *Complete Amnesia.*—One injection after general anæsthesia. Membranes ruptured and os three-quarters dilated on admission, brow presenting; this converted into face under chloroform, and injection of morphia and hyoscine given. As patient was quiet and passive, no further injection was given. The face was at the vulva for nearly two hours and the child was born two and three-quarter hours after the injection, the patient having no recollection of labour or birth. Both did well.

The cases of partial amnesia are quoted in abstract in order to show how frequently the birth of the head was the only part of the labour that was clearly brought back to the patient. This point was more fully discussed in view of the like experience at St. Thomas's, and the statement in the report of those cases that the administration of a general anæsthetic at the end of the second stage would increase the percentage of complete amnesia is borne out. The percentage of cases classed as showing complete amnesia in this series is 50 per cent., whereas in the series from St. Thomas's, under more favourable circumstances as regards isolation and quietude, the percentage was 45. The difference is accounted for by the percentage of cases in which a general anæsthetic was given being 30 in the former and 14·25 in the latter.

Otherwise these cases do not offer any differences from the St. Thomas's series. The last case quoted (No. 19) shows a successful result from one injection given before the patient had recovered consciousness after a general anæsthetic. The prolongation of the second stage is more decided in this series, but on the other hand there is a complete absence of complications in the third stage.

J. S. F.

REPORT ON SIXTY-SEVEN CASES AT QUEEN CHARLOTTE'S HOSPITAL.

(*a*) The administration of scopolamine and morphia at Queen Charlotte's Hospital to sixty-seven patients gave good results. The results were better in the latter series of cases. In 90 per cent. the pains of labour were diminished. Complete amnesia was obtained in 46·2 per cent., partial in 44·7 per cent. Analgesia was complete in thirty-two cases, partial in thirty-one. There were two failures.

(*b*) Indications for commencing the treatment on the character of the pains, which should be regular and strong. The size of the os uteri cannot be taken as a guide.

(*c*) The number of injections given varied. Three patients had only one injection, while one had ten. In all cases but one, the pains were good at the time of the first injection. Indications for repetition of the hyoscine injections seem to be the patients' general condition and the frequency of the pains. In no case was a second dose of morphia given.

(*d*) The memory test cannot be relied on; further observations on the corneal and plantar reflexes were of minor importance in drawing our conclusions.

(*e*) Considerable experience is required by the practitioner in the treatment of the cases of "twilight sleep"; the same remark applies to nurses, who, if left in charge of such cases, should be carefully trained in the routine beforehand, as it is more than possible that dangerous results might supervene if the giving of morphia and hyoscine were left to inexperienced persons. Further, any case of "twilight sleep" demands the constant attendance of the doctor.

(*f*) With regard to general effects upon the patients, mental confusion, thirst, and restlessness were observed in about half the cases, but no active delirium.

(*g*) Labour does not seem to have been prolonged after giving the first injection. In cases noted as prolonged in our series, it was the first stage—i.e., before the injections were given at all.

(*h*) Out of sixty-seven cases, there were twelve forceps deliveries, but the indications were due to obstetric complications, such as occipito-posterior positions, or large head, and not due to the effects of the drugs on uterine contractions.

(*i*) The placenta was spontaneously expelled in sixty-five out of the sixty-seven cases, this being evidence that uterine contraction and retraction were not interfered with.

(*j*) There were two cases in which there was hæmorrhage in the third stage, one due to an adherent placenta, the other to a low-lying succenturiate placenta. These cases could not be laid to the door of the narcosis alone.

(*k*) The puerperium in our sixty-seven cases was undisturbed, nor was involution retarded.

(*l*) In fifty-three out of sixty-eight babies (one case of twins) breathing or crying was noted as spontaneous. In thirteen cases the baby was described as blue, but in one instance only was there cause for alarm. Most of the blue babies at birth were limp, but soon recovered. Hot baths and artificial respiration were only required in the minority of cases. Violent resuscitative methods for such babies are to be deprecated. Three babies died, two were premature, one died of broncho-pneumonia on the sixth day. There were three cases of white asphyxia with forceps deliveries, due to delay from obstetric complications, and not to the drugs used.

(*m*) We cannot express a definite opinion on the effects of hyoscine and morphia on lactation, owing to the short stay of patients in the hospital. Of the sixty-four living babies who left the hospital forty-nine were breast fed. Such cases should, if possible, be followed up in the infant clinics as to their subsequent progress.

(*n*) Observations showed that of the sixty-four living babies thirty-four had regained their birth-weight during the first week.

H. R.,
T. G. S.

REPORT ON SIXTY-FIVE CASES AT THE CITY OF LONDON LYING-IN HOSPITAL.

THE treatment consisted of the injection of morphia and scopolamine. The first injection was given when the cervix was two fingers dilated, and the pains were strong and regular. All the patients were primigravidæ. During the treatment a maximum of quietude was observed, the room was darkened, and the patients' ears were plugged with cotton wool. The first injection consisted of morphia $\frac{1}{4}$ gr., scopolamine $\frac{1}{150}$ gr. The second and succeeding doses were all scopolamine $\frac{1}{450}$ gr. The second dose was given from three-quarters of an hour to one hour after the first, and the succeeding doses at hourly intervals. The number of injections varied from three to fifty-three, with an average of 13·6 injections. In one patient morphia was repeated, in $\frac{1}{6}$ gr. dose, at the thirty-eighth injection, as consciousness was being gradually regained. The exhibition of morphia produced a satisfactory return to the twilight state. *The memory test* was applied in all cases. It was found generally unsatisfactory, the object, being almost without exception, recognized, and in one case the showing of the object was the only fact memorized. Catheterization was, as a rule, necessary during labour.

Result of the Injections.—Complete amnesia was produced in 43 per cent. of the series. Partial amnesia in 52 per cent. Complete analgesia was produced in 12 per cent., excluding those patients in whom complete amnesia was present. Partial analgesia in 18 per cent., the completely amnesic cases being again excluded. Thus in the whole series there were only 3 per cent. of complete failures.

General Effects upon Patient.—*Thirst* was present in 63 per cent. *Nausea* and *vomiting* were present in 24 per cent. *Mental confusion* was marked in 24 per cent., slight in 34 per cent. *Restlessness* was considerable in 55 per cent., slight in 17 per cent. *Excitement* was present in 35 per cent., and a small proportion of these were maniacal. *Pulse* and *respiration* were never affected. *Pains:* The effect on the pains was difficult to estimate, but in 34 per cent. the pains appeared subnormal. The interval between the pains was in all cases normal. Bearing-down efforts were, as a rule, subnormal. They are mentioned as being good in 14 per cent., fair in 3 per cent., slight in 40 per cent., very slight in 3 per cent., and absent in 10 per cent.

Labour.—Labour lasted from six hours, forty minutes, to one hundred hours, fifty minutes—the average being twenty-four and a half hours for the first stage and two and a half hours for the second stage. This shows a prolongation of the second stage of labour, fifty minutes in excess of the normal for this hospital (one hour, forty minutes). *Forceps* were applied in 32 per cent. of the cases. In all of these the application was necessary. This is greatly in excess of the average percentage of forceps delivery in this hospital, which is 11·8 per cent. in primigravidæ. *Perinæal tears* were produced apart from the use of forceps in 14 per cent., this number being slightly greater than in the parallel series of cases without the use of "twilight sleep." This excess is due to difficulty in controlling the patient when the head is on the perinæum. The percentage of *occipito-posterior positions* which did not undergo spontaneous rotation was 39 per cent.—seven out of eighteen. In a series of occipito-posterior labours, apart from the use of "twilight sleep," the percentage which did not rotate was seventeen—seven out of forty-one. This increased failure of rotation is attributable to the lack of bearing-down efforts, which, combined with strong pains, are so important in the spontaneous rotation of these positions. The *third stage of labour* was unaffected.

Effect on the Infant.—On the whole the condition of the baby when born differs slightly from the normal. In some cases, particularly in those in which chloroform was given, a disinclination to breathe was present. This was associated with cyanosis, and general limpness. In marked cases deep, gasping, respiratory efforts recurred at intervals of one-half to three-quarters of a minute for ten to twenty minutes before satisfactory respiration was attained and maintained. Treatment by hot blankets attained absolute success in all cases, and there is certainly no doubt that more vigorous treatment is not only useless, but actually dangerous, and is probably the cause of the deaths which have occurred in babies in this state. Three of the children were stillborn. In one of these pregnancy was only thirty-two weeks advanced. In the other two labour lasted for one hundred and one hours and seventy-two hours respectively. In the former the membranes were ruptured fifty-two hours before delivery; the child was delivered with forceps. On delivery the cord was pulsating 40 beats to the minute and no attempts at respiration were made. A post-mortem showed no traumatic lesion. In the latter the membranes had ruptured ten days before labour started; the pelvic inclination was very steep. The fœtal heart ceased after the thirty-sixth injection. In both of these cases death could easily be attributed, and was almost certainly due, to intra-uterine pressure resulting from drainage away of the liquor amnii. The subsequent condition of the children born alive was in all cases satisfactory, and in no case did the mother show ill-effects as the result of the drug.

Comments.—The value of the treatment must be examined from four standpoints:—

(*a*) *The Mother's.*—Labour looked at from her point of view may be merely a happy sleep, and at the very worst is shortened, and the amount of pain felt greatly diminished. This shortening is one of the most marked features of the treatment, a long labour being memorized as one lasting from a few minutes to, at the most, one or two hours.

(*b*) *The Infant.*—In careful hands no bad results should accrue.

(*c*) *The Nurse.*—Looked at from the point of view of the nurse, the patient is incapable of being reasoned with when noisy or excitable, and requires constant watching. Without exception the patient requires gentle controlling at intervals, and may require forcible controlling in excess of the physical capability of the attendant.

(*d*) *The Doctor.*—The difficulties of the practitioner start, in hospital practice, during the second stage of labour. In normal cases the extreme difficulty of controlling the patient, and the therefore greatly increased risk of a ruptured perinæum, have to be considered. The number of cases in which forceps have to be used is considerable. These deliveries are often relatively difficult owing to the fact that the diminished bearing-down efforts fail to force the head deeply into the outlet, or fail to rotate occipito-posterior positions. The condition of the baby would cause the inexperienced very grave anxiety and, as has already been pointed out, deaths are likely to be produced by too vigorous treatment. In private practice the anxiety of the relatives of the patient has to be combated; their experiences being culled from the descriptions read in the Sunday papers and other literature on the subject, it is difficult for them to appreciate that extreme noisiness of the patient may be attended by a perfectly satisfactory issue.

In conclusion, there is no doubt that the treatment requires considerable experience both of the treatment itself, and of midwifery, and it appears probable that it should be withheld in all cases except those in which the mental equilibrium of the patient is lost early in the first stage of labour.

C. B.
G. L.

DISCUSSION.

Dr. W. A. POTTS (Birmingham): My experience is based on more than 100 cases, many of them conducted in the Birmingham Maternity Hospital, but the majority in private practice, chiefly in a nursing home, which is the most favourable place for treatment, while a few were in their own houses. The drugs used were hyoscine tabloids ($\frac{1}{200}$ gr. usually), and morphia tabloids ($\frac{1}{4}$ or $\frac{1}{6}$ gr.): morphia as a rule was only given with the first dose of hyoscine; the second dose of hyoscine was given, if necessary, in not less than forty-five minutes. Subsequent doses were given at intervals of not less than two hours, as required, careful individualization being essential, as patients differ enormously in their response. To give doses by the clock is not safe. It is most important that all patients should be given water to drink at intervals. It is unnecessary to detail my results, as they are embodied in a paper which is being published in the *British Medical Journal*. In the main, they confirm the results obtained by the readers of the reports, as regards the safety of the treatment under close supervision, &c., except that I consider that in suitable surroundings success can be assured. This is effected by giving, if necessary, a little chloroform (just enough to put the patient to sleep) after the third dose, and also by usually giving chloroform when the head is being born. I believe in the memory test, but with increased experience have found I can judge best when further doses are required by the patient's attitude, demeanour, and conversation. When treatment has to be continued for more than twenty-four hours I sometimes let the patient come right out and have a meal: this is reassuring to the relatives, diminishes the risk of over-dosing, and also diminishes any tendency to uterine inertia. According to some of the reports, restlessness is common: when present in more than a slight degree, which can easily be controlled by a little chloroform, I believe restlessness to be due to over-dosing with scopolamine. I have had no signs of the nausea and vomiting described in one of the reports: possibly this was due to my using hyoscine and not scopolamine. With increased experience I have had no cases recently of oligopnœa. I noticed in the reports that sometimes there has been increased loss of blood during or after the third stage. In two of my cases for this reason I plugged the uterus. I think it is possible that in both these cases, as the hæmorrhage was not alarming, the continued use of simple measures would have controlled the bleeding, as it did in the cases recorded in the reports: I was anxious, however, to make quite certain. The explanation probably is increased capillary oozing after the use of the drugs, from lacerations which might otherwise have given no trouble. I believe that some operating surgeons have found that the use of scopolamine-morphine before a general anæsthetic does predispose afterwards to increased capillary oozing.

which is, however, never serious. Although all cases should be closely supervised by the medical attendant, it is necessary to have specially trained nurses. The greatest difficulty with nurses is to train them not to talk, and also to leave the baby alone, if it is in a state of oligopnœa, avoiding vigorous artificial respiration, and merely keeping it warm. In order to test the results and avoid any misunderstanding, my patients are asked on waking (1) what day they think it is; (2) what time; (3) whether they are satisfied with the treatment; and (4) whether they remember anything specially unpleasant.

Miss MARGARET RORKE: I have now used scopolamine-morphine anæsthesia in childbirth during the last four and a half years: at first, only in selected cases, and then a single dose only was given. Being greatly impressed by the aid this gave to natural labour, I began during the last two years to use it in nearly every case, and in repeated doses as regards hyoscine; no repeat doses of morphia were given. My experience as regards amnesia and anæsthesia has been pretty much on a par with that of other speakers. Some patients have had both conditions almost completely satisfied; others have seemed throughout labour to complain during each pain, almost as much as a patient with no drug might, but between the pains they would sleep soundly, only becoming aroused with each uterine contraction. This second class, however, has had, after labour, no recollection whatever of anything after the first injection. I have given the first injection as soon as labour pains are thoroughly established, and have repeated the doses of hyoscine as indicated by the condition of each individual patient. My cases have not had prolonged labours in consequence of this treatment, though I must confess that I apply low forceps in every primiparous case, when the head is on the perinæum. This method I adopted after watching one or two primiparous cases where the head lay on the perinæum for some hours, the patient lacking the final expulsive effort necessary to deliver the head. But my cases have dilated up fully and advanced also in the second stage to this point where the head lies on the perinæum, in a much shorter time than normally occurs in a primiparous labour. Thus my last case (a 1-para) had os size of a shilling at 1 a.m., when first injection was given, and yet the head was down on the perinæum at 7 a.m., the child in this case weighing 8¾ lb., and having, of course, a large head. Another 1-para, aged 38, had a dry labour (liquor amnii having drained away twenty-four hours before labour pains began). Pains began at 1.30 a.m., and under scopolamine-morphine injections the head was on the perinæum by 11 a.m. Forceps were then applied. Most of the babies have shown slight blueness, and oligopnœa has been present in varying degree; but every child has recovered in fifteen to twenty minutes, treated naturally, and all have done well subsequently. The greatest advantage, to my mind, is the good condition of the mother after labour. There is no shock, and in every case she feels perfectly well on the second day, and in each case is able to suckle.

Dr. J. M. BRYDONE: I am a whole-hearted supporter of the use of scopolamine and morphia in suitable cases. I have had the same experience, precisely, as the authors of the papers read, but I now use the method differently to the original instructions, and get more uniform results. I never now use the memory tests, which may arouse an amnesic patient, and often mislead the doctor. The injections are of the greatest value in the *first* stage, but in the second stage, although the lasting effect is useful, chloroform is preferable, especially for the expulsion of the head. When a patient complains of pain, I give a whiff of chloroform, verify the pelvic condition, and inject $\frac{1}{8}$ gr., $\frac{1}{6}$ gr., or $\frac{1}{4}$ gr. morphia, with $\frac{1}{200}$ gr., $\frac{1}{150}$ gr., and $\frac{1}{100}$ gr. scopolamine, usually $\frac{1}{6}$ gr. and $\frac{1}{150}$ gr. This lasts from two and a half to three hours; if then the os is not fully dilated, another lesser injection is given, and two hours later another. I have never required more than three, and usually two suffice for the first stage. For the second stage, it offers no advantage over small doses of chloroform. No case has lasted more than nine hours in the first stage, and few more than six hours, a rather remarkable fact, and the second and third stages usually about two hours. I conclude that this method shortens the time very much. I usually rest between the injections elsewhere. It must be said that none of the cases were of the lower classes: most were healthy, normal primiparæ, and the chief abnormality was the occipito-posterior position. In 280 consecutive cases since January, 1915, I used it in fifty out of the last 150. Two failed to receive any benefit (one developed puerperal mania, herself the child of an unsound parent). One child was stillborn, the result of the cord prolapsing, after the injection two hours before the birth, in a primipara, a result unconnected with the injection. In the third stage I have had no anxieties about mother or child, due, I believe, to not continuing the injections in the second stage, and I also believe that the total time is greatly shortened, and although I must admit a greater use of forceps than in the figures quoted to-night, I firmly believe the above method involves no risk to mother and child, and ensures a painless labour.

Dr. W. OSBORNE GREENWOOD: The following remarks are based upon a series of 240 cases in which these drugs were used. The dosage I first used was morphia $\frac{1}{4}$ gr., and scopolamine $\frac{1}{100}$ gr., but as experience accumulated I found that this initial dose could be reduced to morphia $\frac{1}{5}$ gr., and scopolamine $\frac{1}{200}$ gr., and in some cases morphia $\frac{1}{6}$ gr., and scopolamine $\frac{1}{150}$ gr. The decision as to the required initial dose was based upon two factors: (I) A rough estimate was formed of the body weight of the patient, and (II) alongside this was placed the mentality of the patient. Thus a heavy woman of a vivacious or nervous temperament was given the largest dose; a small one of a phlegmatic temperament had the smallest. According to the predominance of one of the two factors I used the intermediate dose. This initial dose was followed by scopolamine alone in amounts varying from $\frac{1}{100}$ gr. to $\frac{1}{400}$ gr., repeated as often as necessary. The standardized method of dosage.

as advocated by some, has the inherent fallacy that the patients are not standardized, and further, my experience shows that the usual method of giving $\frac{1}{100}$ gr. every hour is too small to secure the best amnesic results. Taking the average dose in my cases the amount given (had it been administered every hour) would have been about $\frac{1}{250}$ gr. But by individualizing *every* case, I had only two failures in the 240 cases, while some half dozen remembered the final passing of the head over the perinæum. They affirm it was only a knowledge of something occurring and not pain. It is quite possible some of these cases really remember the passing of the placenta later on. The determination of amnesia is essential to the best success, and the reliance on so-called analgesia is certain to lead to failures. Probably every case that goes so far as a perfect amnesia also exhibits some analgesia, and I suggest the term hypalgesia be reserved for these. While a leading question may be good practice in law, it is doubtful whether it is the wiser thing in this work. For this reason, in using the memory test, I show a patient an object, and in half an hour or so, instead of again showing it and asking if she has seen it, I simply ask her what I had showed her a little while ago. It is an interesting speculation as to how the amnesia is caused. It is known that in old age the cerebral cortical cells suffer shrinkage, so that the synapses of the dendrites of the cells are less perfect. Memory is dependent upon the reception of stimuli by the cortical cells, and the *diffusion of these stimuli* to contiguous cells. In old age, if shrinkage has gone far enough to interfere with this diffusion there is loss of memory for recent events. I am suggesting that during the administration of scopolamine-morphine there is retraction of the dendrites of the cerebral cortical cells. This hypothesis agrees with the clinical observation that a patient will remember any unusually powerful stimulus, such as a bright light, a loud noise, for then the stimulus will jump the increased gap, so to speak, just as an electrical current of higher potential will spark across a gap that is too wide for a current of lower potential to jump. Thus the whole question of amnesia is not only important, but interesting. Some would seem to assume that amnesia is a later stage than analgesia, but this is certainly not the case, although there is hypalgesia with amnesia. I am sure, too, that this hypalgesia has the effect of not causing the patient to exhibit her pain by as much demonstration, and so the opinion has been formed that uterine contractions are lessened. In reality, as I have proved in many scores of cases, the contractions occur just as usual, and can be felt through the anterior abdominal wall. So, also, voluntary contractions occur, but have often passed unobserved, and this again has given rise to a false impression that voluntary contractions have ceased, or are weaker.

Lady BARRETT: In a series of thirty cases in which "twilight sleep" was given at hospital, I was impressed with the necessity for a skilled medical practitioner to be in constant supervision of the patient. It is necessary for the medical attendant to see the patient at least every hour, and those cases in which this vigilance was relaxed proved far less successful than those in which it was carefully observed. One child was stillborn, but this was a very small and premature infant. Most of the children showed some delay in breathing, but in most cases a warm bath was all that was needed to resuscitate them. There were no cases of post-partum hæmorrhage. The memory test did not appear to be a true guide to sufficient dosage, several patients who recognized the articles shown them every time, yet had complete amnesia when questioned after labour.

Dr. AMAND ROUTH: I hope some authoritative advice from the Section of Obstetrics and Gynæcology will be given on the subject, especially as to the types of women the method should be used for, and as to when it is contraindicated. Both doctors and patients are awaiting the result of this discussion.

Mr. COMYNS BERKELEY: Although the patients in the City of London Lying-in Hospital are under my care, Mr. Gordon Ley has charge of them during my absence, and most of the information conveyed in the joint report has been the result of Mr. Ley's experience. I am persuaded that "twilight sleep" may be a dangerous method of treatment, unless a highly skilled nurse can be in attendance all the time, and the medical attendant is able to remain in the near vicinity of his patient throughout the whole of her labour. The treatment in some cases has been attended with alarming results, as when the patient becomes very violent, so that I have known it take four nurses to control a patient. In one case the nurse left the lying-in room for a few moments, and on her return was horrified to find that the patient had disappeared. After an anxious search the patient was found inside the wardrobe. I am certain that the great danger to the child is that the medical attendant or nurse will endeavour by more or less forcible means to resuscitate it.

The PRESIDENT: It might be imagined from the speeches of many of those who have taken part in the discussion that this method is quite free from danger. In my opinion this is far from being the case. Hyoscine is a very powerful drug, and cases in which dangerous symptoms have arisen from its use are not unknown. Personally I know of two such cases; in one, a patient of my own, the woman died with all the symptoms of hyoscine poisoning after a dose of something more than $\frac{1}{50}$ gr. and something less than $\frac{1}{100}$ gr. There was no other sufficient reason for her death. When "twilight sleep" was first introduced I used it in a certain number of my private cases, but I soon abandoned the method, and do not intend to resume it. It is obvious from the experience of some of the speakers that it tends to produce uterine inertia, and to raise the forceps rate; it is impossible to raise the forceps rate without increasing the morbidity rate, and the danger to the mother and the child. This fact has been proved beyond any question by various observers, and in itself is a condemnation of the general adoption of so-called "twilight sleep." I consider that the administration of hyoscine and morphia, possibly followed by pituitary extract, and then in some cases by chloroform, to the surgical degree, is likely to lead in certain cases to dangerous results. The practitioner, too, has no power of undoing the harm which may result from the hypodermic injection of these drugs. Lastly, there is the possibility that hyoscine may be used for the purpose of doing away with an unwanted baby, when administered in what would appear to be a perfectly legitimate manner.

Dr. WILLIAMSON (in reply): Very little has been said of the disadvantages of the method. I endorse the President's warning that scopolamine-morphine narcosis is by no means devoid of danger. Experience has taught me that in many cases it has brought a blessed relief from suffering, and that in properly selected cases, when administered under competent supervision, it is of great value. It will be unfortunate, however, if from this meeting the impression goes abroad that it does not frequently interfere with the normal course of labour, or that its administration is entirely without risk. It often prolongs labour, it increases the frequency of forceps deliveries, it occasionally causes an active delirium, and in a not inconsiderable percentage of cases it fails to achieve its object. Its most serious drawback is the effect produced upon the child. I believe this danger is reduced to a minimum when the technique outlined in the paper is carefully observed, but even then it is not entirely got rid of. What I have heard this evening has not led me to modify the conclusions I have drawn from my own experience. I believe that scopolamine-morphine semi-narcosis is of the highest value in selected cases, when administered under the constant supervision of a competent doctor, but if administered in unsuitable cases, or if the patient is not carefully watched throughout by a skilled observer, disasters are likely to attend its employment.

Dr. FAIRBAIRN and Mr. GORDON LEY replied, the latter as follows: I entirely agree with all that has been said by Dr. Williamson with regard to the use of this form of treatment. I consider that the high rate of forceps deliveries, of occipito-posterior presentations, and of perinæal tears, are a sufficient bar to the routine employment of this form of treatment, and I still hold to the statement that the treatment should only be used in those cases in which the mental stability of the patient is subnormal.

Section of Obstetrics and Gynæcology.

President—Dr. G. F. Blacker.

Inversion of the Uterus complicated by Intestinal Obstruction.[1]

By O. Tilson Dinnick, M.B.

Mrs. M., aged 26, was alleged to be suffering from a uterine polypus, and as such was admitted to the hospital on the 22nd of the month. She gave a history of a first confinement twenty days previously: the delivery was instrumental and a retained placenta was removed the following day under anæsthesia. She bled freely during the earlier days of her puerperium. Three days before admission, and on the seventeenth following her delivery, a tumour appeared at the vulva during defæcation. In the last two days she had had urinary retention, requiring catheterization.

She was a very exsanguinated, thin woman, who looked ill. There was a median subumbilical tumour, the distended bladder, and the catheter withdrew some 30 oz. of purulent urine. A tumour the size of a large orange protruded from the vulva. It was purple in colour, velvety in appearance, and bled freely. It was tender to the touch. Nowhere could the examining finger pass beyond it, and the uterus could not be found on bimanual examination. The abdomen was slightly distended; the tongue dry and brown; the pulse 120, the temperature 102° F. The bowels had not moved in the last two days. An enema was returned with only a slightly coloured result. In view of this, and the abdominal distension, nothing was given by mouth, and saline was given subcutaneously. Bladder wash-outs were given thrice daily, and vaginal douches ordered.

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[1] At a meeting of the Section, held May 2, 1918.

On the 24th, two days after admission, the patient vomited, and her distension was marked. The inverted uterus was darker in colour, and there was a purulent vaginal discharge. Urinary retention was still present, the urine was purulent, and contained *Bacillus coli* and staphylococci.

The husband was informed of the seriousness of the patient's condition, and his consent obtained to any surgical measures deemed needful. The patient's condition was grave. Her temperature was 105° F., her pulse 130 ; the constipation was absolute, and the vomiting progressive. The distension, the vomiting and the lack of any great abdominal tenderness aroused suspicion of a mechanical obstruction.

The condition of the urinary bladder, the foul state of the inverted uterus, and, above all, the distension made one loth to attempt any vaginal procedure, and it was decided to do an exploratory laparotomy, in the Trendelenburg posture.

On opening the abdomen subumbilically, a coil of distended small bowel was seen to be adherent to the entrance of the inversion ring, causing an acute angulation. This was gently freed, and the belly packed with moist swabs. The whole of the pelvic peritoneum was dry and deeply injected. The constriction-ring was incised posteriorly, and the inversion reduced. The uterus was the seat of scattered intramural abscesses, the mucous membrane in areas was necrotic, and the whole organ exceedingly friable.

The operator believed it was better not to attempt to save the womb, and a total hysterectomy was done. The vaginal vault was left open, and the pelvic floor repaired. Catgut was used throughout. The bladder, the walls of which were thickened and rigid, hung over the pouch of Douglas, and was a bar to easy pelvic manipulation.

It was noted that the ostia of the Fallopian tubes were open, and no adhesions were present. At the spot where the bowel had been adherent was an area of commencing necrosis.

The patient made an uneventful convalescence, and required no special treatment beyond that indicated for her cystitis. She left the hospital five weeks after admission.

The recorder believes that intestinal obstruction is an unusual complication of inversion of the uterus ; also that grave complications in uterine inversion are decreasingly frequent with the increasing age of the lesion. In the case reported, twenty days elapsed before the patient was threatened with disaster.

Finally, the recorder would be inclined to approach acute inversion requiring operation by the abdominal route, and to leave the various ingenious vaginal procedures for the cases of chronic, or better-named, established inversion of the uterus.

Dr. LAPTHORN SMITH: A great difference in the prognosis in cases of inversion of the uterus depends on whether they are seen within a few hours, or at most a few days, or whether several weeks or months have been allowed to elapse before anything is done. It cannot be too strongly impressed upon the practitioner or midwife that in such an accident even hours are precious. The consultant should assure the family that it is a rare accident, for the occurrence of which no one can be blamed unless help is not called for as soon as the condition is discovered. My own experience is limited to three cases, none of which, however, occurred in my own practice. The first I saw while I was assistant to my predecessor, the late Professor Trenholme; it was a case of several years' standing, and during all that time the woman had been in bad health. It was found to be impossible to do anything by the vagina owing to the contraction of the internal os. Professor Trenholme decided to open the abdomen and dilate the inverted os from that direction, but eventually had to cut almost the whole length of the uterus before it could be reduced. It would have been easy then to have removed the uterus and saved the patient. But in those days there was no preparation of the bowels and no Tredelenburg position, so the uterus was sewn up and the abdomen closed. The patient died a few days later. I only mention this case to show how the treatment has improved in thirty years. Of the two other cases to which I was called by practitioners, one occurred a few hours and the other a few days after the confinement. In both cases I was surprised at the ease with which I was able to replace the inverted organ. The patient was at once cleaned up, sterilized and anæsthetized, and as I had no repositor with me I made one out of an article in general household use. It was a foot long, an inch in diameter, and had a round polished end. It was boiled for half an hour and oiled with carbolic vaseline. The uterus was very œdematous and with a steady pressure the round end of my improvised instrument sank deeper and deeper into the cornu until in ten or fifteen minutes the condition suddenly righted itself. In the other case, which was not discovered until after three days, when the woman sat up to stool, and what was thought to be a polypus dropped out, I followed the same course with an improvised instrument, and the procedure only required a few minutes longer to tire out the contraction ring at the cervix when the deeply indented cornu suddenly went back to its place. Both these patients made normal recoveries. I have had no other case in my own two thousand odd. Whether this is pure luck or whether it is because I have always waited until the patient was out of the anæsthetic before removing the placenta, and even then always by expression and not by traction, I do not know. The accident is certainly becoming more and more rare.

Undiagnosed Cancer of the Body of the Uterus complicating Myoma treated by Total Abdominal Hysterectomy.[1]

By Herbert R. Spencer, M.D.

In March, 1917, I read before this Section two papers (giving the whole of my experience) on the frequency with which sarcoma of the body and carcinoma of the cervix are overlooked in performing hysterectomy for myoma. These papers described two cases of undiagnosed sarcoma met with in thirty supravaginal amputations for "myoma" (6·6 per cent.) and four cases of undiagnosed cancer of the cervix in 200 total abdominal hysterectomies for myoma (2 per cent.).

To-day I describe two cases in which I have overlooked cancer of the *body* of the uterus in the same series of 200 total abdominal hysterectomies for myoma (1 per cent.). But for the fact that I usually explore small bleeding myomatous uteri through the cervix and rarely treat them by abdominal hysterectomy unless complicated with cancer, other cases might have been overlooked amongst those in which I have operated for myoma complicated with cancer of the body; but as a matter of fact in every other case the diagnosis was made by examination of the growth before the operation. And although this is not always possible or desirable in cases of myoma complicated with cancer, the error would appear to be less serious if the uterus is removed by total hysterectomy by Doyen's method (in which the cervix is closed by volsella, and thus the danger of infection or implantation is lessened) than when the cervix is cut across in amputation.

Case I.—M. J. W., single, aged 55, was admitted into University College Hospital on May 3, 1909, complaining of almost continuous loss of blood from the vagina and aching pains in the lower abdomen for nine months; of late the pain had been very severe, stabbing in character, passing down the thigh to the knees, and the discharge had been offensive. Menstruation stopped at the age of 49 for twelve months, then recommenced at the age of 50, occurring every fortnight, with no discharge between the periods till nine months ago. Menstruation began at 14, and, till the age of 49, was regular every four weeks, slightly painful and lasted one week, twelve diapers being used. There was no family history of cancer.

On abdominal examination a hard tumour could be felt rising to a height of 3 in. above the pubes. The hymen was present, the external os was that of a nullipara. The uterus was greatly enlarged by a hard uneven mass as big as a double fist, and this was thought probably to be a fibroid: on account of the pain total abdominal hysterectomy was done, since there was the possibility that cancer was present. At the operation on May 8, 1909, the rectum was found adherent to the back of the uterus, and in making traction on the body the uterus tore across above the cervix, allowing some discharge to escape. After removal of the body and cervix a purse-string suture was applied to the peritoneum and the abdominal wound was closed. The wound healed by first intention, and the patient left the hospital on June 7, 1909.

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[1] At a meeting of the Section, held May 2, 1918.

The uterus measured 12 cm. by 11·5 cm. by 7·5 cm., and weighed 14¾ oz. Its enlargement is due to cancer and to a small intramural myoma about 2 cm. in diameter in its anterior wall. The body of the uterus is occupied by a cancerous growth 3 cm. thick in each wall: the growth has nearly perforated the anterior wall of the lower segment. The left cornu is distended with growth, producing marked asymmetry. There the growth penetrates to the peritoneum, which is retracted and uneven, as if by shrinkage of the growth. The growth is separated from the muscle below by a sinuous outline, and the cut surface is of a yellowish colour permeated by white irregular patches and streaks.

Microscopic Structure.—The growth is a columnar-cell carcinoma of the tubular variety, the cells being in one layer.

Patient felt quite well till December 5, when she was seized with pain in the right iliac region: this became extremely severe. On examination *per vaginam* a mass as big as a virgin uterus was found above the vagina. It was not fixed to the rectum, was adherent to the pelvis, involving the right side of the bladder around the ureter. On February 24, 1910, abdominal section was performed, and the recurrent mass separated from the pelvic wall, the right ureter cut across above the growth, and the whole mass with the right half of the bladder and a small piece of the vagina was removed: the upper end of the ureter was brought out of the abdominal wall through a stab wound. The left half of the bladder was closed with fine silk sutures not involving the mucous membrane. It healed and functioned well: though a minute fistula, the size of a pin's head, led to occasional slight leakage of urine till closed by a touch of the actual cautery. A small nodule was excised from the rectal wall. The mass removed measured 11 cm. by 7 cm. by 5 cm., and consisted of a growth as big as a hen's egg, surrounded by bladder and vagina and a piece of ureter, the vesical orifice of which was 2 cm. from the cut edge. The piece of bladder measured 8 cm. by 6 cm.

The growth had the same structure as the primary growth, being a carcinoma of the adenomatous type: the growth in the rectal wall showed alveoli lined with cubical cells.

The kidney became infected through the exposed ureter, and on May 6 was removed by Mr. Bilton Pollard.

On November 18, 1910, a recurrence had occurred. The patient had obtained so much benefit from the first operation for recurrence, being completely relieved of her agonizing pain, that she was eager for another operation. An exploratory operation revealed a mass, 3 in. by 2 in. in size, adherent to the promontory and to a coil of small intestine which was infiltrated by growth. The abdomen was closed, as the growth was irremovable. The patient died suddenly on July 7, 1911, two years and two months after the hysterectomy.

Case II. -F. R. H., a spinster, aged 49, was admitted into University College Hospital on February 19, 1912, complaining for the last eighteen months of leucorrhœa and menorrhagia, and for several months of bleeding between the periods upon exertion, and all her menstrual life (but especially latterly) of dysmenorrhœa, which continued during the whole time of the flow. Menstruation began at the age of 17, was regular every three weeks, lasting ten days, until eighteen months ago, when the flow became more frequent and profuse. The patient had never suffered from rheumatism, but had had scarlet fever and whooping-cough.

On admission she was rather thin (weight 7 st. 9 lb.), the temperature was 99° F., the respirations 24, the pulse 90, the urine (specific gravity 1016) normal. The cardiac impulse was visible and diffuse and felt in the fifth space external to the nipple line. A systolic murmur was heard at the base and at the apex, whence it was conducted to the axilla.

The lower abdomen was occupied by a hard, slightly nodular tumour, measuring 6 in. across, the upper border of which was 5 in. above the pubes. The cervix, healthy, moved with the tumour; the latter was diagnosed as a multinodular myoma. No suspicion of cancer complicating the myoma was aroused, the leucorrheal discharge being clear, "like water slightly tinged with yellow," and never offensive. Sufficient importance was not attached to the intermenstrual bleeding, which came on only after unusual exertion and during the last few months.

On February 24, 1912, I removed the uterus (with the cystic right ovary) by total abdominal hysterectomy (Doyen's method), opening and severing the vagina with the galvano-cautery. The abdominal wound was closed with through and through stitches of silkworm gut, buried fine silk for the peritoneum and fascia, and horsehair for the skin. The operation lasted forty-five minutes.

The wound healed by first intention: the highest temperature was 99·6° F. on three occasions during the first fortnight. On the fifteenth day the superficial stitches were removed and the temperature reached 100° F., then rapidly fell to normal on the eighteenth day. The patient left the hospital on April 3, 1912, weighing 6 st. 9 lb.

The uterus measured 6½ in. by 5¼ in. by 4 in. (16·5 cm. by 13 cm. by 10 cm.) and weighed 2¼ lb. (1·09 kilo.). The right ovary contained a thin-walled cyst 1¾ in. in diameter. The cervix was healthy, its canal somewhat dilated. The uterus was regularly enlarged except for the presence of seven subperitoneal fibroids measuring 1 in. to ½ in. in diameter. Its enlargement was chiefly due to an intramural myoma as large as an orange. The uterus has been laid open posteriorly (see fig. 1) and sections have been cut, exposing the cavity rendered convex by the myoma in its wall. The lower part of the corporal cavity is smooth: the upper part is occupied by a papillary and lobular growth, the deeper extension of which is seen on section as a granular mass penetrating to within 4 mm. (as shown in the drawing) and at one spot to within 1 mm. of the peritoneum. At one part this growth is more than 2 in. thick, becoming gradually thinner as it passes into the papillary and lobular surface growth.

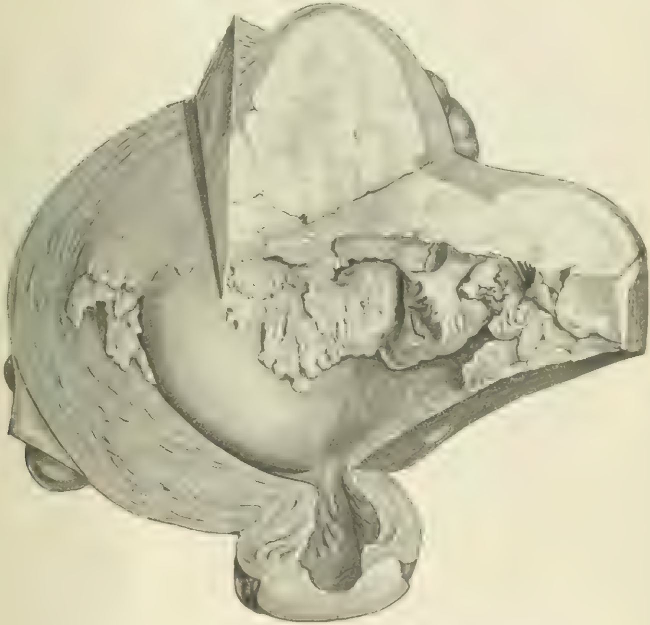

Fig. 1. (Case II.)

Uterus (three-fourths natural size) with myomata complicated with cancer of the body removed by total abdominal hysterectomy. Patient well five years later. Only one of the seven subperitoneal myomata is seen. The lower part of the body is rendered convex by the bulging of an intramural myoma. A papillary and lobulated carcinoma is seen occupying the upper part of the body. Sections have been removed to show the deep extension of the cancer which penetrated at one spot to within 1 mm. of the peritoneum.

Microscopic Structure.—The submucous tumour is a myoma showing extensive hyaline degeneration. The papillary part of the malignant growth at and near its edge is a papillary carcinoma, with an even line towards the muscle, and except at its deeper parts it shows a very slight amount of leucocytic infiltration. The epithelium of the papillary growth is columnar, hazy, stained somewhat reddish with logwood, and for the most part in one layer, but in places in several layers. This epithelium contrasts markedly with the shorter, bluer, clearer epithelium of the normal glands, which in some places are seen immediately beneath the papillary growth and

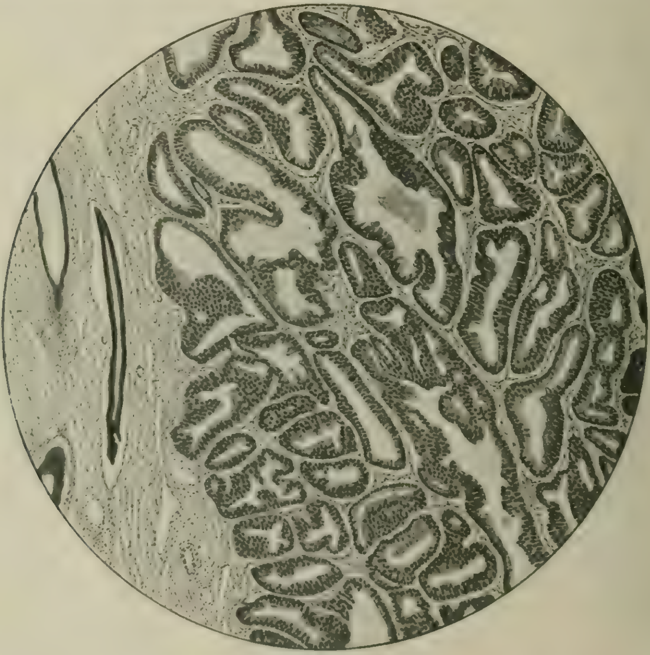

FIG. 2. (Case II.)

Section of the papillary carcinoma near to free edge showing the contrast between the epithelium of the papillary carcinoma and of the two normal endometrial glands seen on the left. In deeper parts of the growth proliferation of the epithelium forms solid tracts and areas of considerable size, which in places are necrotic. **The deepest part of all is shown in fig. 3.**

near its line of demarcation from the normal mucosa (*see* fig. 2). The deeper parts of the growth consist of masses of columnar-cell carcinoma: in places the masses are of large size and show many areas of necrosis; in the deepest portions, towards the peritoneal surface (to within 1 mm. of which the growth extends at one spot) the cell-masses are smaller, and there is considerable small-cell infiltration in the stroma (*see* fig. 3).

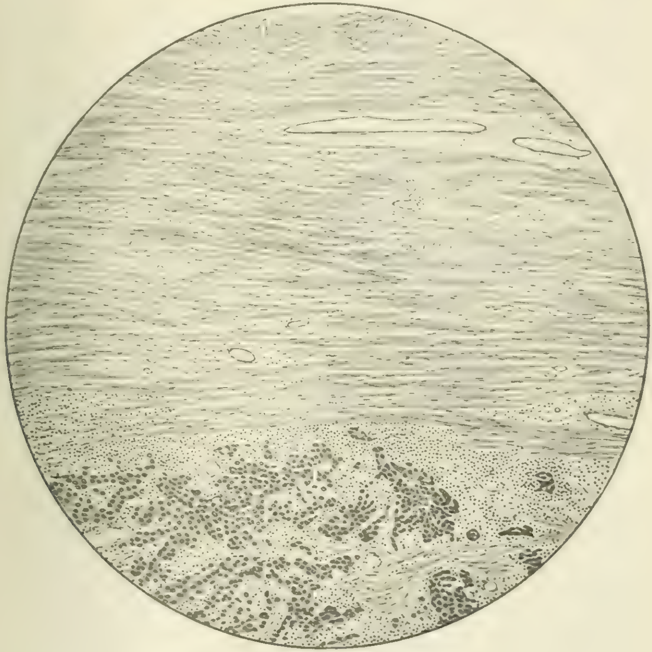

FIG. 3. (Case II.)

Section showing cancer deeply penetrating the uterine wall towards the peritoneum (which is seen at the top of the circle). This penetrating edge shows small solid masses of cancerous epithelium with much small round-cell infiltration. (× 55.)

The patient has had good health since the operation. She was examined by me on July 3, 1917, more than five years after the operation, and was found to be well and free from recurrence. She weighed 7 st. 7 lb. The scars, abdominal and vaginal, were sound, and she had no trouble of any kind.

These cases, like those in the two previous papers, illustrate an important point of superiority of total hysterectomy over supravaginal amputation, which latter operation I ceased to perform more than sixteen years ago and in thirty years have never performed in a case in which cancer of the body existed.

I invite the advocates of supravaginal amputation to give the whole of their experience of undiagnosed cancer of the body in cases of myoma; meanwhile I give particulars of the experience of three such advocates, so far as I have been able to find it published.

Kelly and Cullen in their work on "Myomata of the Uterus" (1909) give notes of sixteen cases of myoma complicated by cancer of the body (which in two cases was not proved by microscopical examination). In eleven of the sixteen cases amputation was done, and in five total hysterectomy. In one of these five cases the uterus was incised before removal; that is, in only four of the sixteen cases, or one-fourth of the whole number, was total hysterectomy done, as ordinarily understood in cases of cancer. In none of the sixteen cases was the after-history for five years given, and in several cases no after-history at all. They state (loc. cit., p. 275): "A glance through the histories of the accompanying cases will show that in most of them carcinoma was not suspected until after removal of the uterus"; and (p. 276): "The association of adeno-carcinoma of the body of the uterus with myoma in about 1·7 per cent. of the cases would naturally suggest total abdominal hysterectomy as a routine procedure." They then make a series of statements on what they "know" and "feel" with regard to the two operations with which no advocate of total hysterectomy by Doyen's method will agree. They advise "opening the uterus to determine if carcinoma exists and cutting open any suspicious myomata to see if by chance sarcomatous changes are present," whereas in some cases it is quite impossible to detect with certainty malignant changes in a fresh specimen, as is shown by one of their cases of sarcoma (loc. cit., p. 194) in which the sarcoma recurred in the cervical stump, and by a case of carcinoma (p. 295) in a very early stage accidentally discovered during a routine microscopic examination of the mucosa. Then they describe (p. 286) a case of myoma in which amputation was performed. "After the operation the uterus was opened and the malignant growth detected and we expected to remove the cervix at a later date." The patient, however, died of peritonitis in a few days, from infection which the authors believe would have been avoided by removal of the cervix. On this they remark: "This case shows the absolute necessity of opening the uterus the minute it is removed." It cannot be insisted upon too strongly that sarcomatous and early carcinomatous change can only be detected with certainty in uteri hardened for several days or weeks—and that total hysterectomy in two stages (amputation followed by excision of the cervix) for myoma complicated with malignant disease is inferior to the operation in one stage by Doyen's method. Altogether the advocates of amputation will not derive much comfort from these cases.

Giles, in his work on the after-results of abdominal operations,[1] gives a list of cases of carcinoma of the body of the uterus: "J i, cases nine, mortality one, traced six." He says: "In the group J i pan-hysterectomy was done in every case but one (No. 5): this was not known to be a case of carcinoma till the section had been cut and examined. I should always do a pan-hysterectomy when the operation was undertaken on account of carcinoma." On referring to the particulars of Table J i (p. 392) it is found that in Case 1 the uterus had *not* been completely removed, "a small normal shell of cervix remained"; in Case 3 also amputation was performed, the cervix being removed a fortnight later; in Case 5, as stated by the author, the cervix was left. Cases 3 and 5 were complicated with fibroids, the cancer was overlooked and supravaginal amputation was performed. After-histories are given in these two cases for thirty-five and thirteen months.

We thus find that neither of the two cases of combined cancer of the body and fibroids was treated by Giles, and only one-fourth of Kelly's and Cullen's sixteen cases were treated by total abdominal hysterectomy at one sitting, removing the uterus intact.

Putting the cases together, out of eighteen only four were treated by total abdominal hysterectomy done at one sitting and the uterus removed intact, and fourteen, or 77 per cent., of the cases were treated by amputation or (in one case) by opening the uterus by an incision before removal. This is a large percentage for an operation which is admitted by these authors to be unsuitable for fibroids complicated with cancer of the body, the danger of sepsis and of implantation of cancer being greater than if the uterus is removed by Doyen's total hysterectomy, in which the cervix is closed at an early stage of the operation by volsella, and especially if the vagina is opened and severed by the cautery, as in the second case now recorded.

How far the frequency of recurrence is affected by the partial operation can only be settled when gynæcologists publish all their cases, show their specimens and sections, and give the after-history of their cases, including the examination of the patient at the end of five years. Again I invite them to do so.

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[1] *Journ. Obstet. and Gynæcol. of the Brit. Emp.*, 1910, xvii, p. 374.

Meanwhile it would seem as if the facile and rapid amputation of the uterus leads some of its advocates to overlook cancer of the body with unusual frequency and involves the patients in considerable risk. This occurs not only in cases of cancer of the body complicating myoma, but also in uncomplicated cancer of the body, for out of twelve abdominal hysterectomies for cancer of the body uncomplicated by myoma, three (25 per cent.) were amputated: two of the three died of recurrence, the other remained well after five and a half years (Cullen, "Cancer of the Uterus," 1900, p. 642).

Having acknowledged my own error in overlooking the cancer in the cases now published, I feel at liberty to state my opinion that a careful attention to the history will in almost every case suggest the presence or absence of cancer of the body and indicate exploration with the finger or curette and microscopic examination of the tissue removed. And I would suggest to the advocates of amputation for myoma who are still unconvinced of the superiority of total hysterectomy the advisability of adopting Kelly's suggestion of circumcising the cervix and clamping the mucosa before cutting it across (with the cautery) as an attempt to obtain one of the advantages which the total removal of the uterus gives in these cases.

SUMMARY.

(1) The writer, an advocate of total abdominal hysterectomy for myoma, has never amputated a uterus containing cancer of the body, whether complicated with myoma or not.

(2) He has overlooked cancer in the body complicating myoma in two cases (here recorded): in the first case the disease had extended to the peritoneum and recurred, the patient surviving the hysterectomy for two years and two months; in the second case also the disease was extensive, but the patient, treated by total abdominal hysterectomy, is in all respects well and free from recurrence after more than five years.

(3) Three writers, advocates of amputation for myoma, have overlooked cancer of the body complicating myoma in fourteen out of eighteen cases, have performed amputation (or, in one case, incision before removal) in these fourteen cases (77 per cent.): one of the cases died of sepsis, due in the opinion of the authors to their not removing the cervix, and no five-year after-history is given.

(4) Advocates of amputation for myoma need to take greater care

than advocates of total hysterectomy in making the diagnosis of cancer of the body as a complication and should clamp the uterus before dividing the cervix (with the cautery).

(5) Total abdominal hysterectomy by Doyen's method, the vagina being opened and severed with the galvano-cautery, is superior to amputation for myoma in lessening the risk from cancer of the body, should it have been overlooked.

DISCUSSION.

Mr. MALCOLM: I have long been of opinion that total hysterectomy is safer than supravaginal hysterectomy, especially when there is any inflammation of the cervix. For many years, in operating for fibromyomata, except in a few cases in which the whole uterus could be left, I have acted on this view. Holding these views I am influenced by Dr. Spencer's argument. The evidence is clear that there is a possibility of not recognizing a malignant growth, and this fact emphasizes the advisability of performing a total rather than a partial hysterectomy.

Dr. ANDREWS: I think that Dr. Spencer has made out a much stronger case for panhysterectomy than he did in his former paper on the subject. Routine subtotal hysterectomy for fibroids in elderly patients must entail some risk of coming across carcinoma unexpectedly. In most cases of carcinoma of the body of the uterus occurring with fibroids, however, the history is suggestive of the presence of carcinoma, and an operator whose practice it is to perform the subtotal operation takes the warning and removes the whole uterus. Bleeding beginning again after a period of amenorrhœa extending over several months, and especially irregular or continuous hæmorrhage in the absence of a fibroid polypus, suggest that the symptoms are due to carcinoma and not to the fibroids. I confess that I not very long ago removed a uterus greatly enlarged by fibroids, and found, on cutting it open, that it contained carcinoma, and on going over the history more carefully realized that I ought to have suspected carcinoma.

Dr. GIBBONS: I consider the preservation of the pelvic floor is a matter of importance in comparing the relative value of these two operations, and I believe that this view is shared by others. It so happens that I have not had a case of carcinoma of the cervix in any of the subtotal hysterectomies I have done, although it is possible that those recently performed may within the next few years show signs of it. Nevertheless the chance of its onset, considering the percentage of the occurrence of carcinoma of the cervix, would not deter me from continuing the operation of subtotal hysterectomy, which is, in my opinion, an excellent operation when no symptoms point to carcinoma, and if I followed the invariable rule I have of carefully examining the uterus and its contents directly after amputation, so that, if necessary, the cervix could also be immediately removed.

Mr. O. TILSON DINNICK: The greater number of cases are admirably dealt with by a supravaginal amputation, which leaves the cervix as the "keystone" of the vaginal fornix, and does not rob the vagina of depth. Where the cervix is lacerated, and the source of vaginal discharge, total hysterectomy is more suitable and the post-operative course of a series of total hysterectomies in competent hands is less eventful than that of a similar series of supravaginal amputations. Should hæmorrhage occur, it is sooner detected, and there is an absence of the slight temperature which so often occurs in the supravaginal amputations—perhaps explicable by the facility afforded for oozing from the parametrial tissues.

Dr. LAPTHORN SMITH: Although I cannot agree entirely with all that Dr. Herbert Spencer says, I think the whole profession is indebted to him for his papers on the early diagnosis of cancer of the uterus. Owing to his work and to that of others the attention of practitioners has been so often called to the danger of delay in seeking special advice in all cases of irregular hæmorrhage in women about 45 years old, that most of them are now suspecting cancer and saving the lives of women who ten years ago would have followed a wait-and-see policy until it was too late. It is well known that malignant infection of fibroid tumours of the uterus is a very common incident in their dangerous career and that if all fibroids were removed as soon as discovered there would be at once a great drop in the death-rate from cancer of the body of the uterus. I quite agree with Dr. Herbert Spencer when he says that total hysterectomy, including the cervix, is the only operation for tumours of the body of the uterus which are *believed* or even *suspected* to be carcinomatous or sarcomatous. But when there is no suspicion of malignancy I do not agree with him in saying the cervix should be removed. He claimed that total removal is no more difficult than subtotal removal, that is leaving the cervix, but I have seen many of the world's great operators doing the two operations in cases suited for each and they all agreed with me in saying that the removal of the cervix adds considerably to the time and difficulty of the operation, especially in the case of a fat woman. Moreover all are agreed that in an absolutely non-malignant case the cervix can be made use of for suspending a prolapsed pelvic floor by the broad ligaments, or as I have often done by fastening the prolapsed cervix to the lower end of the abdominal incision, thereby lengthening instead of shortening the vagina and greatly supporting the relaxed pelvic floor. I have removed a great many fibroid tumours by abdominal subtotal hysterectomy and have seen or heard from most of the patients for many years after operation without having known of one of them developing cancer in the remaining cervix.

Dr. HERBERT SPENCER (in reply): I agree that the average man performing total hysterectomy or amputation, without special study of the best means of performing the former operation, might have a higher mortality with the total operation: but my conception of the duty of a gynæcological teacher is to show which operation is the better in the best hands, and I have no doubt that the total operation by Doyen's method is the better and safer operation. I do not agree that it would have been fair to exclude Case I, which is a case of fibroids complicated with unrecognized cancer of the body —the subject of my paper; but its exclusion would have strengthened my position considerably. For the same reason the volsella was used; in cases diagnosed as cancer I always make traction with forceps. I am glad that Dr. Russell Andrews thinks that I have made a stronger case in this paper than in the former one dealing with sarcoma, because Dr. Russell Andrews[1] stated his opinion that the case that was presented on sarcoma was not a strong one: but the only specified point of weakness was conveyed in his question whether I could give any evidence of recurrence of sarcoma in the cervical stump after amputation. That evidence I was able to give, and I hope the case has been transformed by that evidence into a strong one. About a hundred cases of malignant disease in the cervical stump after amputation have already been recorded, and significant examples have been mentioned to-night. This grave danger exists, and is in a different category from the fancied troubles with the pelvic floor or vaginal shortening, the former of which does not occur in a properly conducted operation, and the latter is of trivial importance. Gynæcologists who condemn total abdominal hysterectomy on those grounds should condemn vaginal hysterectomy still more strongly. I am glad to have in Mr. Malcolm a supporter of the total operation, which compares favourably with amputation in the smoothness of the recovery.

Removal of an Endotheliomatous Dermoid of the Ovary and (thirteen and a half years later) a Fibro-endothelioma of the Vagina.[2]

By HERBERT R. SPENCER, M.D.

M. D., a virgin, aged 43, was admitted into University College Hospital on December 18, 1902, complaining of "a tumour in the womb" and almost continual bleeding from the vagina for thirteen months, never being free from hæmorrhage for more than a week until a month ago, when it ceased. The continued bleeding came on after an attack of influenza. A doctor discovered the abdominal tumour in April, 1901. There had been no pain. Menstruation began at the age of 14, was always regular, every four weeks, except at the age of 30, when it ceased for two or three months; eight diapers were used as a rule, but more during the last six years. The patient's father died of an "ulcer of the foot" at the age of 66; her mother was 68 years of age and nearly blind; an aunt died of a tumour in the abdomen.

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[1] *Proc. Roy. Soc. Med.*, x, 1917 (Sect. Obst. and Gynæcol.), p. 131.

[2] At a meeting of the Section, held May 2, 1918.

On admission the patient was rather pale and sallow, the breasts of medium size, the heart sounds normal. The abdomen was rather full: a body of the size of a tangerine orange could be felt above the pubes. The hymen was intact. The cervix was pushed forwards by a hard tumour as big as a double fist, which appeared to be a uterine fibroid pushing the uterus forward and to the right and blocking the pelvis. The body felt by the abdomen was the fundus.

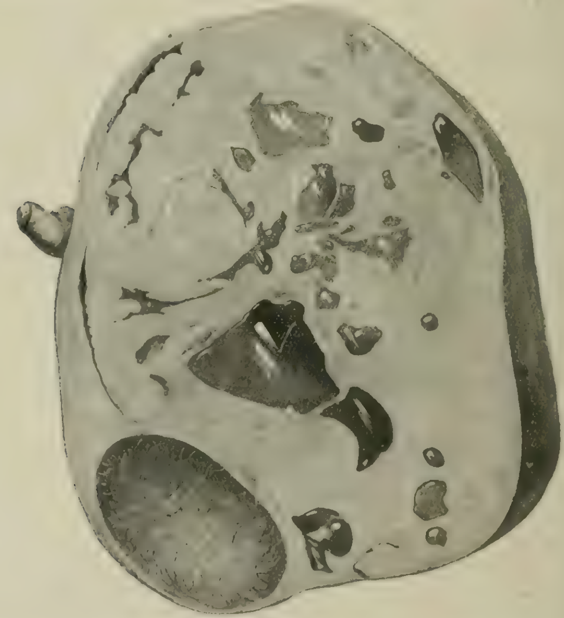

Fig. 1.

Endotheliomatous dermoid of the ovary, showing the dermoid cyst and the endothelioma.

It was thought that a submucous fibroid tumour was also present, but none was found on dilating the uterus, which was 4½ in. in length.

The uterus was curetted on December 20, and the tumour was diagnosed as a subperitoneal fibroid. The patient remained free from bleeding till April 10, when hæmorrhage recurred and continued till her admission on May 19, 1903. The fundus of the uterus could be felt at a height of 3½ in. above the pubes; the os was open, and in the left posterior quarter of the pelvis a hard smooth tumour, as big as a double fist, could be felt. The tumour was removed by abdominal section on May 23, 1903, and found to be an ovarian tumour, and externally looked like a dermoid. It was sent uncut to the Museum to be hardened and examined. This was duly carried out after some weeks, but the description was, by inadvertence, not inserted in the hospital notes, where it appeared simply as an ovarian tumour. The left ovary had a small cyst near the hilum and was removed, but not examined microscopically. The wound healed by first intention, and the patient left the hospital on June 15, 1903.

The specimen (No. 482, U.C.L. Gynæcological Catalogue, 1911), is a solid ovarian dermoid measuring 12 cm. by 9 cm. by 7 cm., smooth on the surface, from which projects a single dermoid cyst with blood-stained contents. On section (fig. 1) there is a single dermoid cyst measuring 4 cm. by 2·5 cm., filled with sebaceous material and containing hair growing from its wall; the rest is made up of a solid white tumour with clear indications in places of capsulation and with several cysts, particularly towards the centre, filled with blood-stained jelly-like material; the great bulk of the growth has a fibrous appearance. The opposite ovary was not enlarged, but had a single cyst near the hilum.

Microscopic Structure.—The dermoid cyst is lined with stratified epithelium. The growth (fig. 2) consists of a dense fibrous stroma permeated by a close network of epithelioid cells, usually two or three cells deep, in some places forming broader bands. These seem to lie in vascular spaces. In some places a blood-vessel is seen in the centre of the growth, which is intimately connected with the outer wall. The cells are elongated, with their axes transverse to the long axis of the strands and have large nuclei. The growth has in places the appearance of a perithelioma; but in other parts of the section all stages of endothelial growth may be seen, showing that it is an endothelioma.

Thirteen and a half years later the patient, then aged 57, was re-admitted into University College Hospital on January 9, 1917, complaining of discomfort on walking, of a tumour as big as a walnut protruding through the vulva, with a blood-stained discharge occasionally during the last six months.

Menstruation ceased after the operation in 1903. The abdominal scar was sound. There was leucoderma of the skin around the vulva.

On the posterior wall of the vagina, an inch from the fourchette, was a pedunculated tumour as big as a pigeon's egg, with a fairly broad pedicle. It felt like a fibroid and bled a little on examination. The uterus was atrophied and freely movable. A small nodule of the size of a split pea could be felt just to the right of the os, probably

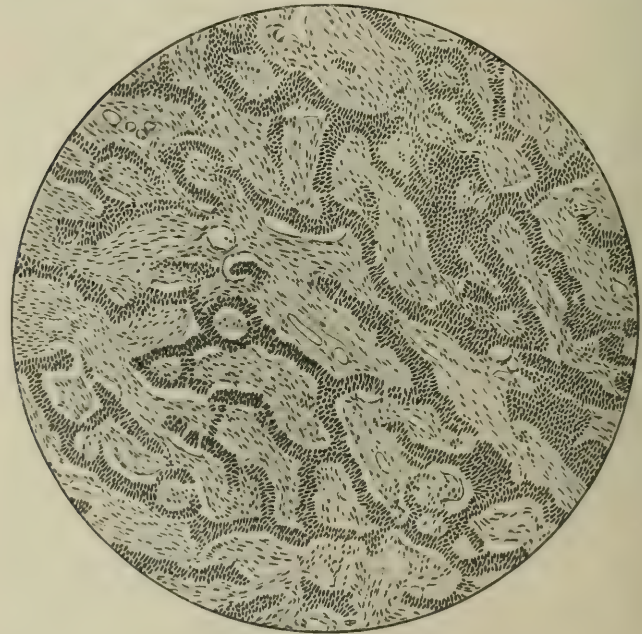

FIG. 2.

Endotheliomatous dermoid of the ovary. Section showing the endothelioma with its plexiform arrangement which takes the course of vascular spaces. The long axes of its cells are for the most part arranged at right angles to the course of the vessels. In some sections the origin of the growth from the endothelium of blood-vessels is clearly seen (*see* Report of Pathology Committee, p. 64). (× 115.)

the remains of the atrophied portio. The vaginal tumour was removed by drawing it down and making an elliptical incision around its base. The wound healed by first intention, and the patient left the hospital on January 24, 1917. She remained quite well in January, 1918.

The tumour was hardened and cut into after a day or two for microscopic purposes. A piece of the white growth at the surface was examined; it consisted mainly of coarse fibres, and was thought to be a fibroma.

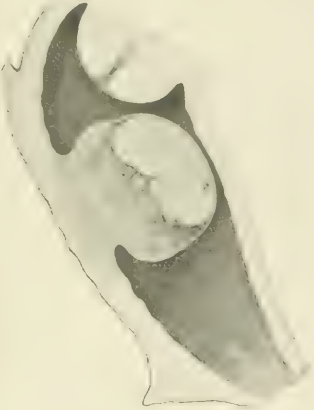

FIG. 3.

Fibro-endothelioma of vagina. Drawing of tumour by Mr. Shiels from specimen, inserted in diagram to show position. The outer white part of the growth is a fibroma invaded with endothelioma, a "fibro-endothelioma"; the duskier part of the growth is pure endothelioma similar to that of the ovarian growth (fig. 2).

I had the specimen thoroughly hardened and then made a section completely through it, and found that beneath the hard white cortex was a softer, duskier growth, which permeated the tumour towards the pedicle (*see* fig. 3). Sections of this growth showed it to be an endothelioma. Endothelioma can also be seen in the white fibrous part. The tumour may be called a fibro-endothelioma.

The former notes of the patient were then consulted to ascertain the nature of the ovarian tumour removed in 1903, but, as already stated, without result. On looking up the museum catalogue description it was found that the ovarian tumour removed thirteen and a half years ago was also an endothelioma, the description of which has been already given. The microscopic appearances of the deeper parts of the vaginal growth and of the ovarian growth are so similar that a separate illustration is unnecessary.

Endothelioma in a dermoid tumour of the ovary is rare: this is the only specimen of endothelioma I have met with in over 500 ovariotomies. It is on account of the after-history that I bring it before the Section. The appearance of an endothelioma in the posterior wall of the vagina thirteen and a half years after the ovariotomy raises the question as to whether this is a "recurrence" or not. I am inclined to think it is not a true recurrence. Although I have seen *cancer* of the uterus recur in the posterior vaginal wall at some distance from the fornix, I have seen it more often, though still rarely, low down in the anterior vaginal wall. The fact that the patient remains well after more than fourteen years and has no sign of other growths is, I think, against the view that the vaginal tumour is a true recurrence; but on this point I should like to have the opinion of members of the Section.

The specimens were referred to the Pathology Committee.

Report of Pathology Committee on Dr. Herbert Spencer's Specimen of Endothelioma in a Dermoid Tumour.—"We have examined sections made from six different places in this tumour and find that they all have the same essential structure, namely, that of an endothelioma. Part of one of the sections shows the origin from the endothelium of blood-vessels very clearly. There is no evidence of any other kind of tissue, such as we should expect to find in an embryoma. A section of the large cystic space containing hair is lined by epidermis. The recurrent growth also has the same essential structure of an endothelioma like the original tumour."

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